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Experiments on Redistribution, Trust, and Entitlements

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EXPERIMENTS ON REDISTRIBUTION, TRUST, AND ENTITLMENTS

BY
DANIEL THOMAS HALL

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
Doctor of Philosophy
in the
Andrew Young School of Policy Studies
of
Georgia State University

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ACCEPTANCE

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

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CONTENTS

ACKNOWLEDGEMENTS ..... **vi**

LIST OF TABLES ..... **x**

LIST OF FIGURES ..... **xi**

ABSTRACT ..... **xiii**

CHAPTER I INTRODUCTION **1**

CHAPTER II **10**

THE EFFICIENCY-EQUALITY TRADEOFF IN PUBLIC SECTOR CHARITY PROVISION **10**

Introduction 10

Background and Literature 10

Experimental Design 13

Results and Analysis 22

Survey Analysis 44

Conclusion 49

CHAPTER III USING ROLE-REVERSAL IN LABORATORY EXPERIMENTS **51**

Introduction 51

Background and Literature 53

How Should a Role-Reversal Protocol be Designed? 58

Experimental Design 64

Investment Game Control Treatment 67

Role-Reversal Treatment 68

Results and Analysis 70

Sender Role Decisions 70

Receiver Role Decisions 72

What Else Can be Learned From the Role-Reversal Protocol? 75

Survey Analysis 76

Conclusion 78

CHAPTER IV **80**

TRUST IN PRIVATE AND COMMON PROPERTY EXPERIMENTS: EFFECTS OF STRONGER PROPERTY RIGHT ENTITLEMENTS **80**

Introduction 80

Theory and Hypotheses 82

The Cox, Ostrom, and Walker et al. (2009) Study 88

Adding Stronger Property Right Entitlements 94
Cox and Hall Sequential Move Protocol Treatments 97
Adding the Strategy Method Protocol 112
Cox and Hall Strategy Method Protocol Treatments CH4 and CH5 114
Testing for Saliency Effects 121
Survey Analysis 129
Conclusion 134
CHAPTER V CONCLUSION 138
APPENDIX A EXPERIMENT INSTRUCTIONS FOR CHAPTER II 141
APPENDIX B EXPERIMENT SURVEY FOR CHAPTER II 145
APPENDIX C EXPERIMENT INSTRUCTIONS FOR CHAPTER III 149
APPENDIX D EXPERIMENT SURVEY FOR CHAPTER III 156
APPENDIX E EXPERIMENT INSTRUCTIONS FOR CHAPTER IV 160
APPENDIX F EXPERIMENT SURVEY FOR CHAPTER IV 166
REFERENCES 170
VITA 176

LIST OF TABLES

Table		Page
1.	Group Assignment According to Rank in Double Pay Period	16
2.	Group Treatment and Session Assignment	22
3.	Tobit Regressions of Leisure Seconds Taken	42
4.	Tobit Regressions of Effort Proxy Variable	43
5.	Group Mean Leisure Seconds Taken and Group Mean Survey Responses	47
6.	Correlations of Mean Survey Responses to Group Mean Leisure Seconds	48
7.	Detailed Summary of Sender and Receiver Decisions by Treatment	70
8.	Statistical Tests for Treatment Differences in Amounts Sent	71
9.	Tobit Regression of Return Decisions	73
10.	Tobit Regression of Amount Sent Under Role-Reversal	75
11.	Tobit Regression of Amount Returned Under Role-Reversal	76
12.	Role-Reversal and Opportunistic Senders	76
13.	Survey Correlations with the Amounts Sent and Returned in both Treatments	78
14.	Subject Decisions for COW1 Treatment	89
15.	Subject Decisions for COW2 Treatment	90
16.	Parametric and Non-Parametric Tests of Type X Decisions	90
17.	Tobit Analysis of Type Y Decisions in COW Study	93
18.	Subject Decisions for CH1 Treatment	102
19.	Subject Decisions for CH2 Treatment	102
20.	Subject Decisions for CH3 Treatment	103
21.	Treatments II and III, Combined Common Property Trust Game Data	104
22.	Means and Distribution Tests for Treatment Differences in Type X Decisions	106
23.	Variance Tests for Treatment Differences in Type X Decisions	107
24.	Proportions Tests of Type X Decisions	107
25.	Tobit Regressions of Type Y Decisions	110
26.	Subject Decisions for CH4 Treatment	116
27.	Subject Decisions for CH5 Treatment	117
28.	Parametric and Non-Parametric Tests of Type X Decisions	119
29.	Tobit Analysis of Type Y Decisions in Treatments CH4 and CH5	121
30.	Tobit Regressions of Type Y Decisions	128
31.	Type Y Average Responses to 10 Sent (0 Withdrawn)	128
32.	Tobit Regressions of Type X Decision with Questionnaire Analysis	131
33.	Tobit Regressions of Type Y Decision with Questionnaire Analysis	133

LIST OF FIGURES

Figure		Page
1.	Computer Screenshot of a Subject's Labor Task	15
2.	Computer Screenshot of a Subject's Leisure Task	18
3.	Computer Screenshot of a Subject's End of Period Earnings Information	19
4.	Efficiency Measured in Productivity by Treatment	24
5.	Efficiency Measured in Productivity by Session and Group	25
6.	Efficiency Loss Measured in Percentage Change	29
7.	Changes in Inequality of Labor Earnings by Session and Group	29
8.	The Efficiency-Equality Tradeoff (All Periods Averaged)	32
9.	Tradeoff in the 1st, 6th, and 12th Regular Pay Periods	33
10.	Leisure Seconds Taken by Treatment	35
11.	Leisure Seconds Taken by Subjects (Bar Width = 30 Seconds)	36
12.	Work Intensity by Treatment	38
13.	Effort Proxy by Subjects (Bar Width = 0.1)	38
14.	Layout of Rooms Used During the Experiment	65
15.	Layout of Rooms During the Payoff Procedure	66
16.	Amount Sent by Treatment	71
17.	Amount Returned by Treatment	74
18.	Amount Returned Divided by Amount Partner Sent by Treatment	74
19.	Type X Decisions: COW1, Private Property Trust Game	89
20.	Type X Decisions: COW2, Common Property Trust Game	91
21.	Comparison of Type X Decisions for COW1 and COW2	91
22.	Type Y Decisions for COW1 and COW2	93
23.	Computer Screenshot of the Real Effort Task	99
24.	Type X Decisions: CH1, Private Property Trust Game	102
25.	Type X Decisions for CH2, Common Property Trust Game	103
26.	Type X Decisions: CH3, Common Property Trust Game	104
27.	Comparison of Type X Decisions for CH1, CH2, and CH3	105
28.	Type Y Decisions for CH1 and CH2	108
29.	Type Y Decisions for CH1 and CH2 & CH3 (pooled)	109
30.	Type Y Decision Sheet for Eliciting Decisions Under the Strategy Method Protocol	115
31.	Type X Decisions for CH4, Private Property Trust Game	116
32.	Type X Decisions for CH5, Common Property Trust Game	118
33.	Comparison of Type X Decisions for CH4 and CH5	118
34.	Type Y Decisions for CH4 and CH5	120
35.	Comparison of Type X Decisions in the Private Property Trust Game	122
36.	Comparison of Type X Decisions in the Common Property Trust Game	123
37.	Effect of Real Effort Task on Type Y Decisions in the Private Property Trust Game	125
38.	Effect of Strategy Method Protocol on Type Y Decisions for Private Property Trust Game	125
39.	Effect of Real Effort Task on Type Y Decisions for Common Property Trust Game	126

Figure **Page**

40. Effect of Strategy Method Protocol on Type Y Decisions for Common Property Trust Game 126

ABSTRACT

EXPERIMENTS ON REDISTRIBUTION, TRUST, AND ENTITLEMENTS
BY
DANIEL THOMAS HALL

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This dissertation comprises three essays. The unifying theme is experiments used as the empirical methodology. Each essay is an independent study, but aspects of behavior related to cooperation, trust, and entitlement are present in each essay.

The first essay looks at the efficiency-equality tradeoff of increasing redistribution in a small group setting. Subjects generate a stronger sense of entitlement to their labor earnings by performing a real effort task. Subjects must trust other members of their group to work in order to keep labor a profitable activity under higher levels of redistribution. We find a significant efficiency-equality tradeoff explained by lowered work incentives. Labor supply decisions also show strategic and cooperative behavior similar to behavior found in public goods experiments. The efficiency-equality tradeoff calls for a reconsideration of increasing dependence on the public sector for charity provision.

The second essay investigates how the application of a role-reversal protocol affects behavior in cooperative games. Subjects play all possible player roles in the game under a role-reversal protocol. We test if behavior results from a two-role trust game are robust to applying a role-reversal protocol. We find that paying subjects for one role

leads to no significant role-reversal effect whereas previous studies paying for both roles find reductions in generosity in both roles.

The third essay is co-authored with Dr. James C. Cox. We test for differences in trust-related behavior under private and common property environments. Subjects participate in payoff equivalent 2-person Private Property Trust Game or Common Property Trust Game. We strengthen property right entitlements by asking subjects perform a real effort task to earn their private or common property endowments. Strengthening entitlements leads to behavioral differences in the two trust games not previously found. We find second mover generosity in response to first mover decisions is lower in the Common Property Trust Game. Second movers are relatively less generous because first movers overturn the status quo opportunity set which is most generous and signals “full trust.” Many first movers anticipate this and respond optimally by choosing extremes which signal “full trust” or “no trust” in the game.

CHAPTER I

INTRODUCTION

The purpose of this dissertation is to further knowledge of social preferences and investigate some recurring methodological issues in measuring and interpreting related behavior in laboratory experiments. This dissertation comprises three essays. The effect of redistribution on work incentives is the theme of the first essay. The effect of role-reversal on trust-related behavior is the theme of the second essay. The effect of earned entitlements on trust-related behavior in private and common property environments is the theme of the third essay. The unifying theme of the three essays is the empirical methodology of experimental economics and choices made in environments which reveal social preferences. Aspects of behavior related to redistribution, trust, and entitlements overlap in all three essays.

Redistribution, the voluntary or involuntary reallocation of income amongst individuals, is present in all three essays. In the first essay, redistribution of labor earnings is involuntary but can be avoided if the subject chooses to reduce labor supply. In the second and third essays redistribution is carried out through the social exchange of endowments and surpluses generated or preserved by trust.

Trust-related behavior is present in all three essays. Trust is defined as an action of generosity and/or cooperation in hope or expectation others will return the favor. In economics experiments the betrayal of trust is financially rewarding for the trustee and financially damaging to the trustor. In the first essay trust takes the form of cooperation. Cooperation is signaled to members of the group through labor participation. Higher group cooperation minimizes the burdens of redistribution on total group income. It may

benefit others to be uncooperative in the short-run, but not in the long-run because there is less income to redistribute. In the second and third essays, subjects play the role of trustor, trustee, or both. Three trust games are examined: the Investment Game, the Private Property Trust Game, and the Common Property Trust Game. The social dilemmas surrounding trust are clearer here because the two roles are played out sequentially. All three trust games have the same equilibrium prediction: the trustee will betray the trust of the trustor to maximize his/her own income, and the trustor in expectation of this will not trust. However, evidence shows that many people trust anonymous strangers with their money and their trust is often rewarded (Berg, Dickhaut, and McCabe 1995; Cox 2004; Cox et al. 2009).

Finally, behavior related to entitlements can be found in three essays. Since subjects are paid in all three experiments, subjects are bound to feel some sense of entitlement to their monetary endowments and final earnings. In the first essay, entitlements are strengthened because effort is expended to earn the money that is subject to redistribution by performing a real effort task. Perceptions of entitlements mix with perceptions of fairness: subjects who are relatively less productive benefit from redistribution while subjects who are relatively more productive incur losses from redistribution. In the second essay, entitlements are weakened under role-reversal because only one of the roles is selected for payoff instead of both. Since subjects do not know which role will be selected while making their decisions they may feel less entitled to the endowment or surplus controlled by the first mover and second mover roles. In the third essay, entitlements are strengthened by requiring subjects to meet a performance quota on a real effort task to earn their private property or common property endowments in a trust

game. If they do not meet the performance quota then they do not get to play the trust game.

The purpose of the first essay is to assess the labor supply effects of increasing redistribution by the public sector to provide charitable goods and services. Since redistribution is the public sector's instrument for providing charity, an experiment is designed to test how different levels of redistribution affect the size and distribution of charitable goods and services. Income redistribution, by transferring income from the rich to the poor, will provide charity and increase income equality but may also reduce total income, leading to an efficiency-equality tradeoff. The efficiency loss associated with income redistribution has at least two possible sources: (1) the costs associated with administering income redistribution; and (2) decreased output as income redistribution lowers incentives for work.

The contribution of this essay is the focus placed on how redistribution affects work incentives. This is accomplished by removing the administration costs of redistribution from the experimental design. Income redistribution is implemented by proportionately taxing individuals' labor earnings which are divided equally amongst the group members. Low and high tax rates are implemented in separate treatments to measure effects on efficiency, equality, and labor supply. A second contribution is that work incentives are salient because leisure is offered as an alternative paid activity for subjects. Entry and exit in the labor task is frictionless as subjects freely allocate time between labor and leisure to test how redistribution affects work incentives. A third contribution is the emphasis placed on the fact that many people care not only how redistribution affects their earnings but also how it affects others' earnings and the drivers

behind the outcomes of redistribution. Subjects receive feedback each period telling them whether they are net taxpayers into redistribution or net recipients from it. They are told they are net taxpayers if they are more productive (and perhaps working harder) than the average member of the group. They are told they are net recipients if they are less productive than the average member of the group.

The first essay finds the following. A significant efficiency-equality tradeoff explained by lowered work incentives exists. However, the size of the tradeoff depends on which pairs of low tax and high tax groups are compared. Within each redistribution level, efficiency and equality actually move in the same direction across groups. If a subject is paying more into the redistribution account than they are receiving from it then they know they are more productive and likely working harder than other members of their group. Labor participation becomes the signal of cooperation, and more cooperative groups tend to have both more efficiency and equality within each redistribution level. Labor supply decisions show strategic and cooperative behavior similar to behavior found in public goods experiments (Ledyard, Kagel, and Roth 1995). The efficiency-equality tradeoff calls for a reconsideration of increasing dependence on the public sector for charity provision. The cooperation by labor participation findings leads to promising new research on how fairness attitudes may mitigate or exacerbate the negative effects of redistribution.

The second essay looks at previous experiments designed to see if the results of sequential games, where subjects play a single-role, are robust to applying a role-reversal protocol. In games where there are multiple roles and individuals who play only a single role they may *picture themselves in the other player's shoes* in determining the best

action to take. If role-reversal is applied to the game then a player knows in advance that he or she will be *actually wearing the other player's shoes* in a repetition of the game. Role-reversal may encourage strategic play or promote empathy as a result. Role-reversal may also weaken a subject's sense of entitlement to their role because all subjects have the same opportunity to play each role.

Some researchers use role-reversal protocols to gather more information about how decisions are made in the single-role protocol. This makes role-reversal a valuable empirical tool as long as behavior in the single-role protocol is robust to the application of a role-reversal protocol. A contribution to the literature is made by surveying previous role-reversal studies. The essay finds that evidence on the effects of role-reversal is mixed. The mixed evidence is due to the lack of within-study tests of role-reversal, and the lack of consensus amongst researchers on how to design a role-reversal protocol.

An experiment is conducted comparing behavior in a trust game called the Investment Game (Berg, Dickhaut, and McCabe 1995) to behavior in its role-reversal protocol. The contribution of this essay is to make a distinction between the joint effects of role-reversal and role-payment previously interpreted as a single role-reversal effect. Only one role decision is selected for payoff in the role-reversal protocol to look for a fundamental role-reversal effect in trust games similar to the Investment Game. Paying both roles makes each role decision more payoff salient, but it carries the crucial disadvantage of adding income effects, portfolio effects, and/or feedback effects which cannot be separated from any potential role-reversal effect.

The second essay finds the following. Paying subjects for one role instead of all roles in the role-reversal treatment leads to no significant role-reversal effect. This differs from a previous study which paid both roles and found that applying role-reversal first reduced generosity in both roles (Burks, Carpenter, and Verhoogen 2003).

The third essay, co-authored with James C. Cox, reports a theory-testing experiment designed to test for differences in trust-related behavior amongst private and common property environments. The policy literature assumes that less cooperation and trust will occur in the common property regime. This is due to a confusion of common property environments with open access environments and that a "tragedy of the commons" (Hardin 1968) will occur under common property environments.[1] Cox, Ostrom, and Walker et al. (2009) test payoff equivalent Private Property Trust Games and Common Property Trust Games to test for differences in trust-related behavior. While many theories predict that there should be no behavioral differences between the two games, Revealed Altruism Theory, developed by Cox, Friedman, and Sadiraj (2008), predicts that positive reciprocity (trustworthiness in trust games) could be relatively lower in a Common Property Trust Game if second movers are sensitive to first mover deviations from the status quo opportunity set offered to the second mover. The status quo opportunity set is upheld by the first mover not sending any of his/her private fund to the second mover in the Private Property Trust Game. The status quo opportunity set is upheld by the first mover not withdrawing any of the common fund shared with the second mover in the Common Property Trust Game. Withdrawing any funds from the common fund overturns the status quo set, which may cause second mover preferences to

[1] The "tragedy of the commons" according to Hardin is that more people requires more resources from the commons (our planet), and births are nonexcludable extractions from the resource, unchecked population growth will destroy the commons (Hardin 1968)

become less altruistic in the Common Property Trust Game than if an equivalent amount was not sent to the second mover in the Private Property Trust Game. Cox, Ostrom, and Walker et al. (2009) found marginally greater, albeit insignificant, levels trust and trustworthiness in a Common Property Trust Game compared to the Private Property Trust Game (Cox et al. 2009). We investigate if these results are robust to assigning stronger property right entitlements.

We strengthen subjects' property rights by requiring that all subjects meet a performance quota in an effort task to earn their private or common property endowment. One contribution is that the protocol for strengthening entitlements is kept simple. The size of the private and common property endowments is independent of performance in the effort task. First mover and second mover roles are randomly assigned and are also independent of performance on the real effort task. This keeps senses of entitlement symmetric across roles: no subject should feel more entitled to the property right than the other. Subjects who meet the performance quota will then use their private or common property endowment in the respective Private or Common Property Trust Game. In subsequent treatments we change the manner in which second movers submit decisions: instead of second movers making one decision after learning their paired first mover's decision they are asked to submit a planned response to each possible first mover choice before learning the actual choice. This elicitation method is known as the strategy method protocol (Brandts and Charness 2000). We use the strategy method protocol to provide a direct test of the prediction made by revealed altruism theory within subjects. Because we run treatments with and without the strategy method protocol a second contribution is made in the debate on the effect of applying a strategy method protocol: two new games

are tested and the domain of the debate is expanded to stronger property right entitlements.

We find similar levels of cooperation in both games and a significantly higher variance of cooperation in the Common Property Trust Game. The stronger property rights lead to a divergence between decisions made in the Private and Common Property trust games not found in the previous study. First mover decisions in the Common Property Trust Game move to the extreme decisions of zero generosity and full generosity, traditionally interpreted as “no trust” and “full trust” towards the second mover. First mover decisions in the Private Property Trust Game are similar to decisions made in the previous study. When we switch to the strategy method protocol we find that lower positive reciprocity is found in the Common Property Trust Game. These findings provide support for Revealed Altruism Theory. The question remains why these findings did not occur until the real effort task was added. We offer the following explanation: stronger property rights, by making property ownership more salient, lower second mover generosity in response to withdrawals and motivate first movers to anticipate this in the Common Property Trust Game. We also find that reductions in saliency, achieved either by removing the real effort task or by switching to the strategy method protocol, lead to more generous decisions in both first movers and second movers. We offer the following explanation: reductions in saliency may lower the emotional expectations of first movers and second movers. First movers may be less attached to the money lost and thereby are less prone to betrayal aversion when there is no real effort task. First movers may be less prone to betrayal aversion when second movers submit a strategy method protocol instead of a more direct and personal response. Second movers may not expect

as much when they put less effort towards their earnings and are therefore less disappointed with less generous first mover decisions. Second movers are forced to compare all first mover choices under the strategy method protocol and are less likely to play ignorance to fact that the first mover could have offered less generous opportunity sets.

Findings from each essay contribute to our understanding of social preferences by examining behavior that is motivated by trust, reciprocity, fairness, and cooperation. A particular contribution is in the methodology of examining this type of behavior in laboratory experiments. Traditionally subjects are given a randomly assigned role which comes with an endowment or inducement with which they use to make decisions dictated by the order of the game. These conventions are broken in this dissertation. Endowments are earned, sometimes more than one role is played, and sometimes decisions are made in advance. These changes move laboratory experiments another step in the direction of external validity, which can lead to more robust theory and better inform policy.

CHAPTER II

THE EFFICIENCY-EQUALITY TRADEOFF IN PUBLIC SECTOR CHARITY PROVISION

Introduction

The welfare of the unfortunate continues to be a concern to society. Many people help those less fortunate by donating time, money, and/or other resources through various independent sector organizations; but, sometimes voluntary efforts for the provision of public goods such as poverty alleviation do not have satisfactory scale. On that basis some people advocate for government provision.[2]

The government provision of public goods has the advantage that it can compel the acquisition of capital through taxation. But, government cannot ratchet up taxes without limit. Economic theory, as well as empirical and experimental evidence, suggests that the government has diminishing and perhaps even negative returns over some levels of taxation. This study assesses the inefficiency of public sector charity provision; specifically, the inefficiencies of redistribution of income from an effort based task.

Background and Literature

One type of charitable redistribution commonly used by the public sector is income redistribution. For example, some people pay positive federal income taxes, and others receive federal earned income tax credits. Income redistribution, however, can lead to inefficiencies. This indicates that there can be a tradeoff between efficiency and reductions in poverty and inequality. We must decide how much efficiency we are willing to sacrifice for a given reduction in poverty and inequality (Okun 1975).

[2] The private sector consists of all for-profit firms, the public sector consists of all government entities, and the independent sector consists of households, nonprofit businesses, individual volunteers, and donors (Cornuelle 1965).

What is the source of the inefficiency? Some of it comes from the administration costs of redistribution. Administration costs include the resources used by the government to collect and redistribute income, as well as the compliance costs of net taxpayers and application costs of net recipients.

A second source of inefficiency is those distortions in work incentives caused by redistribution financed by an income tax. Theory tells us that people who are “net tax payers” work less when the substitution effect (a negative labor supply response to higher taxes on wages) dominates the income effect (a positive labor supply response to maintain the same standard of living. Such a reduction in work effort results in inefficiency (Break 1957; Brown 1980; Okun 1975).

Income redistribution can also result in allocative inefficiency. The public may at first advocate for the public sector to increase redistribution in order to provide a social safety net. Redistribution funds can be used for other purposes instead (Mueller 2003). In particular, redistribution funds can be captured by groups with outside interests through rent-seeking activities (Linster 1993; Stigler 1971).

The effectiveness of the public sector in reducing poverty is mitigated by the problems associated with income redistribution: administration costs, disincentives to work, and the misallocation of redistribution funds. The beauty of laboratory experiments is that a controlled and simplified environment can be set up to analyze one problem at a time. This research asks, “Assuming zero administration costs and proper allocation of redistribution funds, does redistribution still result in an efficiency loss due to lowered work incentives?”

This experiment builds upon previous experiments that give predictions on how redistribution may affect the labor supply of taxpayers and recipients. The Laffer Curve states that tax revenue will rise, peak, and fall as the tax rate increases (Laffer 2004). Naturally, the public sector's capacity for charity provision follows this pattern.[3] Field studies have looked at the effects of income redistribution on the recipients' labor supply. The most famous and expensive studies were the negative income tax experiments. The Negative IncomeTax experiments provided in-cash transfers to households which fell below a specified minimum income level. The transfers decreased in size as the recipient's household income approached the maximum level of eligibility. This created dynamic income and substitution effects. Five separate NIT experiments were conducted in the 1960s and 1970s. Four of the five programs found significant reductions in labor supply (Brown 1980).[4]

While the variance of tax rates in the field is limited, laboratory experiments have the advantage of looking at all possible tax rates. Some have in fact replicated the Laffer Curve (Lévy-Garboua, Masclet, and Montmarquette 2009; Ottone and Ponzano 2007; Sutter and Weck-Hannemann 2003; Swenson 1988).

The experiment runs low and high tax rate treatments to look for changes in efficiency, equality, and labor supply. Subjects allocate their time between performing a labor task and taking leisure each period. Subjects' labor earnings were taxed and redistributed equally at the end of each period, making subjects who earned above (below) the group average net taxpayers (net recipients). We find a significant efficiency-

[3] Unlike the Laffer Curve, the efficiency-equality tradeoff only requires an overall negative labor supply response to higher taxes.
[4] The New Jersey Experiment (or Urban Income Maintenance Experiment) showed an insignificant household labor supply response.

equality tradeoff. The primary efficiency loss comes from reduced output following subjects' reduced labor supply. Subjects reduce their labor supply by allocating more time towards leisure activities. We also find that the efficiency-equality tradeoff does not always hold: if labor productivity and cooperation is high enough to keep inequality low, intensive redistribution can reduce both efficiency and equality. A higher level of income redistribution significantly reduces income inequality, but fewer funds are available for charity provision when there are large reductions in labor supply.

Experimental Design[5]

The experimental design consists of two treatments differing only in the tax level used to carry out redistribution. Subjects participate in only one of the two tax treatments: a 20% tax treatment or an 80% tax treatment. The experiment is designed to test the following hypotheses: (1) efficiency will be lower in the 80% tax treatment; (2) inequality after redistribution will be lower in the 80% tax treatment; (3) labor supply will be lower in the 80% tax treatment; and (4) the significance of the tradeoff will depend on within-group cooperation as subjects compare their labor participation and output to other group members.

Key features of the experimental design include: (1) zero administration costs in the redistribution process; (2) homogeneous labor task and leisure options; (3) subjects make labor-leisure choices in real time; (4) subjects receive feedback on whether they gain or lose money from income redistribution; and (5) subjects receive information on their opportunity cost of labor (their potential leisure earnings).

[5] Subject instructions for the experiment are located in Appendix A.

These features are important for testing the efficiency-equality tradeoff effectively. Administration costs are removed such that efficiency is only a measure of subject output. Subjects have homogeneous labor and leisure tasks to allow inequality to arise naturally: inequality comes only from heterogeneity in the ability and effort levels subjects bring to the lab. Homogenous labor and leisure tasks also remove fairness considerations external to the fairness considerations surrounding redistribution. Allowing subjects to allocate his or her time between labor and leisure removes friction from the process of self-selecting into and out of the labor force. Subjects are given feedback on whether they gain or lose from redistribution so we can test whether net taxpayers and net recipients behave differently. Subjects are also given information on their potential leisure earnings because this information will also play a role in their decision to substitute labor for leisure.

Both treatments of this experiment involve subjects performing a repetitive labor task. Subjects perform the labor task on a computer using a program created in Visual Basic. The labor task is to type, exactly as shown on the screen, a random sequence of five single-digit numbers between 0 and 9. Figure 1 shows a screenshot of the labor task. Each time a subject copies the sequence of characters correctly a confirmation of "CORRECT!" flashes on their screen and the subject receives a piece-rate payment. Each time a subject copies the sequence of characters incorrectly a confirmation of "WRONG!" flashes on their screen, but there is no penalty, only the opportunity cost of forgone leisure earnings he or she could have made instead while typing the incorrect sequence. This work task follows a similar work task used by Dickinson (1999) where subjects were paid a piece-rate each time they copied a paragraph (Dickinson 1999).

There are thirteen periods in each treatment: one Double Pay Period followed by 12 Regular Pay Periods. Each period lasts 5 minutes and 30 seconds. The last thirty seconds of each period give subjects time to review their earnings. The Regular Pay Periods last a total of one hour plus six minutes of review time.

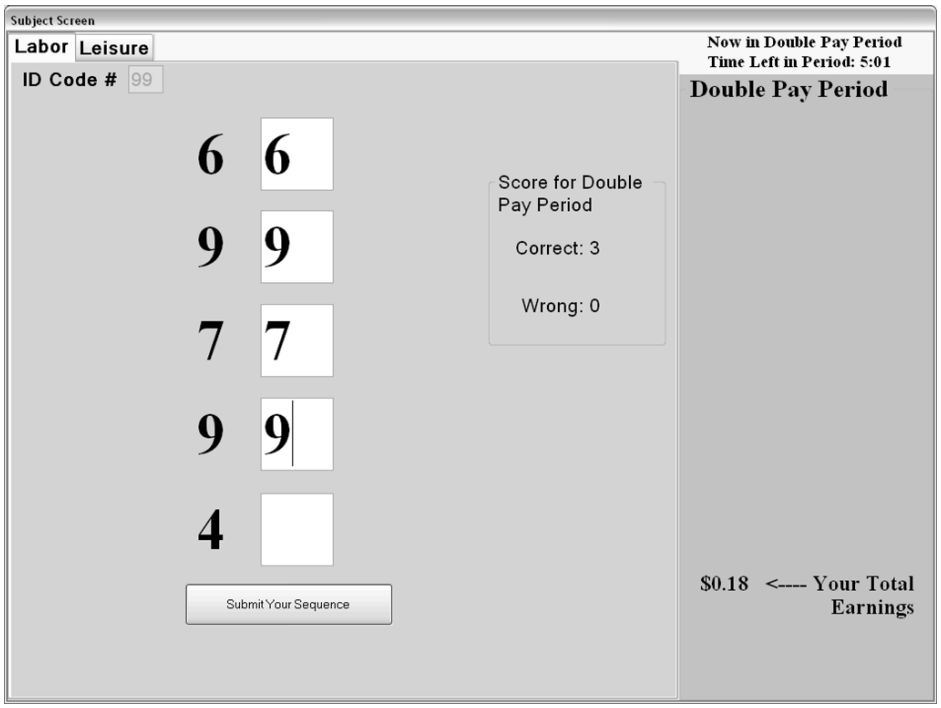

Figure 1. Computer Screenshot of a Subject's Labor Task

In the Double Pay Period subjects earn a piece-rate of $0.06 for each correctly typed sequence. This is double the piece-rate subjects are paid for the remaining Regular Pay periods. We double the pay in order to give subjects extra incentive to work at their productivity threshold. The number of correct sequences a subject types during the Double Pay Period gives a raw measure of ability. We use subjects' ability measures to control for heterogeneity in ability, since changes in effort are of greater interest. The Double Pay Period also ranks subjects on their productivity to build groups of five for the

Regular Pay Periods. We rank subjects on their productivity to create groups with similar compositions of low, medium, and high productivity members. The rank-to-group matching procedure is taken from Blumkin, Ruffle, and Ganon (2008), is shown in Table 1 (Blumkin, Ruffle, and Ganun 2008). This group formation procedure serves two major purposes: (1) to ensure that a wide initial income distribution exists in each group given similar effort levels; and (2) to reduce variation in total productivity across groups for better intergroup comparison.

Table 1. Group Assignment According to Rank in Double Pay Period

Rank in Double Pay Period	Group Number
1	1
2	2
3	3
4	3
5	2
6	1
7	1
8	2
9	3
10	3
11	2
12	1
13	1
14	2
15	3

The Regular Pay Periods introduce more complex work incentives because subjects now make real labor-leisure decisions. At any time during the Regular Pay Periods subjects can switch from labor to leisure and back by clicking the Labor and Leisure Tabs shown on their computer screen. Subjects can make labor earnings and leisure earnings but not both at the same time, so they must allocate their time between

receiving labor and leisure earnings. We induce subjects' value for the leisure task: subjects earn 1 cent for every two seconds the Leisure Tab is active. Induced values of leisure are used instead of homegrown values because Esarey, Salmon, and Barrilleaux (2007) found that the redistributive tax rate had no significant impact on productivity in their experiment. The explanation they offer is "subjects had nothing else to do but produce" (Esarey, Salmon, and Barrilleaux 2007). Figure 2 shows a subject screenshot of the leisure task. When the Leisure Tab is active, there is no real leisure activity for subjects to take part in. Subjects could rest their head, daydream, or read quietly, but no suggestions for leisure activities are made. This is done to remove any positive or negative framing for taking leisure over labor. External validity lies in favor of homegrown values of leisure, but homegrown values are difficult to measure. In addition homegrown values are too low in the lab because subjects participate with intentions to make money and can delay taking leisure until after the experiment. The objective is to take a step closer towards external validity by allowing subjects not only to endogenously choose their productivity output, but also self-select into and out of the taxed labor sector freely.

The Regular Pay Periods have the same labor task as the Double Pay Period except subjects receive a lower pay and face redistribution. First, they earn a lower piece-rate of 3 cents for each correct labor task. Second, their labor earnings are subject to redistribution. Subjects' leisure earnings are free from taxation, so they simply keep their leisure earnings. Subjects' labor earnings are subject to redistribution as shown in the following equation:

$$\pi_i^A = (1-\tau)\pi_i^B + {}^1\!/_5 \sum_{i=1}^{5} \tau\pi_i^B$$

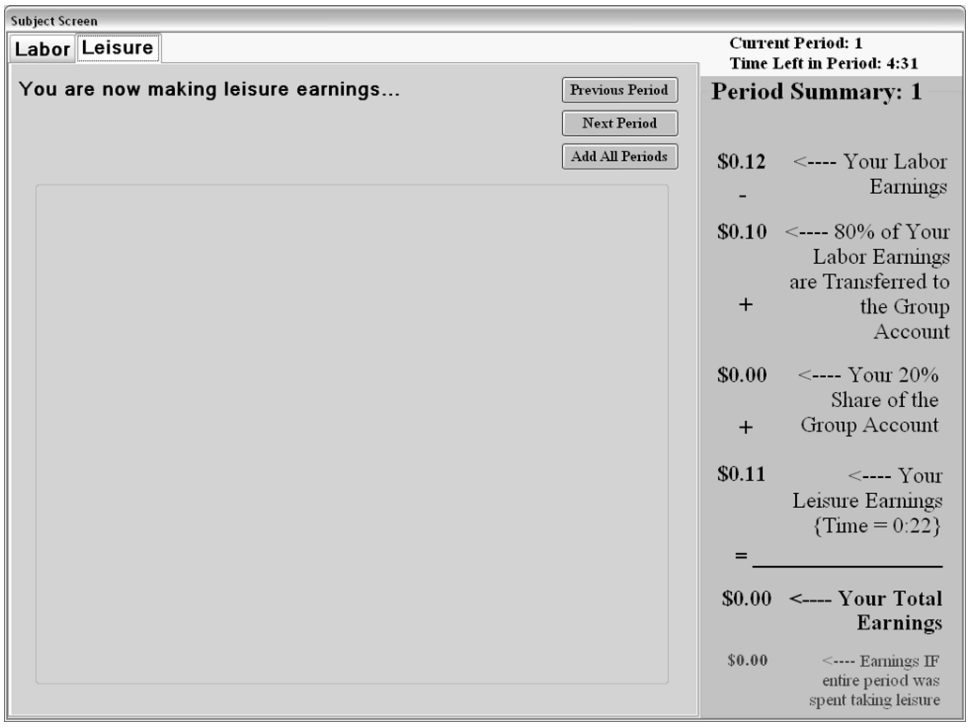

Figure 2. Computer Screenshot of a Subject's Leisure Task

At the end of each Regular Pay Period, represents the labor earnings before taxation and redistribution, and represents the labor earnings after. Here is the tax rate, or the proportion of labor earnings to collect each period for redistribution. The collected labor earnings are put into a Group Account, which is split equally amongst all members of the group. The redistribution equation keeps subjects from trading positions on the income distribution due to redistribution, so position changes will only occur if a subject becomes relatively more productive (or another subject becomes relatively less productive) in the next period.

Subjects receive feedback on how the choices of others in their group affect their earnings through income redistribution. Subjects receive feedback information, shown in Figure 3, on the right side of their screen. During a period, subjects receive constant feedback on what their labor earnings are before redistribution and how much labor

earnings they are transferring to the Group Account. Subjects' leisure earnings increase continuously whenever their Leisure Tab is active. At the end of each period, subjects are given their 20% or one-fifth share of the Group Account. Their total earnings at the end of each period equal:

Total Earnings = Labor Earnings – Labor Earnings Transferred To the Group Account + 20% Share of the Group Account + Leisure Earnings

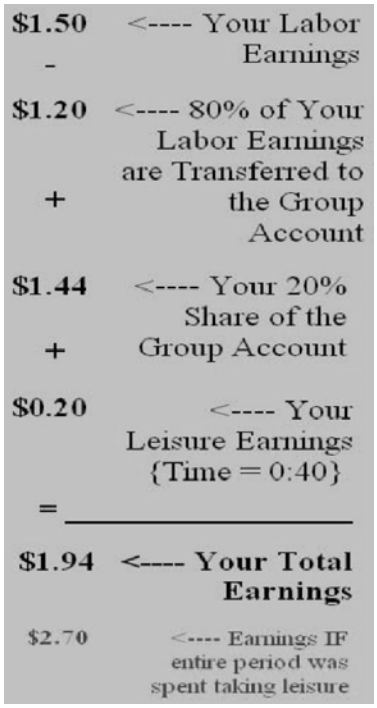

Figure 3. Computer Screenshot of a Subject's End of Period Earnings Information

Subjects are told that if the amount they transfer to the Group Account is larger (smaller) than their 20% share of the Group Account then: (1) they have completed more (fewer) sequences than the average member of their group; and (2) have contributed more to the Group Account than at least one other member. Also given at the end of each period is additional information on "Earnings if the entire period was spent taking

leisure." This gives subjects their opportunity cost of labor under income redistribution, which is simply their forgone leisure earnings plus their share of the Group Account (without their labor earnings transfer). We tell subjects that this number represents what their earnings could have been if they spent the entire period with the Leisure Tab active. Subjects see how this number is calculated in the instructions. It is the maximum leisure earnings of $1.50 (equal to 300 seconds * $0.005) plus their 20% share of the Group Account after excluding their transfers. We remind subjects that this is hypothetical earnings and that if everyone in the group spent the entire period in leisure then the Group Account would be zero and each member would make $1.50 for the period. Subjects receive this information to help them recognize the incentives present in the design and to lower their decision making cost.

The Double Pay Period is the same for both treatments. The Regular Pay Periods differ across treatments only in the tax level. Two treatments are run: a 20% tax rate ($\tau = 0.2$) and an 80% tax rate ($\tau = 0.8$). Using an across-subjects treatment design does have the potential drawback of sample heterogeneity across treatments, but using a within-subjects treatment design can cloud treatment effects because order effects can arise as productivity and cooperation vary over time with any given tax rate. In all treatments, subjects' earnings are the sum of their Double Pay Period earnings and Regular Pay Periods earnings.

This design creates incentives quite similar to those present in voluntary public goods provision experiments which also have important implications for charity (Ledyard, Kagel, and Roth 1995): (1) the pool of funds collected for redistribution are in fact a public good; (2) income earned from leisure provides a pure private return; (3)

income earned from labor provides a private and public return; (4) the supply of labor becomes a 100% contribution to the public good as the tax rate approaches 100%; and (5) subjects are given feedback at the end of multiple periods which allows them to compare their contribution to the public good (labor productivity in this experiment) to other group members' contributions.

There are major differences, however, between this experiment and other voluntary public goods experiments. First, the activity (labor) which increases provision of the public good (the Group Account) yields both a public (taxed labor earnings) and a private return (untaxed labor earnings). The marginal per-capita return from typing one correct sequence differs across tax treatments:

1. The marginal per-capita return from the Group Account is 0.2*0.2*0.03= $0.0012 for the 20% tax rate and 0.8*0.2*0.03 = $0.0048 for the 80% tax rate. Here the return is 4 times larger in the 80% tax treatment.
2. The marginal after-tax return is 0.8*0.03=$0.024 for the 20% tax rate and 0.2*0.03=$0.006 for the 80% tax rate. Here the return is 4 times larger in the 20% tax treatment.
3. The marginal total return to labor is $0.0012+$0.024 = $0.0252 for the 20% tax rate and $0.0048+$0.006=$0.108. Here the return is 2.333 times larger in the 20% tax rate.

Leisure yields a 100% private return of $0.005 per second. Effort is not directly observable. Each correctly typed sequence is a function of an individual's ability, effort, and time supplied to labor, which cannot be perfectly separated. Subjects' labor time and effort are observable to the experimenter, but subjects do not know other members' exact

time and effort placed into increasing the Group Account. Subjects have to infer what each group member's labor supply is by comparing their share of the Group Account with their transfers to the Group Account. Finally, the individual returns to labor and leisure are dependent not only on the contributions of others but also on the individual's productivity. Therefore the endowment of productive ability is endogenous, thus making the ability to contribute asymmetric across subjects.

Results and Analysis

The 20% and 80% tax treatments each consisted of two sessions. All four sessions were conducted using undergraduate students at Georgia State University. Each session included 15 subjects, who were split into three groups of five for the Regular Pay Periods. Table 2 labels the six 20% tax groups 1-6 and the six 80% tax groups 7-12.

Table 2. Group Treatment and Session Assignment

Group Number	Treatment	Treatment Session
1	20% Tax	1st Session
2	20% Tax	1st Session
3	20% Tax	1st Session
4	20% Tax	2nd Session
5	20% Tax	2nd Session
6	20% Tax	2nd Session
7	80% Tax	1st Session
8	80% Tax	1st Session
9	80% Tax	1st Session
10	80% Tax	2nd Session
11	80% Tax	2nd Session
12	80% Tax	2nd Session

Efficiency is measured in terms of productivity so the loss of efficiency will be measured by the reduction in number of correct tasks completed. Figure 4 shows the mean number of tasks completed by treatment. Period 0 represents the mean number of

correct sequences typed in the Double Pay Period, which shows that on average the latent ability to type sequences is the same across treatments. Periods 1 through 12 represent the Regular Pay Periods. Here the mean of all individual correct tasks completed equals the mean of the group means for each tax rate. Under the 20% tax, production is relatively stable and increases towards the later periods. Under the 80% tax production is more erratic and declines as subjects approach later periods. The mean number of correct tasks performed in the Regular Pay Periods is 50.69 for groups facing a 20% tax with an 8.36 standard deviation across the group means. For groups facing an 80% tax the mean number of sequences typed correctly is 32.32 with an 8.24 standard deviation across the group means. This is a 36.3% average productivity loss for each period. Using a t-test with unequal variances to compare the mean of the group means for each tax level, the null hypothesis that the means of the six group means for the two tax levels are the same is rejected at the 1% level (p-value = 0.0024), with the 20% tax groups being more efficient (p-value = 0.0012).

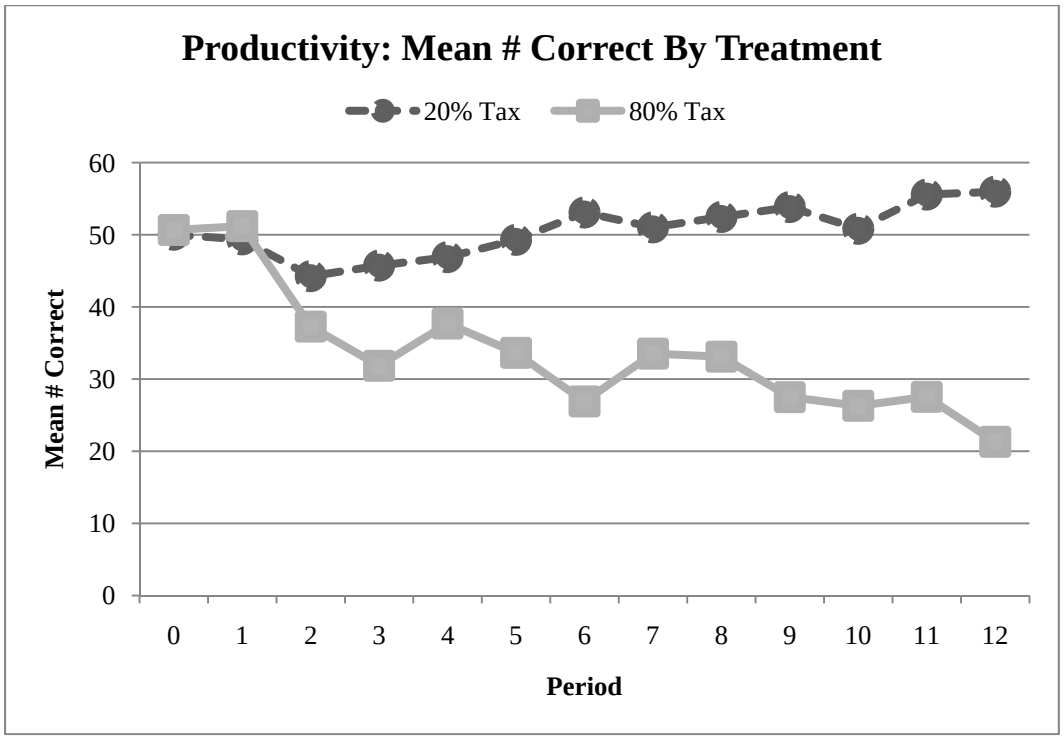

Figure 4. Efficiency Measured in Productivity by Treatment

Figure 5 breaks down the mean number correct by session into four graphs. Each graph shows the mean productivity of each group in the session. Average performance in the Double Pay Period is very close across groups in each session. Production under the 20% tax level is relatively the same for all groups, and the null hypothesis that they are the same cannot be rejected at the 10% level of statistical significance. Production under the 80% tax level does vary across sessions as the second session does not have as dramatic decay in productivity as the first. This is because the groups in the second 80% tax session choose to be more productive. Using a t-test with unequal variances to compare the mean of the group means for each 80% tax session, the null hypothesis that the mean of the three group means for each 80% tax session are the same is not rejected at the two-tailed test (p-value = 0.2837).

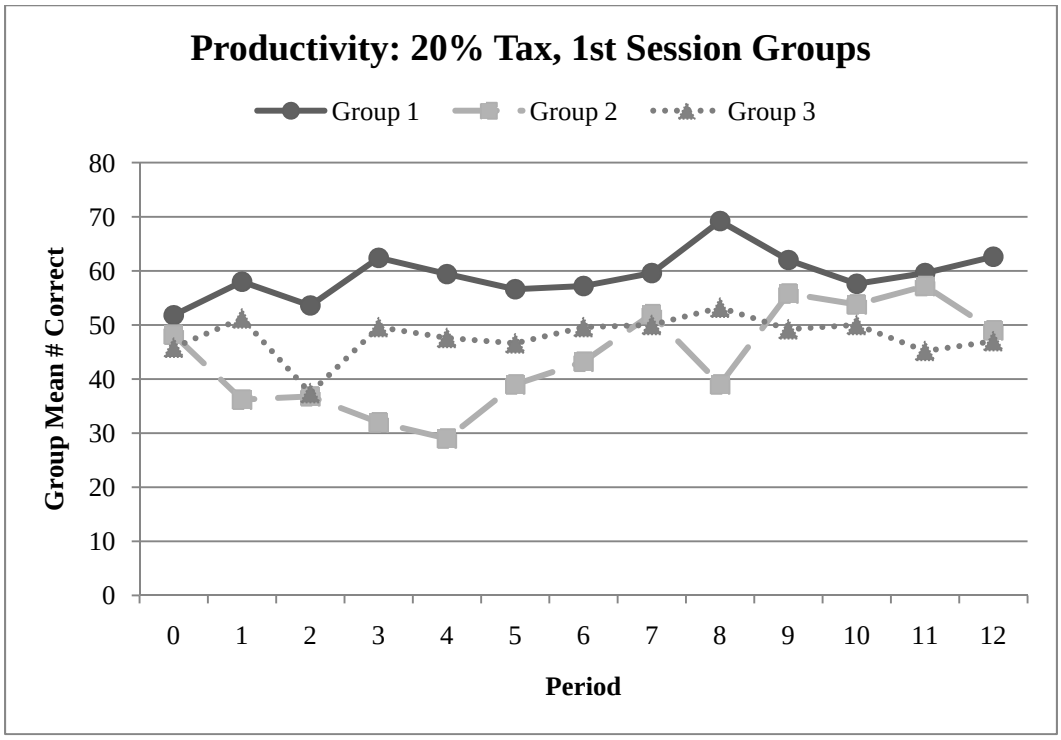

Figure 5. Efficiency Measured in Productivity by Session and Group

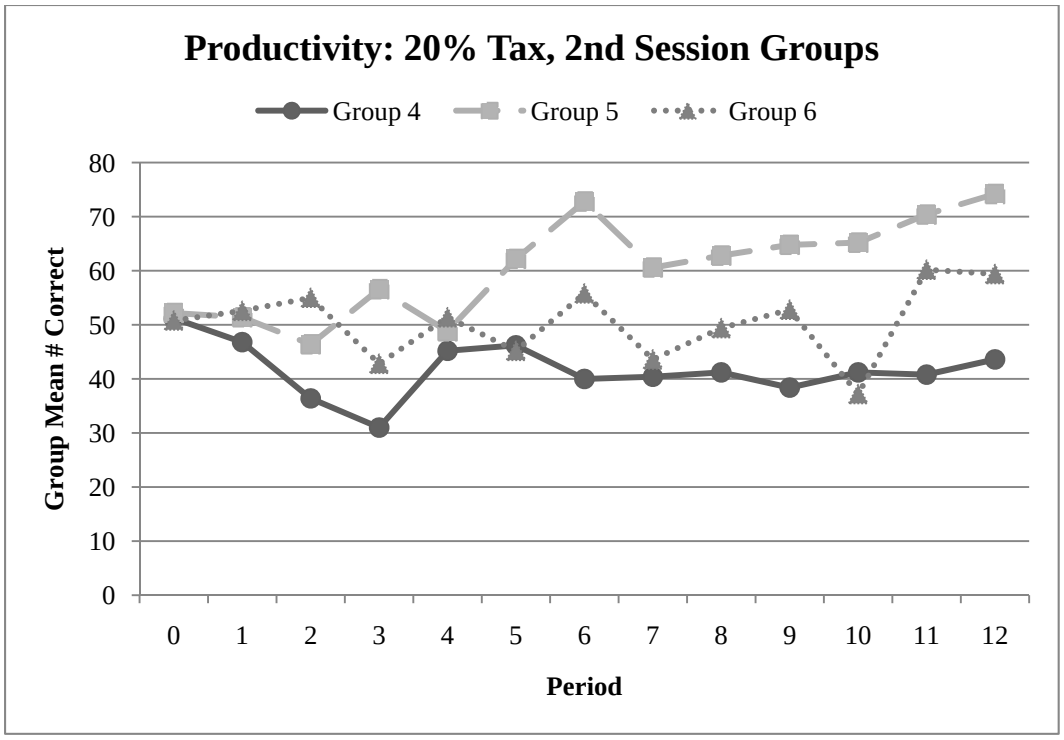

Figure 5 (Continued). Efficiency Measured in Productivity by Session and Group

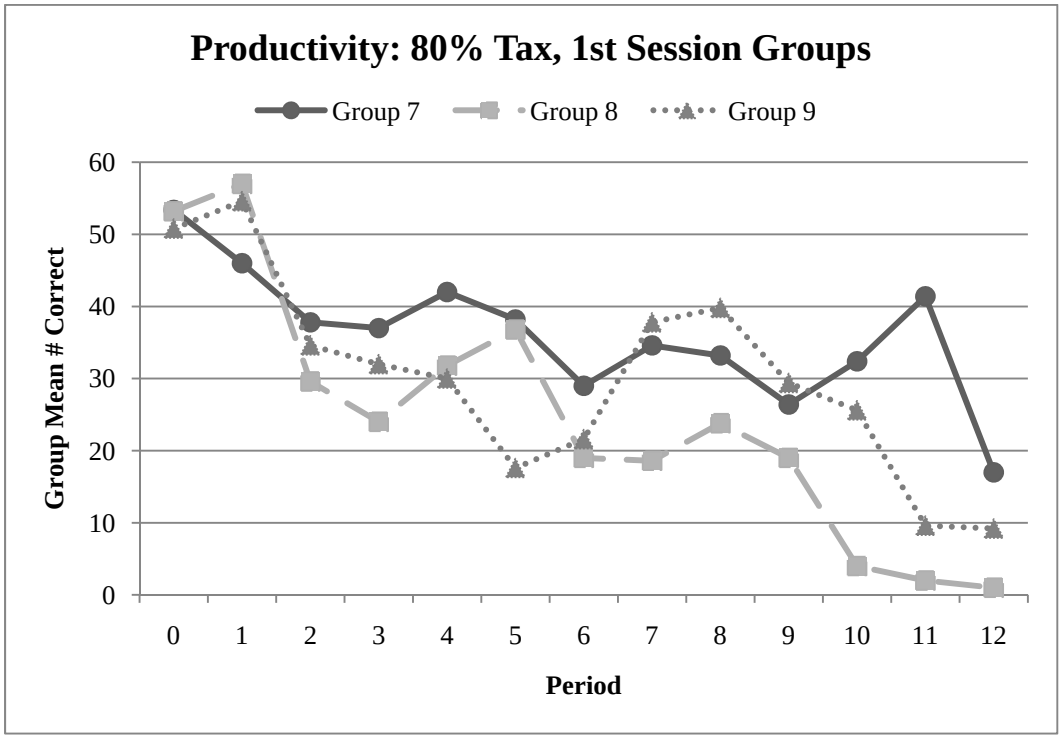

Figure 5 (Continued): Efficiency Measured in Productivity by Session and Group

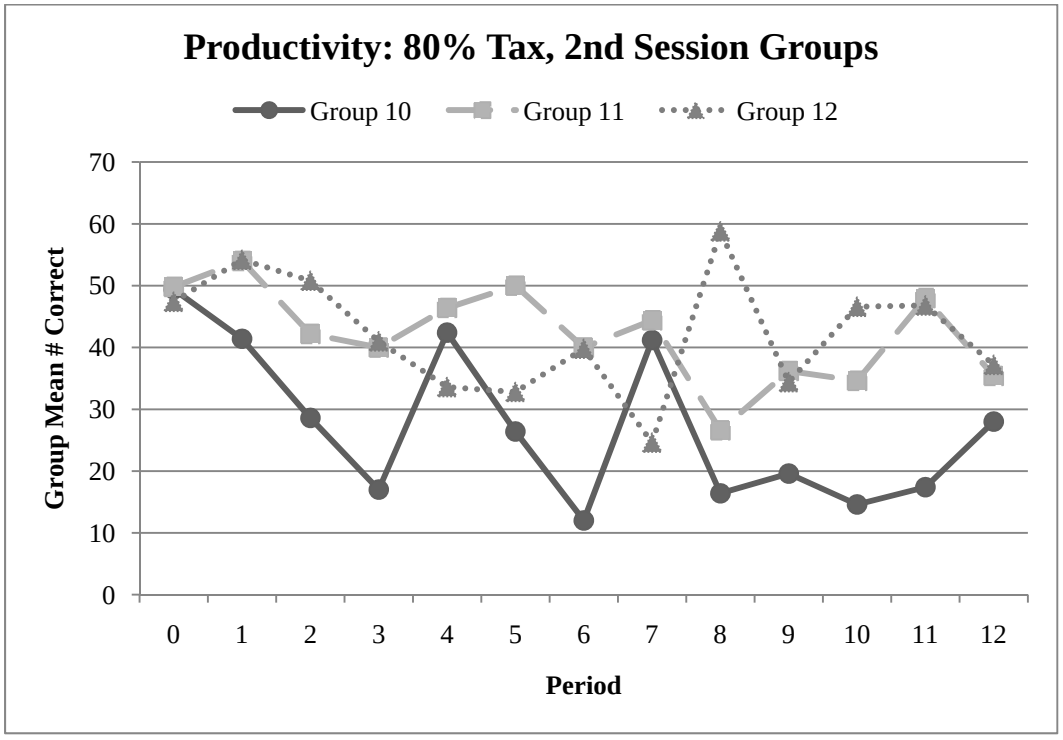

Figure 5 (Continued). Efficiency Measured in Productivity by Session and Group

Figure 6 shows the percentage loss in mean productivity over periods. The percentage loss in mean productivity increases over periods. If subjects were merely responding to the decrease in wage from the tax, then the response should happen rather quickly since the changes in wages from the tax are very salient. Subjects are responding to something else, perhaps subjects learn about their productivity and the size of the Group Account over time and decide that leisure is a more attractive option in the 80% tax treatments. Cooperation may play an important role in differences in productivity across groups.

The results show a significant loss in efficiency (productivity) over time when the tax is increased from 20% to 80%. Following Okun's Leaky-Bucket test, is there a gain in equality that would make us tolerant of this efficiency loss? There are many ways to measure the inequality of an income distribution. We use the GINI coefficient because it carries the benefit of scale invariance so the inequalities can be compared across treatments. GINI coefficients of labor income inequality and productivity inequality are the same due to scale invariance and the piece-rate pay. The GINI coefficient measures the dispersion of distributions giving a score of 0 for a perfectly equal distribution to 1 for a perfectly unequal distribution. What was the level of inequality in the Double Pay Period, before redistribution was introduced? In the first Double Pay Period the 20% tax groups' GINI coefficients are 0.081, 0.086, 0.089, 0.117, 0.128, 0.141 (average 0.107), and the 80% tax groups' GINI coefficients are 0.111, 0.112, 0.113, 0.118, 0.118, and 0.125 (average 0.116). As the GINI coefficients demonstrate, inequality of productivity in performing the labor task starts low, a consequence of the group procedure designed to

create groups similar in distribution of raw ability and the low difficulty of performing the labor task.

In the Regular Pay Periods the inequality of labor earnings after redistribution are of central interest for this is the final income distribution. The 80% tax is designed to reduce inequality more than the 20% tax, but by how much? Figure 7 shows how inequality within groups changed during the Regular Pay Periods. Again, period 0 represents the Double Pay Period. Looking at the graphs, there is greater separation in GINI coefficients over periods in the 20% tax sessions. The average of the GINI coefficients for each group over all periods is taken and averaged with the other groups of the same tax rate. This gives an average GINI coefficient of 0.212 for the 20% tax and a GINI coefficient of 0.094 for the 80% tax. GINI coefficients were quite low in both treatments on average, which is likely due to the fact that labor was homogeneous across tasks leading subjects to become more equal in ability with practice.

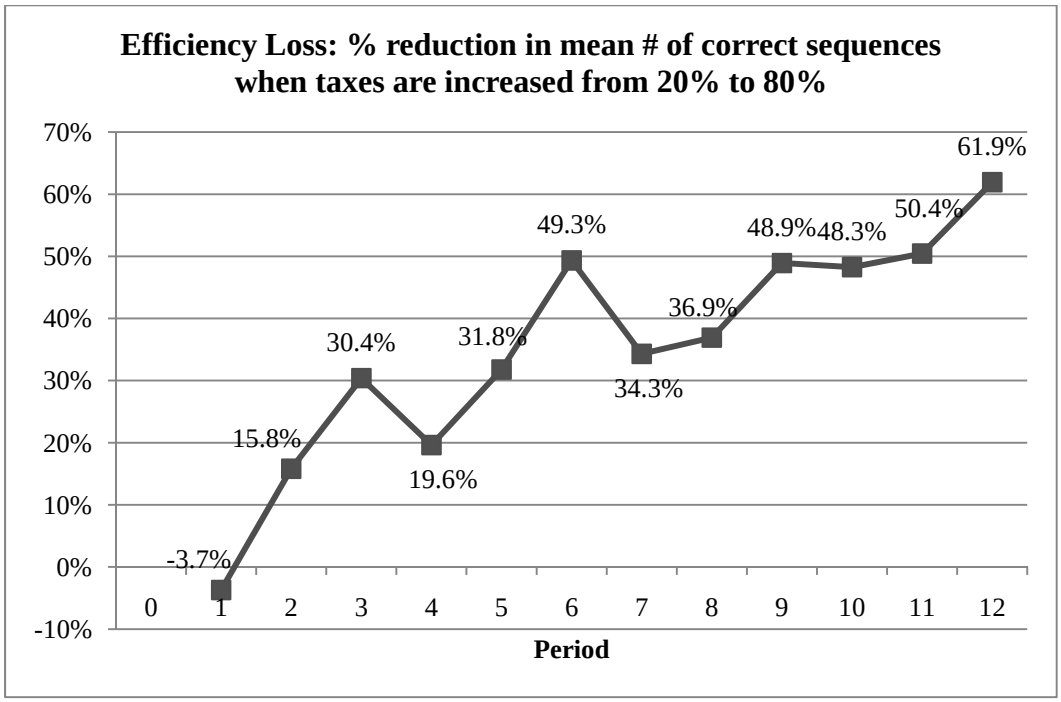

Figure 6. Efficiency Loss Measured in Percentage Change

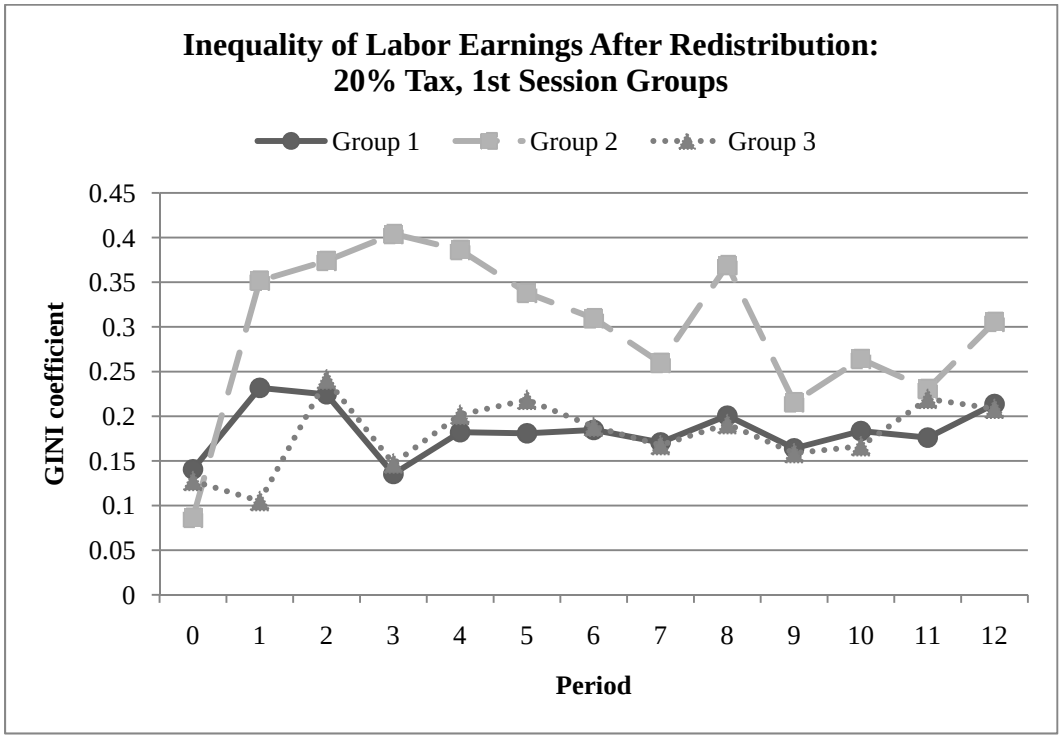

Figure 7. Changes in Inequality of Labor Earnings by Session and Group

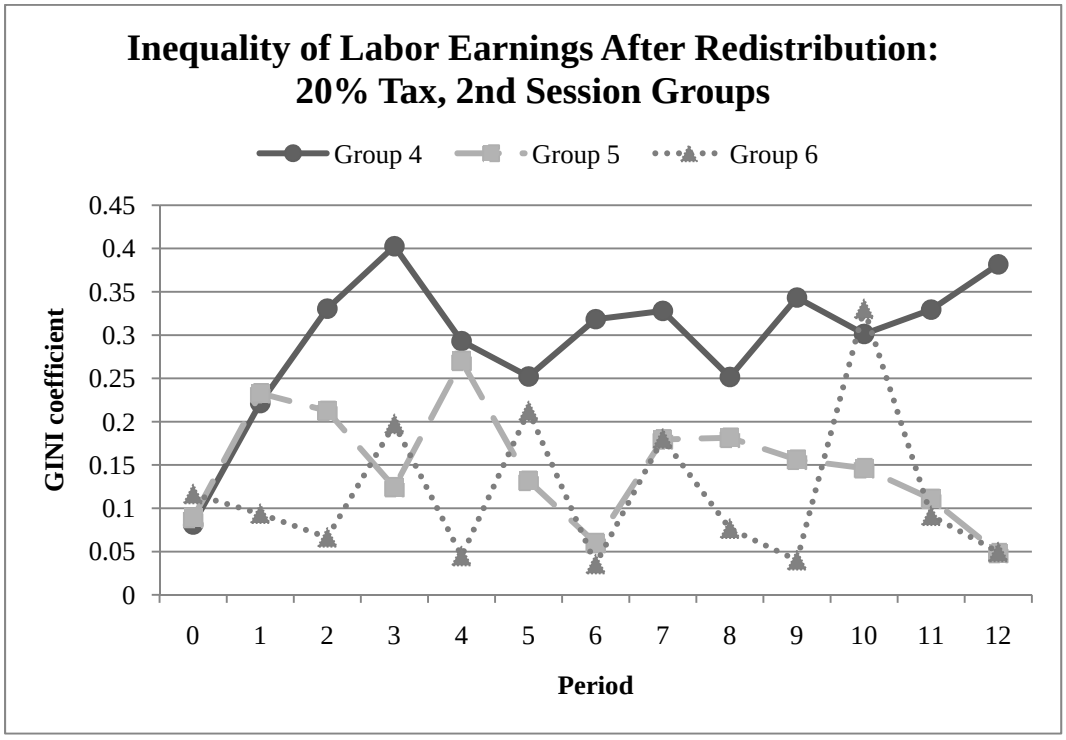

Figure 7 (Continued). Changes in Inequality of Labor Earnings by Session and Group

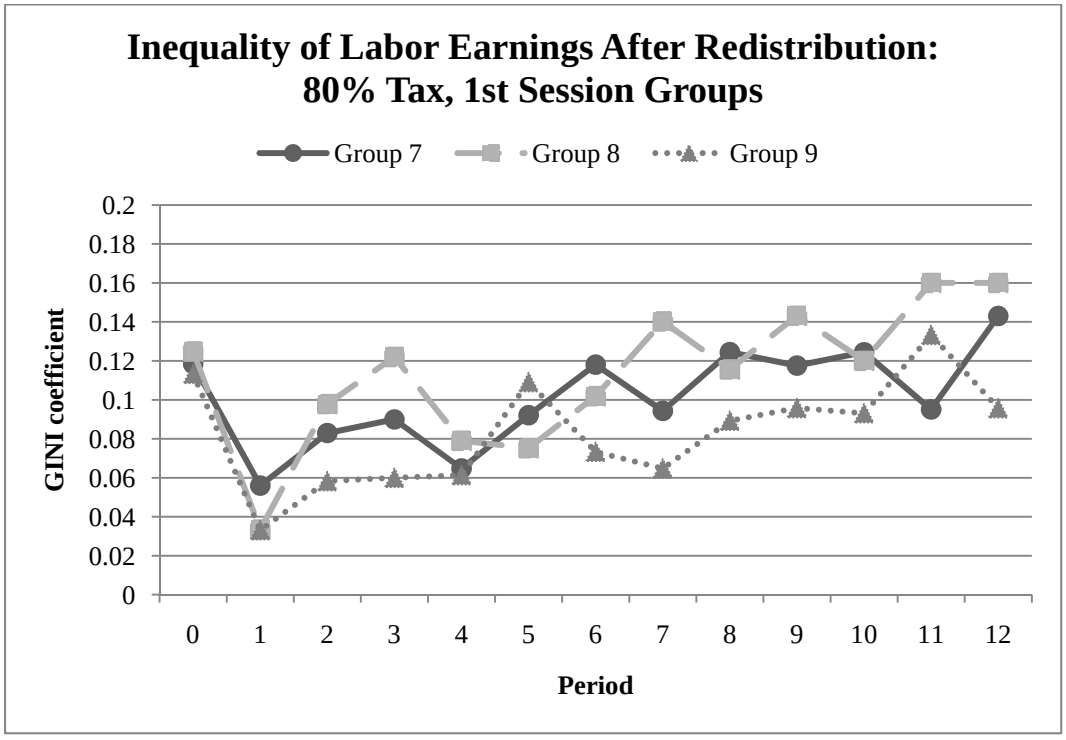

Figure 7 (Continued). Changes in Inequality of Labor Earnings by Session and Group

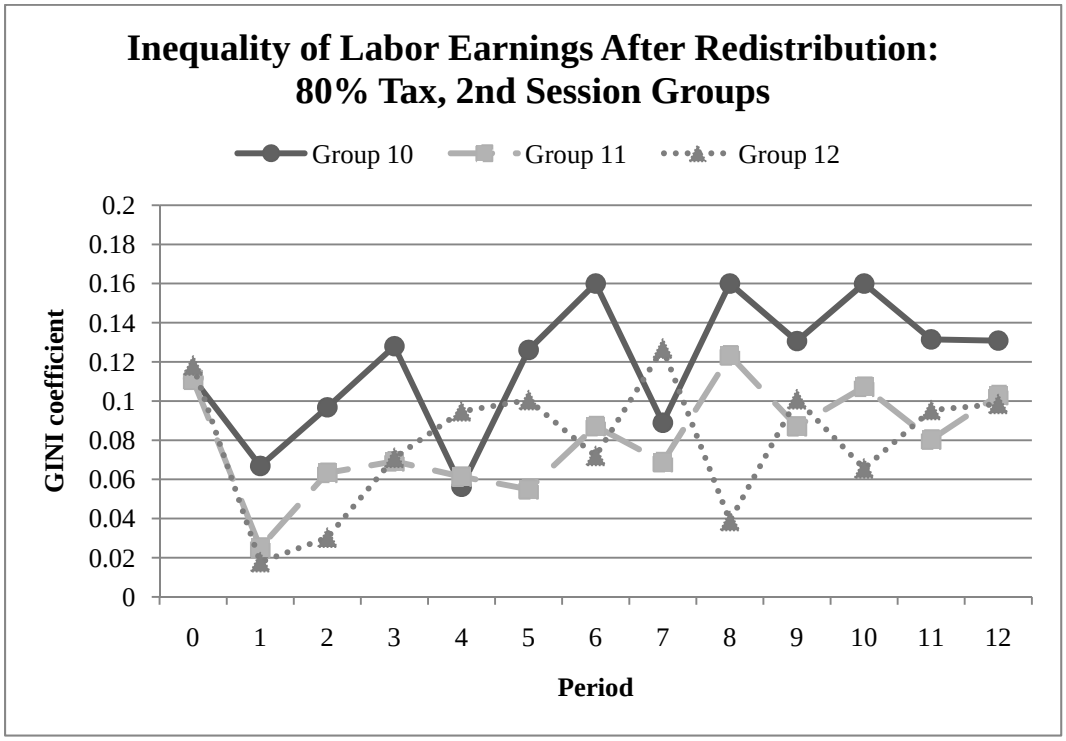

Figure 7 (Continued). Changes in Inequality of Labor Earnings by Session and Group

What are the efficiency-equality tradeoffs across-groups? Figure 8 maps the average efficiency and inequality averaged over all Regular Pay Periods for each group. It is clear from the graph that there is an overall efficiency-equality tradeoff when the redistribution level is increased by increasing labor taxes. However, the tradeoff varies depending on which groups are paired for comparison. If the least efficient 80% tax groups are paired with the most efficient 20% tax groups a much larger tradeoff (a large drop in productivity for a small gain in equality) is observed. If the most efficient 80% tax groups are paired with the least efficient 20% tax groups a much smaller tradeoff is observed. Some may argue that the real tradeoff is measured in the long run because individuals learn how redistribution affects them and they reduce their labor over time in response to the incentives created by redistribution. In consideration of this argument

Figure 9 shows three graphs of group tradeoffs in the 1st, 6th, and 12th Regular Pay Periods. After the 6th Regular Pay Period the inequality of labor earnings after redistribution for the two most productive 20% tax groups are actually lower than all of the 80% tax groups. The results suggest that it is possible to lose both efficiency and equality over time if the tax rate is high enough. If groups are only compared with other groups facing the same tax then there is no tradeoff because efficiency and equality move in the same direction. This can be seen in Figures 8 and 9. This is likely due to the fact that inequality was already low in the Double Pay Period, so if everyone is working then efficiency is high and inequality stays low.

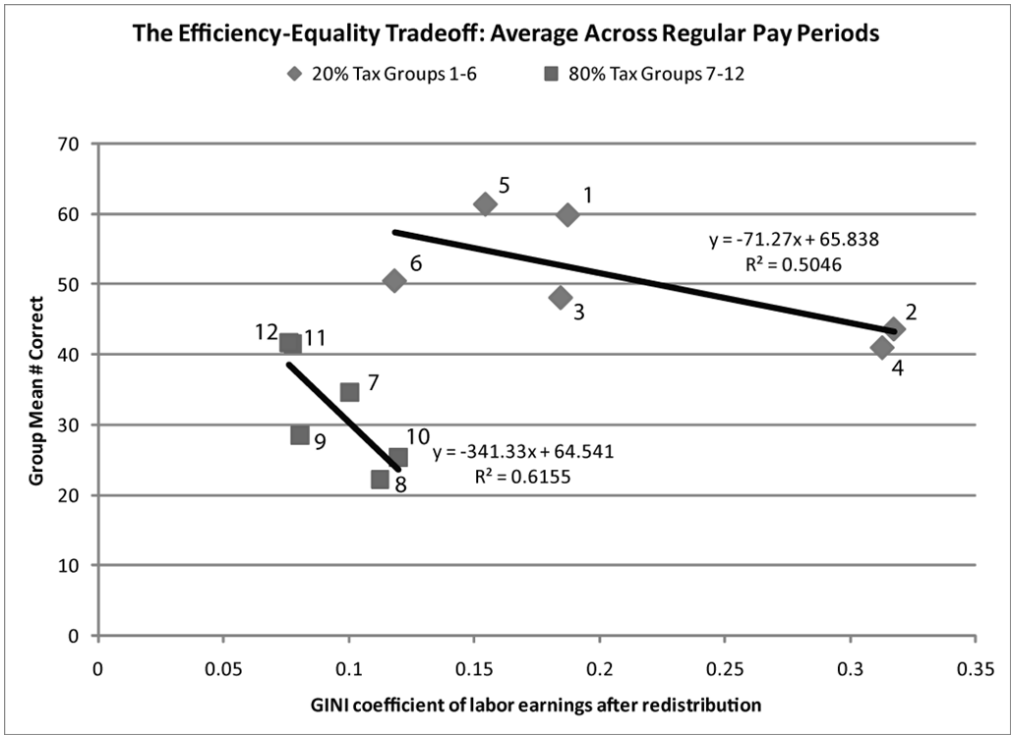

Figure 8. The Efficiency-Equality Tradeoff (All Periods Averaged)

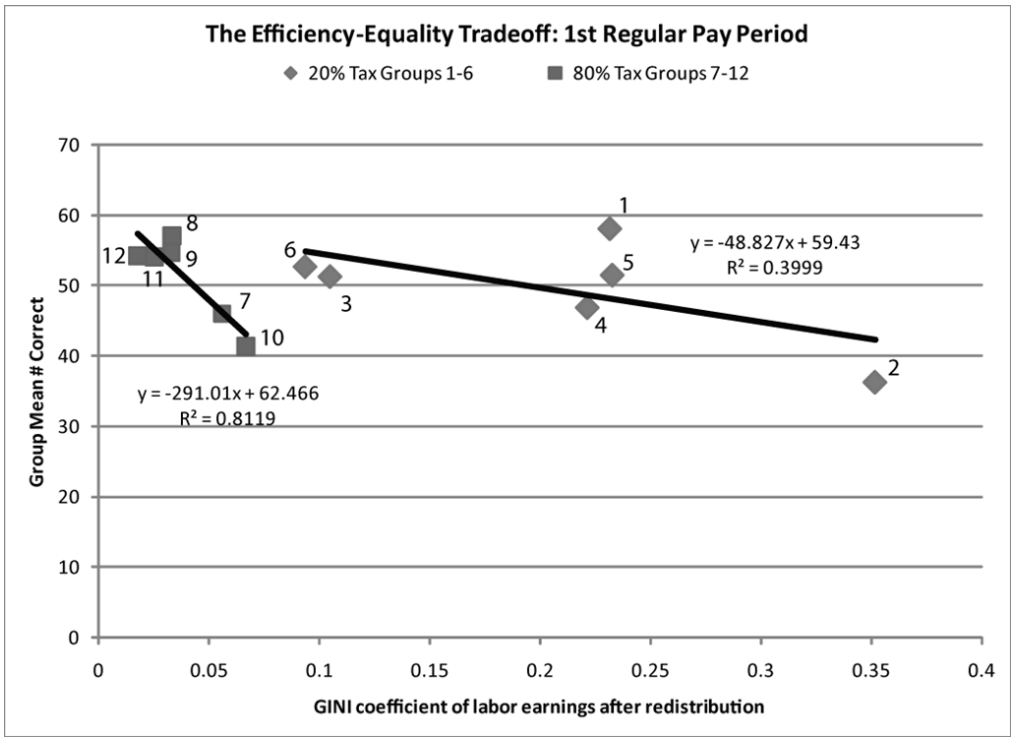

Figure 9. Tradeoff in the 1st, 6th, and 12th Regular Pay Periods

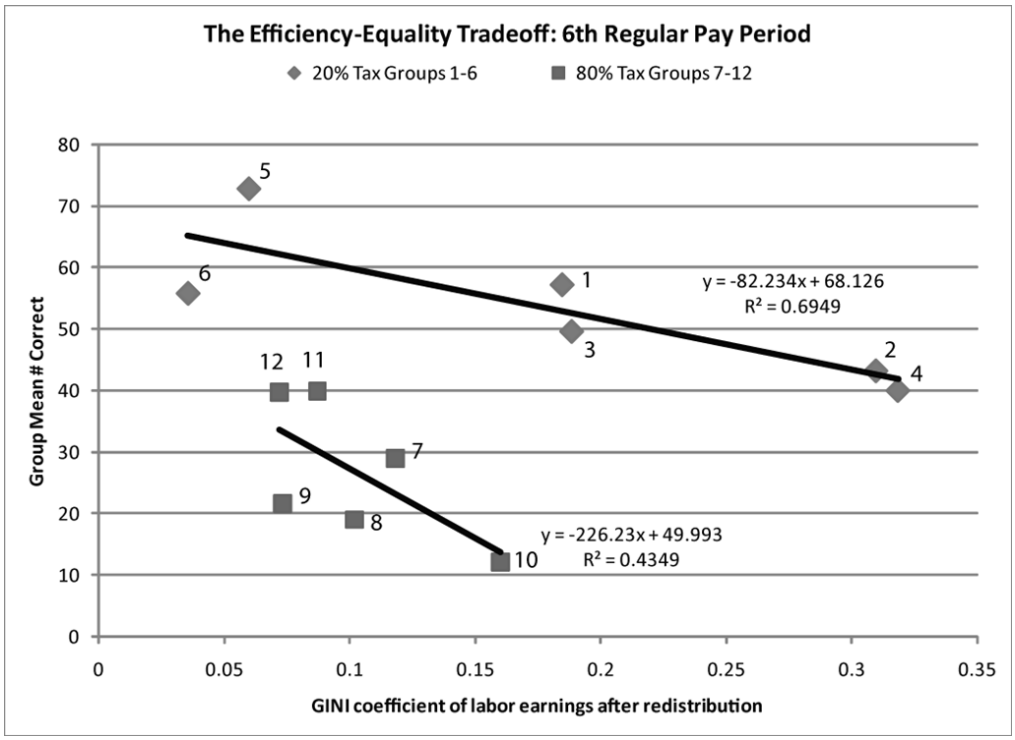

Figure 9 (Continued). Tradeoff in the 1st, 6th, and 12th Regular Pay Periods

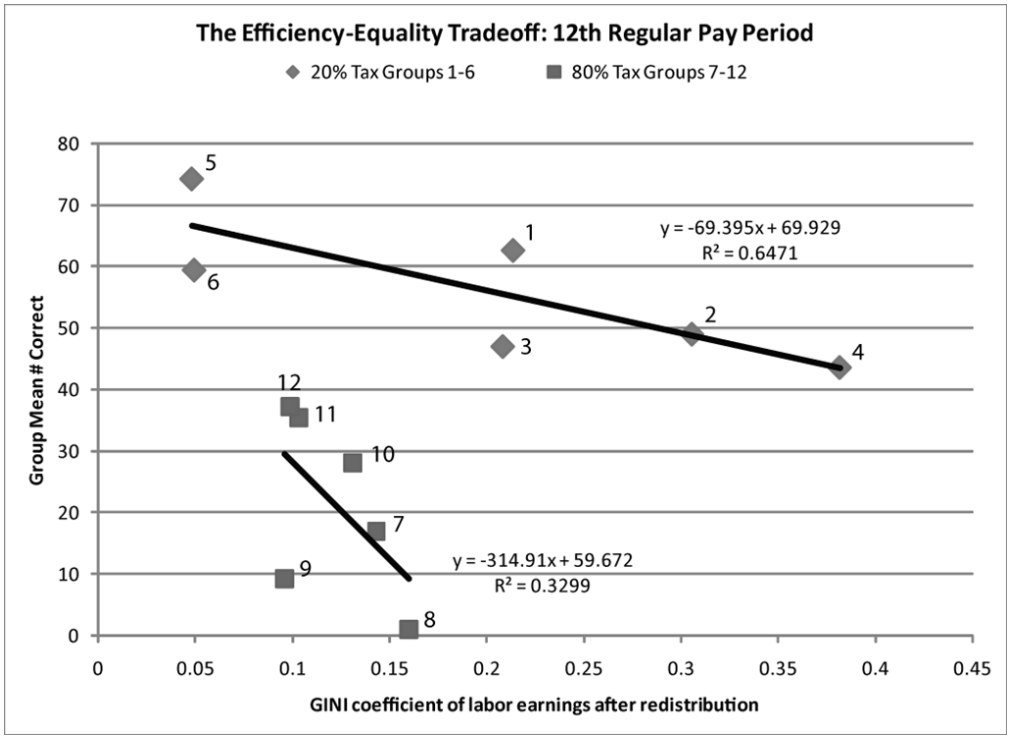

Figure 9 (Continued). Tradeoff in the 1st, 6th, and 12th Regular Pay Periods

Can the tradeoff found be explained by a reduction in work incentives? Although a subjects' exact effort level cannot be measured, changes in effort can be inferred from: (1) changes in time allocation between labor and leisure; and (2) changes in work intensity while performing labor. Changes in time allocation are the primary measure of the effect of income redistribution on work incentives. Figure 10 shows the mean number of seconds per period spent on leisure. The figure clearly shows that the reduction in productivity is coming primarily from subjects spending more of their time in leisure under an 80% tax. Comparing Figure 10 with Figure 4, which shows the changes in productivity over periods, one can see that changes in average productivity are almost a horizontal reflection of changes in average leisure time. It is because of this strong correlation that graphs showing the breakdown of leisure seconds for each group and

each session are not included. Figure 11 shows histograms of the leisure seconds taken by subjects in the 1st, 6th, and 12th Regular Pay Periods of each tax. The histograms confirm how strongly time allocation is driving the majority of changes in productivity as the vast majority of decisions made involve subjects working the entire time or not at all for both tax treatments. The mean number of leisure seconds is 81.46 (31.63 standard deviation across group means) under the 20% tax and 154.27 (37.36 standard deviation across group means) under the 80% tax. Using a t-test with unequal variances to compare the mean of the six group means of leisure seconds taken over all Regular Pay Periods for each tax level, the null hypothesis that the mean leisure seconds for each treatment are the same is rejected at the 1% level (p-value = 0.0035) with leisure time being significantly less for 20% tax groups (p-value = 0.0018).

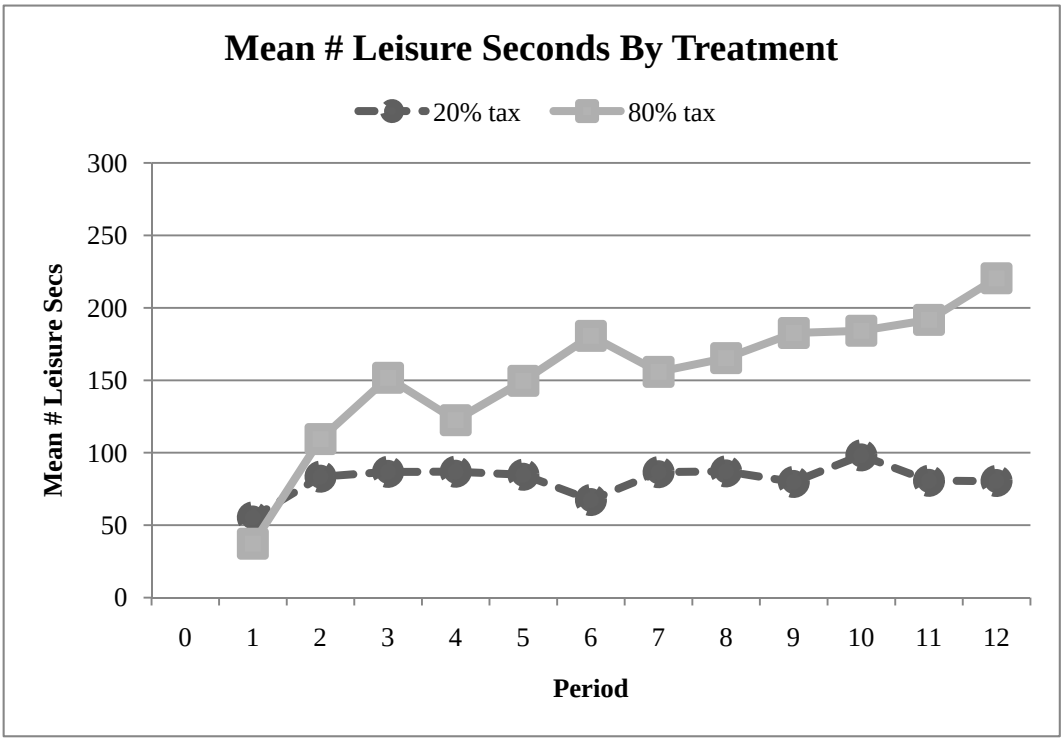

Figure 10. Leisure Seconds Taken by Treatment

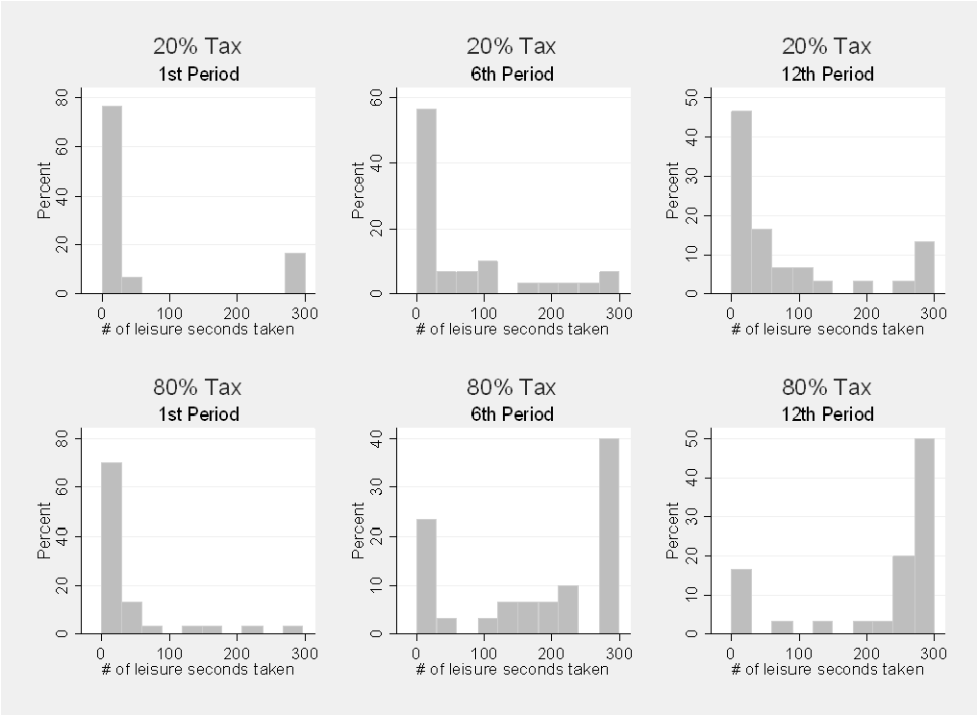

Figure 11. Leisure Seconds Taken by Subjects (Bar Width = 30 Seconds)

What happens across treatments when subjects are working? Does work intensity also vary? Work intensity is defined as the number of sequences correctly typed per minute of labor time. The mean number of correct sequences per minute of labor is 13.32 under the 20% tax and 12.43 under the 80% tax. Figure 12 shows that work intensity is the same under the 20% and 80% tax treatments across periods. Subjects also become more productive at performing the labor task over time in both treatments. Why is there no difference? One explanation is time is money in this experiment: it is rational for a subject to work at their maximum ability even if he or she intends to spend part of the period on leisure. The correlation between work intensity and leisure seconds overall is -0.455, but the correlation is weak (-0.092) if you only include observations with leisure seconds less than 270 and strong (-0.856) if you only include observations with leisure

seconds greater than or equal to 270. Perhaps subjects spending 30 seconds or less on the labor task do not have enough time to get into the groove of typing fast, or are typing slowly because they are already considering switching to leisure. Although insignificant, intensity is slightly less on average in the 80% tax, which could be due to the higher number of subjects spending almost all of their time on leisure.

How can overall work effort be measured? It is a function of labor time supplied and work intensity. The number of sequences typed correctly serves as their measure of productivity, but the differences in ability must be controlled for. Ability is exogenous in the Double Pay Period, but endogenous thereafter because ability can increase through learning by doing. The effort proxy variable is generated by taking the number of sequences typed correctly in the current period and dividing it by the maximum number of sequences typed thus far in any period. The Double Pay Period is included in case a subject puts forth little effort in the Regular Pay Periods. If an individual scores a 0, then they are not working at all. If an individual's score equals 1, then they have reached their maximum productivity threshold established in this period or in a previous period. This effort proxy only measures a decrease in effort relative to a revealed maximum productivity level, for increases in effort are confounded with increases in ability. During the Regular Pay Periods, the average work effort measure for any given individual in any given period was 0.75 for the 20% tax treatment and 0.49 for the 80% tax treatment. Figure 13 shows histograms of the work effort variable for the 1st, 6th, and 12th Regular Pay Periods of each tax rate. The majority of subjects in the 20% tax treatment continue to establish new or come close to their maximum over the periods. The majority of subjects in the 80% tax treatment decrease effort in later periods.

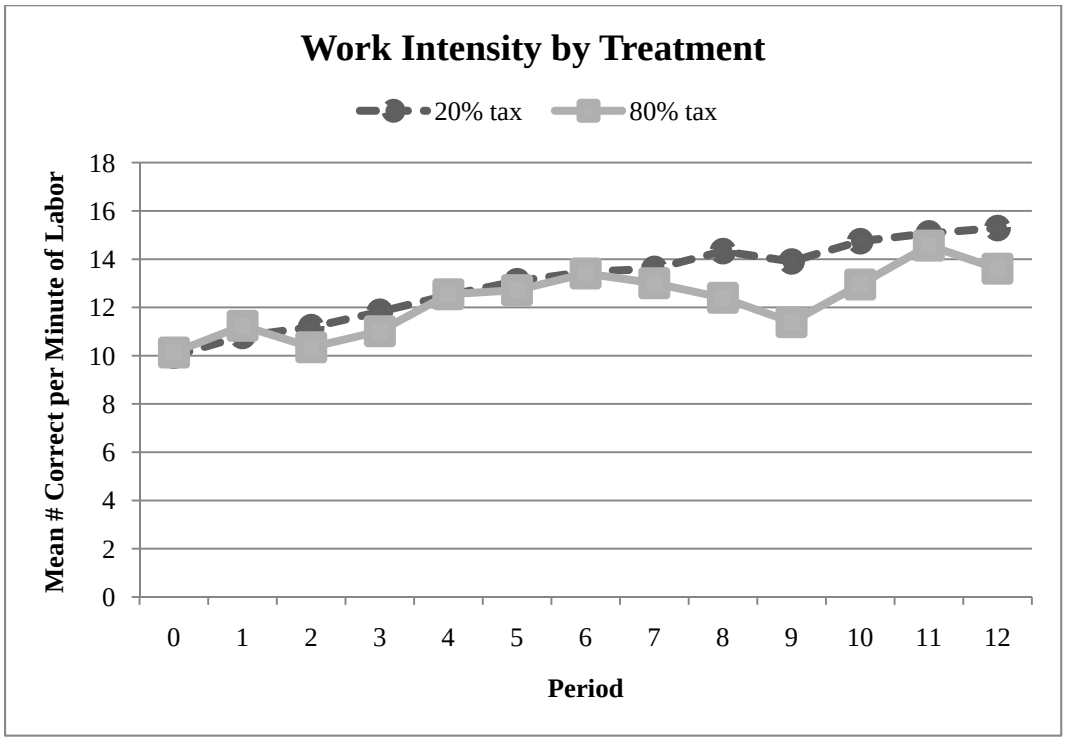

Figure 12. Work Intensity by Treatment

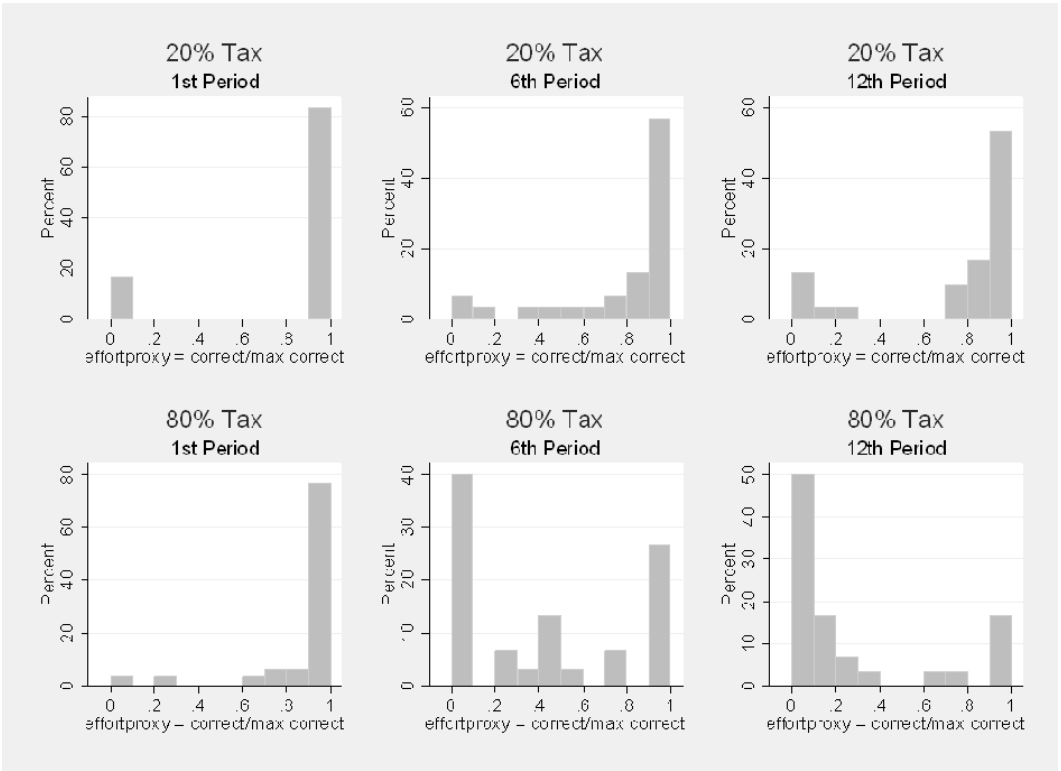

Figure 13. Effort Proxy by Subjects (Bar Width = 0.1)

At the end of each period subjects are told their hypothetical earnings if they had spent the entire last period on leisure. Some could argue that this information is too salient and encourages subjects to perform leisure if it pays more than labor. Maybe so, but this salient information was given to subjects so the following question can be asked: "Do subjects follow the myopic best response, or do they have strategic and/or other regarding preferences?" A choice is consistent with the myopic best response if they see that they would have been better off (worse off) switching to leisure in one period and, subsequently, they spend the better half of the next period in leisure (labor). Over all decisions made after the first Regular Pay Period, 430 out of 660 (65.15%) are consistent with myopic best response. However, 230 out of 660 (34.85%) are not, including 80 out of 330 choices from the 20% tax treatment (24.24%) and 150 out of 330 choices from the 80% tax treatment (45.45%). Of the decisions inconsistent with myopic best response, 221 out of 230 (96.09%) cases occurred where the subject learned that he or she could have earned more by performing only leisure in the previous period yet chose to spend the majority of the following period on labor instead of leisure.

This 34.85% of decisions inconsistent with myopic best response need to be explained. Subjects receive feedback at the end of each period. They are told whether their transfers to the Group Account are above or below the group average. They are told their opportunity cost of labor in terms of their hypothetical "earnings if entire period was spent on leisure." Finally subjects are told that if everyone takes leisure then the Group Account will be zero for the period and everyone will only make $1.50. Subjects are aware of the non-excludability of income collected for redistribution and are told that the

size of the Group Account is conditional on the productivity of the other group members as well as their own.

How does this feedback affect individual time allocation between labor and leisure? Figure 3 shows the feedback given to subjects at the end of each regular pay period. Here they can compare their transfers to the group account (or tax payments) with their share of the group account to see if they contributed more than the group average (themselves included) to the group account. They can also compare their total earnings with their hypothetical "earnings if entire period was spent taking leisure" to see if they would have been better off not working at all that period. Both of these comparisons are highly dependent on how many sequences the other four members' type and the amount of labor income they transfer into the group account. As seen from Figure 11, the vast majority of labor-leisure time allocations involved subjects spending the entire period on labor or on leisure. This calls for a tobit random-effects estimation of the panel data which sets limits for the dependent variable, the number of leisure seconds taken in a period, from the left at 0 leisure seconds and from the right at 300 leisure seconds.

Table 3 shows results of three separate two-limit tobit estimations of the panel data. The first tobit regression includes both 20% tax and 80% tax data for the Regular Pay Periods. The tax dummy variable, equal to 0 for 20% tax data and 1 for 80% tax data, came up highly significant and increased the leisure seconds by a large amount. The remaining explanatory variables are lagged because they are the feedback from the previous period which subjects respond to. The first variable, the leisure seconds (lagged one period), is included to control for the possibility that subjects who spend the majority of their time on leisure are more likely to do so in later periods. This variable is

significant in the tobit regression of all data and the tobit regression of the 80% tax data. The second variable, the others' mean transfer to the Group Account (lagged one period and in cents), is calculated as the Group Account minus the subject's transfer to the Group Account with the difference divided by 4. This variable was included to see if individuals responded to how much the other four members contributed to the Group Account, which they could determine indirectly by comparing the feedback information given to them at the end of each period. The third variable is the second variable squared, which is included to capture a possible quadratic relationship of the second variable to leisure seconds in the current period. Overall, other group members' transfers to the Group Account were found to significantly decrease the number of leisure seconds taken at a significantly decreasing rate. The marginal effect of this variable calculated at the mean of the variable is -2.484+2*(0.0121)*(55.03) = -1.152. This variable does not come up significant when only the 20% tax data is used, but it is significant when only the 80% tax data is used (the marginal effect is -3.204+2*(0.0166)*(79.93) = -0.55).

Table 3. Tobit Regressions of Leisure Seconds Taken

Dep. Var. = Leisure Seconds	20% & 80% Tax Data	20% Tax Data Only	80% Tax Data Only
n-obs	660	330	330
Constant	97.536	33.022	259.94
	(0.006)	(0.710)	(0.000)
Tax Dummy Variable (1 = 80% tax, 0 = 20% tax)	147.896 (0.001)***	…	…
Leisure Seconds (lagged)	0.226 (0.001)***	0.109 (0.192)	0.318 (0.003)**
Others' Mean Transfer to GA (lagged and in cents)	-2.48 (0.002)**	1.959 (0.733)	-3.204 (0.006)**
[Others' Mean Transfer]^2 (lagged and in cents)	.0121 (0.017)*	-0.0414 (0.667)	0.0166 (0.021)*

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

An alternative dependent variable may be overall work effort as defined previously and shown in the histograms of Figure 13. How does this information and feedback affect individuals' overall work effort? Table 4 shows a tobit regression censored from the left at 0 and at the right at 1. Using all of the data there were 157 left-censored observations, 351 uncensored observations, and 152 right-censored observations. The tax dummy shows a significant effect on reducing effort. The lagged leisure seconds variable has a negative and significant effect on the effort proxy using all of the data and using only 80% tax data. Other group members' transfers to the Group Account were found to significantly increase effort but at a significantly decreasing rate. The marginal effect of this variable calculated at the mean of the variable including all data is 0.0079+2*(-0.00004)*(55.03) = 0.0035. The marginal effect is 0.0111+2*(-0.00006)*(79.93) = 0.0015 using all data.

Table 4. Tobit Regressions of Effort Proxy Variable

Dep. Var. = Effort Proxy	20% and 80% Tax Data	20% Tax Data Only	80% Tax Data Only
n-obs	660	330	330
Constant	0.648	0.679	0.047
	(0.000)	(0.025)	(0.815)
Tax Dummy Variable	-0.519	…	…
(1 = 80% tax, 0 = 20% tax)	(0.000)***		
Leisure Seconds	-0.0007	-0.0002	-0.0009
(lagged)	(0.009)**	(0.605)	(0.014)*
Others' Mean Transfer To GA	0.008	0.0083	0.0110
(lagged and in cents)	(0.007)**	(0.675)	(0.008)**
[Others' Mean Transfer To GA]^2	-0.00004	-0.0001	-0.00005
(lagged and in cents)	(0.029)*	(0.661)	(0.021)*

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

In both measurements of labor supply there is some evidence that individuals are responding to the contributions of other members to the Group Account, where the stronger responses are found in the 80% tax treatment. It is also shown that efficiency in the 80% tax groups decay over periods in a manner similar to the decay of contributions in voluntary public goods experiments. Recall that at a tax level of 100%, performing labor tasks becomes a 100% contribution to the public good (the Group Account). If the tax rate is 0%, or if there is no Group Account, then there is no public good and subjects will simply select the activity which pays them more. This means that the 80% tax treatment is more comparable to the standard voluntary public goods provision experiment.

Survey Analysis

After performing all Regular Pay Periods subjects were asked to fill out a questionnaire. Subjects answered 30 questions which are listed in Appendix B. Questions 1 through 7 asked subjects their sex, year in college, major, mother's education, father's education, marital status, and household income. Responses for these questions were coded from 0 to N for subjects choosing the 1^{st} to $(N+1)^{th}$ answer. Question 3, the students' major, remains uncoded. Questions 8 and 9 asked subjects how often they lent money or other items to friends. Questions 10 through 15 asked subjects how much they generally trust others to be fair and helpful. Questions 16 through 22 asked subjects how satisfied they were with various aspects of their life. Questions 23 through 26 elicited the subject's fairness attitudes and perceptions. Questions 27 through 30 had subjects vote on distributions.

Are any of the survey responses correlated with labor supply? The labor supply of individuals is highly driven by the tax rate and the labor supply of other group members, so individual correlations are expected to be weak. However, the average survey responses of each group might be correlated with the average labor supply of the group. Table 5 shows the average individual leisure seconds taken by each group across the 12 Regular Pay Periods and the average group survey response to each survey question.

Table 6 displays the correlations of the average group responses to the average group leisure seconds taken. Consider the response to Question 10: *Do you think most people would try to take advantage of you if they got the chance, or would they try to be fair?* If on average the group believes that "most people would take advantage," then they could be less likely to coordinate and cooperate on labor supply resulting in a higher

average leisure seconds taken by the group across all Regular Pay Periods. Table 6 shows a relatively high negative correlation between this question and leisure seconds taken with a coefficient of -0.79 for the 20% tax treatment groups, -0.40 for the 80% tax groups, and -0.59 for all groups combined. This claim would be more plausible if subjects took the survey before the Regular Pay Periods because there is a possible order effect. Members who just finished an hour in a group that failed to coordinate and cooperate in labor supply are more likely to believe that "most people would take advantage" afterwards.

Another interesting finding is that questions 9, 12, and 18 had high correlations in both treatments but with an opposite sign in each treatment. Question 9 asks: *How often do you lend personal possessions to friends?* Higher group lending has a strong and positive correlation with leisure seconds taken in the 20% tax treatment but a strong and negative correlation in the 80% tax treatment. Question 12 asks: *Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?* More group trust is positively correlated with group leisure seconds in the 20% tax treatment and negatively correlated in the 80% tax treatment. Question 18 asks: *How satisfied are you with your social life?* This question is important because subjects more satisfied with their social life may feel less of a need to please others outside their well-rounded social circle for self affirmation. More satisfied groups have less leisure seconds in the 20% tax groups and more leisure seconds in the 80% tax groups. How can these opposite signs be explained? High levels of trust and cooperation are a good for average group labor supply in the high tax treatment because cooperation levels have to be kept high to keep labor a profitable activity. This explanation seems

intuitive for the high tax treatment, so why is the result the opposite for the low tax treatment? One answer is that there were still free-riders who spent most of the Regular Pay Periods on the leisure tab. Free-riding is more tempting when the other group members are highly cooperative in both tax treatments in the short run, but it may only remain profitable in the long run for the low tax treatment. This is because one free-rider is a lot more damaging to others' earnings in the high tax treatment, so cooperators are much more likely to switch to leisure in response to a free-rider in the high tax treatment. In short, the opposite effect in the 20% tax treatment can be attributed to the fact that a more cooperative group is also more tempting to a free-rider and cooperative groups are also more tolerant of free-riding in the low tax treatment were free riding is less hurtful to cooperators.

Table 5. Group Mean Leisure Seconds Taken and Group Mean Survey Responses

	20% Tax Rate Groups						80% Tax Rate Groups					
group number	5	3	1	6	2	4	12	11	7	9	10	8
leisure seconds	50.7	60.0	64.1	71.3	118.5	124.1	106.9	111.6	152.1	178.4	185.7	190.9
q1	1	0.6	0.6	1	0.4	0.2	0.6	0.4	0.4	0.8	0.2	0.6
q2	1.2	2.2	1.6	1.8	2.2	1.8	1.4	2.2	6.4	1.8	2	2.6
q4	2	3	2.8	1.6	2.4	1.8	2.4	2.2	2.2	1.6	1.8	2.4
q5	2.8	2.4	3.2	2	2	1.8	2.4	2.6	1.6	2	2.4	1.8
q6	0.4	0.2	0	0.4	0.2	0.4	0	0.2	0	0	0.2	0
q7	1	1.8	2	2	2.8	2.8	1.8	1.4	2.8	1.8	3.4	1.8
q8	0.6	3	1	1.2	1.2	1	2.6	1.2	0.4	0.8	0.4	0.8
q9	0.8	2.8	1	0.8	4.6	1.6	3.8	2.2	1.2	2.4	1.2	1.8
q10	4.8	5.2	5	5.8	3.6	4	4.6	4.2	5.2	3	1.2	5
q11	3.6	5.8	4.4	3.4	3.6	4.2	4.8	5.4	4	5.6	2.4	4.4
q12	1.25	4.2	3.6	3.6	5.6	4.4	4.4	4.4	3	3	2.2	3
q13	5.75	7.8	6.8	6	7.2	5.2	6.6	7.2	6	7.8	4	5.2
q14	2.75	3.4	2.6	1.8	3.2	2.2	6.2	4	2.2	2.8	0.6	5.2
q15	5	6.4	3.4	4.8	6	5	5	5.2	6.4	5.8	4.2	5.4
q16	5.8	8.6	5.6	6.4	6.8	6.6	4.2	7.2	6.4	7	7.8	7.2
q17	5.6	5.4	6.2	4.6	7.8	6.4	5	6.4	7.2	4.8	7.4	8.4
q18	7	7.8	7.8	6.2	6	5.6	4.6	6.8	5.4	7	7.6	7.4
q19	5	7.2	4.6	5.6	5.6	4.6	3.4	6.6	5.8	3.2	9.2	5.2
q20	4.4	7.8	6.2	7	7.4	7	4.6	6.6	7	7	8	8.2
q21	2.4	4.4	2.4	3.8	3.4	4.2	2.8	2	3	1.8	4.6	2.6
q22	3	5.6	6.75	7.6	5.2	5.6	3.4	6.4	4.8	3.8	5.2	5.4
q23	3.2	2	3.8	5.6	2	2.8	2.6	4.4	2.8	3.2	5.2	4.6
q24	4	1.4	0.8	4.2	0.8	2.6	1.8	2.75	0.2	1.8	2.4	1.6
q25	0.8	0.6	1.2	1.4	0.2	2.4	2	2.8	2.2	5.2	0.6	1.6
q26	5.4	2	2.4	4.8	4.4	3.2	4.6	4	3.8	1.2	5.2	3.4
q27	1.4	0.8	1.4	1.6	1.4	1	2	1.4	0.2	1.8	1	1.6
q28	1.4	0.6	1.2	1.6	1.6	1	2	1.4	0.6	1.8	0.8	1.6
q29	2	1.8	2	2	1.8	1.4	2	1.6	1.8	1.8	2	2
q30	2	1.8	2	2	1.8	1.6	2	2	2	1.8	1.8	2

Table 6. Correlations of Mean Survey Responses to Group Mean Leisure Seconds

Question	20% Tax Groups	80% Tax Groups	All Groups
q1	-0.81*	0.03	-0.46
q2	0.47	0.08	0.35
q4	-0.28	-0.48	-0.37
q5	-0.73	-0.52	-0.59**
q6	0.15	-0.12	-0.46
q7	0.92***	0.46	0.49
q8	-0.19	-0.74*	-0.46
q9	0.54	-0.63	0.07
q10	-0.79*	-0.40	-0.59**
q11	-0.24	-0.44	-0.13
q12	0.74*	-0.92**	-0.12
q13	-0.22	-0.50	-0.37
q14	-0.09	-0.50	-0.02
q15	0.22	-0.02	0.18
q16	0.05	0.66	0.27
q17	0.70	0.49	0.54*
q18	-0.80*	0.71	-0.03
q19	-0.27	0.24	0.10
q20	0.48	0.86**	0.53*
q21	0.41	0.32	-0.04
q22	0.10	0.04	-0.19
q23	-0.33	0.46	0.22
q24	-0.28	-0.16	-0.31
q25	0.31	-0.06	0.41
q26	0.00	-0.31	-0.11
q27	-0.13	-0.18	-0.05
q28	0.15	-0.27	0.04
q29	-0.78*	0.42	-0.08
q30	-0.78*	-0.58	-0.22

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Conclusion

The results demonstrate the potential for a significant efficiency-equality tradeoff in government-mandated income redistribution. The tradeoff can be explained by lower work incentives in the high tax treatment. If the primary goal of the public sector is to reduce income inequality they will be successful: income inequality is lower under the high tax treatment. However, this comes with a significant reduction in efficiency: output in the high tax treatment is 36.3% lower than the low tax treatment on average. There will be less income available for charity if the efficiency loss from redistribution becomes sufficiently large.

Significant reductions in labor time and overall work effort supplied occurred in the high tax. This supports the hypothesis that lowered work incentives are an unavoidable consequence of income redistribution based upon wage taxation. Most of the efficiency loss comes from the lower labor time supplied in the high tax treatment. Subjects in the high tax treatment are also no less productive when they are working, as long as they spend more than 10% (30 seconds on the leisure tab) of the period performing the labor task. Constructing a work effort proxy, we find that on average subjects work at half of their revealed output capacity in the high tax treatment. If the public sector implements an intense redistribution program we might expect fewer charitable goods and services if people significantly reduce their labor supply.

We find some evidence of strategic and social preferences. 34.85% of subjects' decisions cannot be explained as myopic best responses to the relative returns to labor and leisure. Since tax revenue was redistributed as a public good, a comparison of this experiment to other public goods experiments was included. The level of cooperation

within a group can have a profound effect on group efficiency. Here labor participation is the signal of cooperation. One surprising result was that the two most cooperative and trusting low tax groups had greater efficiency and more equality than all of the high tax groups. This peculiar result suggests that, if a group is very cooperative, redistribution can do more harm than good regardless of whether your objective is efficiency, equality, or providing charitable goods and services.

Results from this experiment are based solely upon the size and distribution of labor income. Outside the laboratory, there are of course other measures that go into evaluating the impact of redistribution. For example, while crowd out of charitable public goods is often incomplete there may be quality effects if government feedback mechanisms are weaker than in the independent sector. On the other hand, some positive unintended consequences may occur. In this experiment, labor activity yielded both a public and private return while leisure only provided a private return. Outside this experiment, leisure may provide both a private and public return. People who already spend some of their leisure time volunteering will volunteer more as they reduce their labor supply. In fact, volunteerism may increase more than proportionately with leisure time taken if volunteers grow dissatisfied with the quality of public charity provision and decide to offer competing alternatives. Future research may want to take account of the quality dimension of charitable goods as well as the unintended consequences of tax policy on the volunteer labor supply.

CHAPTER III

USING ROLE-REVERSAL IN LABORATORY EXPERIMENTS

Introduction

In determining the right decision to make, we will often consider how each decision affects our outcomes, how it affects others' outcomes, and how others might react to each decision. This forecasting exercise is called role thinking: we mentally assume the role of our counterpart and react to each of our possible sets of decisions (Green and Armstrong 2009). Role-thinking helps people play competitive, coordination, and cooperative games better. Role-thinking in cooperative games helps us determine if our planned action is fair or generous towards our counterpart. Cooperative games include trust games, bargaining games, and dictator games. A popular cooperative role-thinking convention is to follow the Golden Rule: do unto others as you would have them do unto you. The purpose of this study is to investigate whether adding saliency to role-thinking, by applying a role-reversal protocol, affects behavior in cooperative games.

Role thinking is a mental exercise made under a level of saliency that depends on how often an individual plays the counterpart's role. Role thinking under low levels of saliency may be more vulnerable to personal biases. One type of bias is called the *consensus effect*. The consensus effect states that people use information about themselves to predict other people's decisions (Alicke and Largo 1995; Engelmann and Strobel 2000; Marks and Miller 1987; Mullen et al. 1985). Dawes (1989) states that thinking that another will behave like oneself in the same situation is rational as long as the individual recognizes that they are making predictions based on drawing a random sample of size one (Brosig, Weimann, and Yang 2003; Dawes 1989). Another type of

bias is called the *positive self-image effect.* The positive self-image effect states that people are psychologically driven to view themselves in a favorable manner (Singh, Choo, and Poh 1998; Wicklund and Gollwitzer 1981). This means that under a single-role protocol subjects can engage in role-thinking yet maintain a false belief that they would be more generous playing the counterpart's role.

A role-reversal protocol can possibly reduce personal biases such as the consensus effect and positive self-image effect. The role-reversal protocol requires subjects to fully explore a game by playing all roles in the game. Brosig et al. (2003) describes how a role-reversal protocol can reduce biases:

> If subjects not only think about what they would do if they were in the role of their opponent, but are in addition aware that they will subsequently be given that opportunity, they are likely to undertake greater self-reflection right from the beginning and can hardly maintain unrealistic and positive beliefs about themselves without producing cognitive dissonance. As a result, each subject's expectations about their counterpart's decision and their own decision in the counterpart's role are much more likely to be in line with each other (Brosig, Weimann, and Yang 2003).

This suggests that the behavioral effect of a role-reversal protocol may be due to the mitigation of potential bias of role-thinking in the single-role protocol. However, subjects who may not engage in role-thinking in a single-role protocol would be forced to do so under the role-reversal protocol, and a role-reversal protocol may affect their behavior differently. Role-reversal requires more salient role thinking, for an individual must actually put himself or herself in the shoes of his or her counterpart by facing the same environment, opportunities, decision tasks, and outcomes accompanying the counterpart's role.

Background and Literature

Literature on the effect of a role-reversal protocol in cooperative games can be separated into two groups: studies that use the role-reversal protocol to learn more about behavior in the single-role protocol, and studies that test for the effect of using a role-reversal protocol. Results from the second group of studies have implications for the first. If behavior under the single-role protocol is not robust to role-reversal, then the role-reversal protocol cannot be effectively used to learn more about behavior in the single-role protocol.

Some studies are not directly interested in the effect of role-reversal. When decisions vary in a particular game and there are two or more different roles, experimenters may want to compare role decisions within individuals rather than across. One study applied role-reversal to a trust game to look for behavioral types between sexes and found that men with low trust in the first mover role are less trustworthy in the second mover role while women with low trust are no less trustworthy than women with high trust (Vyrastekova and Onderstal 2005). Another study used a role-reversal protocol of the Ultimatum Game and found that economists keep more and accept less (Carter and Irons 1991). Another study of a trust game using the role-reversal protocol found that high proportions returned implies high amounts sent but the converse did not hold (Chaudhuri and Gangadharan 2007). Other studies wanted to see if subjects' choices as first and second mover were consistent in the Ultimatum Game. Subjects passed their consistency test if the amount they proposed to leave the second mover was no less than the minimal amount they would accept themselves (Andreoni, Castillo, and Petrie 2003; Guth, Schmittberger, and Schwarze 1982).

Other researchers are directly interested in the testing for a role-reversal effect. They will compare role-reversal protocols to single-role protocols across studies, or they will conduct a role-reversal and single-role protocols to compare behavior within a study. Looking for role-reversal effects across studies we get mixed evidence. Comparing an extensive form trust game with a role-reversal protocol to its single-role protocol, subjects who play both roles displayed more trust and trustworthiness (Güth, Ockenfels, and Wendel 1997; Snijders and Keren 1994). Vyrastekova and Onderstal (2005) compare their trust game with role-reversal to results from the Investment Game (Berg, Dickhaut, and McCabe 1995), a single-role trust game, and find no significant role-reversal effect. Role-reversal may lead to higher or lower proposer demands and more or less rejections in the Ultimatum Game depending on which studies are paired for comparison (Burks, Carpenter, and Verhoogen 2003; Carter and Irons 1991; Forsythe et al. 1994; Güth and Tietz 1990). Behavior in a role-reversal protocol of a two-stage bargaining game was found to be more in line with the equilibrium prediction under the single-role protocol (Binmore, Shaked, and Sutton 1985; Güth, Ockenfels, and Wendel 1997).

Looking at role-reversal within studies we also get mixed evidence. Charness and Rabin conducted a series of two-person, one-shot dictator and response games where they applied role-reversal (Charness and Rabin 2002).[6] Later, they ran the same games without role-reversal and found the results were not statistically different (Charness and Rabin 2005). However, another study of dictator and response games found that those

[6] The response games are a collection two-person and three-person games designed to measure social preferences. Decisions were elicited from a first mover (Player A) and a responder (Player B) using the strategy method protocol. In these games Player A decides to get out of the game or enter, and entering gives Player B a more or less generous opportunity set and Player B rewards or punishes Player A for doing so. The response games also varied on whether or not there was a cost for Player B to punish or reward (Charness and Rabin 2002).

participating in the role-reversal treatment made selfish choices less frequently and engaged in costly surplus creating actions more frequently (Iriberri and Ray-Biel 2009). Weg and Smith (1993) conduct several two-period two-person bargaining games similar to the Ultimatum Game. In one game the player proposes twice and the other player responds twice. In the other game the two players alternate the proposer and responder roles. More is offered by the first proposer in the second game because he/she has less bargaining power. They also conducted a single-role protocol of the Ultimatum Game, where proposer offers are in between offers made in the other two treatments (Weg and Smith 1993). Another study which conducted both the standard and role-reversal protocols of the Ultimatum Game have some evidence of role-reversal leading to more generous proposals and a higher number of equal splits (Guth, Schmittberger, and Schwarze 1982).[7] One study that replicated the Investment Game single-role protocol and conducted a role-reversal protocol found that significantly less was sent by the first mover and less was returned by the second mover (Burks, Carpenter, and Verhoogen 2003).

Why is evidence on the behavioral effect of a role-reversal protocol so mixed? One explanation is that not enough studies have been conducted to discover any consistent trends that may or may not exist. Effects found comparing across studies can be discounted: studies are conducted by different experimenters, using different subject pools, using different protocols and game designs, and are conducted at different times. Within studies there is no consensus on a single role-reversal protocol. Since the evidence

[7] This is a weak test of role-reversal effects because second movers react in their single-role protocol use the strategy method protocol in their role-reversal protocol. Also, the amount to be bargained varies around $7 in the single-role protocol whereas it is constant at $7 in their role-reversal protocol.

is spread amongst multiple role-reversal protocols and multiple cooperative games it is not too surprising that the evidence is mixed.

This study compares role-reversal and single-role protocols of the Investment Game (Berg, Dickhaut, and McCabe 1995). The key design features of the Investment Game are:

1. Subjects are split in half into two rooms: A and B.
2. Both Room A subjects and Room B subjects are given a $10 endowment.
3. The Room B subject is told to pocket the $10, while the Room A subject must decide whether to send none, some, or all of his $10 (in $1 increments) to the subject he is paired with in Room B and keep the remaining amount.
4. The experimenters triple the amount sent before it reaches the paired Room B subject.
5. The Room B subject must then decide whether she wants to return none, some, or all of her tripled amount (in $1 increments) to the paired subject in Room A. The amount returned is not multiplied and is returned to the paired Room A subject.
6. The game is played only once.
7. Complete privacy is ensured through a double-blind protocol.

In the Investment Game, henceforth called IG, the Room A subject is the first mover, or Sender, and the Room B subject is the second mover, or Receiver. Assuming that players are rational and care only about maximizing their own payoffs, the Nash-equilibrium prediction of the IG is: the Receiver will choose to keep the entire amount received and the Sender sends nothing in expectation of this. The game stops

immediately and both Sender and Receiver leave with their starting endowment of $10. The results of Berg et al. (1995) ran contrary to the Nash equilibrium prediction: an average of $5.16 was sent and an average of $4.66 was returned (Berg, Dickhaut, and McCabe 1995). This result has been replicated in other trust and Investment Game studies as well (Cox 2004; Cox and Deck 2005; Glaeser et al. 2000). Amounts sent can be motivated by the Sender's trust in the Receiver and/or altruistic preferences towards the Receiver. Amounts returned can be motivated by the Receiver's desire to reward the Sender's generosity, be trustworthy, and/or altruistic preferences towards the Sender.[8]

This will not be an exhaustive study of the role-reversal effect since only the Investment Game is tested. This is a good place to start since there is evidence that senders and receivers have different perspectives due to their asymmetric roles (Malhotra 2004).[9] The following null hypothesis is tested: Decisions made in the Investment Game under our role-reversal protocol will not be statistically different than decisions made under the single-role protocol.

The behavioral results of the Investment Game under the single-role protocol are robust to the role-reversal protocol of this study. The send and return decisions made under the role-reversal protocol are not significantly different than decisions made under the single role protocol. The lack of a significant role-reversal effect can likely be attributed the decision to use the sequential move protocol instead of a strategy method protocol and to select role for payoff instead of multiple roles in the role-reversal

[8] Cox (2004) showed that positive amounts sent by the Sender can be separated into trust and unconditional altruism and that the positive amounts returned by the Receiver can be separated into trustworthiness (also known as positive reciprocity) and unconditional altruism (Cox 2004).

[9] Malthora (2004) showed that Senders are more concerned with the risk associated with trusting while Receivers are more concerned with the benefit of the Sender's trusting action. Neither Sender nor Receiver were found to be sensitive to the other's perspective (Malhotra 2004).

protocol. The rest of the paper is organized as follows: Section 2 discusses the methodology of choosing a role-reversal protocol and predictions on how it might affect behavior. Section 3 discusses the experimental design and procedures. Section 4 discusses the results and Section 5 concludes.

How Should a Role-Reversal Protocol be Designed?

The minimal requirements for a role-reversal protocol are common knowledge that each subject will play each possible role in the game, and that each role decision has the potential to affect their earnings (saliency). From there all bets are off. However, if one wants to test for a fundamental role-reversal effect a good place to start is by minimizing the number of departures from the single-role protocol. One unavoidable departure is that any role-reversal protocol requires repetitions of the game: subjects will have more decisions to make if the single-role protocol is not repeated. This alone may cause a role-reversal effect, but further decisions must be made in designing a role-reversal protocol which may also have an effect.

The first decision is on whether to pay only one or multiple roles played. Selecting only one role for payoff after decisions are made carries the benefit of more data per dollar. Subjects, by playing both roles, experience the higher mental cost from making multiple decisions. Selecting one decision for payoff reduces the saliency of payoff outcomes for each decision. Furthermore, the subject's risk preferences must be consistent with the independence axiom of expected utility theory for the subject to treat the role decisions independently. Otherwise, selecting one role for payoff can lead to different behavior (Cox 2010). Thus, we do not know whether changes in behavior are

due to role-reversal effects, violations of the independence axiom, or a combination thereof.

Paying for more than one of the roles a subject plays increases payoff saliency, but it also creates problems of wealth effects, feedback effects, and portfolio effects, which may confound any fundamental effect of role-reversal present. Wealth effects occur when subjects are paid for more than one role and the potential payoffs of each role remain unchanged. Controlling for wealth effects in a role-reversal protocol will require reducing the payment for each role which in turn reduces the saliency of each decision. Despite how the experimenter decides to handle wealth effects, the experimenter must then decide if he wants to introduce potential portfolio or feedback effects into the design.

A portfolio effect can exist when a subject is paid for two or more decision tasks, yet does not know the payoff outcomes of any decision until the end of the experiment.[10] When a subject plays more than one role without payoff feedback from the previous role, he/she may view their role decisions, although played out independently, as a diversified investment portfolio. Using the IG as an example subjects could maximize a portfolio of expected earnings by making a riskier trust decision in the first mover role while returning less in the second mover role to insure against losses from a betrayal of trust. Subjects could also maximize a portfolio of interpersonal wealth preferences in the IG and other cooperative games. A subject may defer the responsibility of increasing their partner's payoff to their partner's partner in a separate role pairing. Burks, Carpenter, and Verhoogen (2003), who use this role-reversal protocol, find reductions in first and second

[10] Literature on the preference reversal phenomenon was the first to discuss potential the problems of portfolio effects (Cox 2010; Grether and Plott 1979).

mover generosity in the IG and explain their results as a “reduced responsibility effect” (Burks, Carpenter, and Verhoogen 2003).

One way to avoid possible portfolio effects is to provide the subject feedback on the outcome of their role decision before making their next role decision. Feedback, however, will force the study of role-reversal to adopt the complexity of repeated game theory. Since all roles are paid the problems associated with wealth effects are not avoided. Wealth effects can lead to changes in risk preferences which will change role decisions made under risk (Cox 2010). Wealth effects can also lead to changes in the willingness of subjects to pay for an increase (or decrease) in the incomes of their counterparts.

Those interested in the effect of role-reversal could argue that portfolio effects, feedback effects, and/or wealth effects are not separable from role-reversal in some environments. However, researchers interested in using role-reversal protocols to learn more about behavior in the single-role protocol should avoid these effects by selecting only one role for payment. This study will select one decision for payment to test for a fundamental role-reversal effect.

Another role-reversal protocol design choice is whether to use a sequential move protocol or a strategy method protocol in addition to the role-reversal protocol. Decisions are more often elicited using the sequential move protocol, where the decisions are made in the sequence decisions are played out in the game. This carries the advantage of getting a “hot” or a more emotional response to the first mover’s decision, but it also carries the disadvantage of fewer observations per subject. In the strategy method protocol all decisions are made simultaneously even though the decisions are played

sequentially in the game. Those who move after other players make decisions by submitting a strategy of planned responses to all possible decisions previously-moving players can make. The decisions are then played out using the decisions and strategies previously submitted. The strategy method protocol carries the advantage of more observations per subject possibly giving more information about the effect of role-reversal. The strategy method protocol carries the disadvantage of getting a "cold" second mover response decision. Another disadvantage of the strategy method protocol has already been listed as a disadvantage for selecting one role for payoff in role-reversal: a greater mental cost is imposed on subjects since more decisions must be made under strategy elicitation. The saliency of payoffs for each decision is reduced since only one decision from each strategy set of ten decisions determines final payoffs. Saliency is even further reduced when only one role decision is selected for payoff.

Several studies have looked for behavioral differences between the sequential move protocol and the strategy method protocol (Brandts and Charness 2000; Brosig, Weimann, and Yang 2003; Casari and Cason 2009; Cason and Mui 1998; Oxoby and McLeish 2004). Casari and Cason (2009) find a hot versus cold effect in a binary-decision trust game: second movers are less trustworthy under the strategy method protocol (Casari and Cason 2009). Brandts and Charness (2000) use a role-reversal protocol and do not find a hot versus cold effect. Brosig et al. (2003) use the single-role protocol and do find a hot versus cold effect (Brandts and Charness 2000; Brosig, Weimann, and Yang 2003). Using a role-reversal protocol has not been ruled out as an influence on the hot versus cold effect, so conversely using the strategy method protocol may influence the role-reversal effect as well. Preliminary evidence from Vyrastekova

and Onderstal (2005) suggests that using role-reversal with the strategy method protocol could remove the role-reversal effect, since they found no role-reversal effect comparing their results to Berg et al. (1995) (Vyrastekova and Onderstal 2005).

The strategy method protocol does carry the advantage of being able to minimize problems with order effects in role-reversal protocols for sequential games. Subjects can make all roles decisions with zero feedback under the strategy method protocol. If both role decisions are presented to the subject simultaneously on the same decision sheet or computer screen then subjects are free to make their "first mover" role decision first or their "second mover" decision first. One can then test for possible order effects from the task representation mixing the order in which role decisions are presented on the computer screen or sheet.

One can test order effects to a limited degree if the sequential move protocol is used, and it gets exponentially complicated as the number of roles in the game increase. Consider the simplest case: the 2-person, sequential game. First two types must be created:

- Type A: a subject who plays the first mover role then the second mover role
- Type B: a subject who plays the second mover role then the first mover role

Then the following matching protocols can be created:

- Matching protocol AAA: Type A subjects paired only with Type A subjects who are also paired only with Type A subjects
- Matching protocol ABA/BAB: Type A subjects are paired only with Type B subjects who are paired only with Type A subjects. This naturally creates Type B

subjects paired only with Type A subjects who are paired only with Type B subjects.

- All other matching protocols require at least one subject to be paired with a Type A for one role and a Type B for another. Matching protocol BBB is impossible because at least one subject must play first mover first to create a matching protocol.

Then the following tests of order effects can be conducted:

- Type A subjects in matching protocol AAA can be compared with Type B subjects in matching protocol ABA/BAB to test for order effects in the sequence of roles subjects play.
- Type A subjects in matching protocol AAA can be compared with Type A subjects in matching protocol ABA/BAB to test for order effects in the sequence of roles a subject's partner plays. This can be done for Type A subjects only.

Although there is no consensus on either design issue, this study will only take on the disagreement between selecting one or multiple roles for payment. Burks, Carpenter, and Verhoogen (2003) test for a prior knowledge of role-reversal effect in the Investment Game and find significant reductions in generosity by the first mover and second mover. They use the sequential move protocol version of the Investment Game and use matching protocol AAA in their role-reversal treatment. We test the possibility that their results are sensitive to a design choice to pay for both Sender and Receiver roles by randomly selecting one role decision for payoff in our design. We cannot definitively settle the argument because we do not replicate their role-reversal treatment. For now, we can

make across-studies comparisons of two within-study comparisons of role-reversal in the Investment Game to see if role payment is a prime suspect for the wide variation of role-reversal effects found in the literature.

Experimental Design

The experimental design asks the question: “What effect does applying role-reversal to the Investment Game have on the amounts sent and returned when only one decision is selected for payoff?” Undergraduates from Georgia State University were invited to participate in this experiment through the Experimental Economics Center (ExCEN) recruiter system. The experiment took place in two rooms, labeled Room A and Room B, in the Andrew Young School of Policy Studies building of Georgia State University. Figure 14 shows a layout of the two seminar rooms. Room A and Room B are actually two parts of a large seminar room divided by a thick yet collapsible wall (see zig-zag line). Once the divider is closed, people must enter Room A through door 3 and Room B through door 4. On the left wall of Room A is a large closet area with doors 1 and 2 providing access to it, which is the Experimenters’ Room.

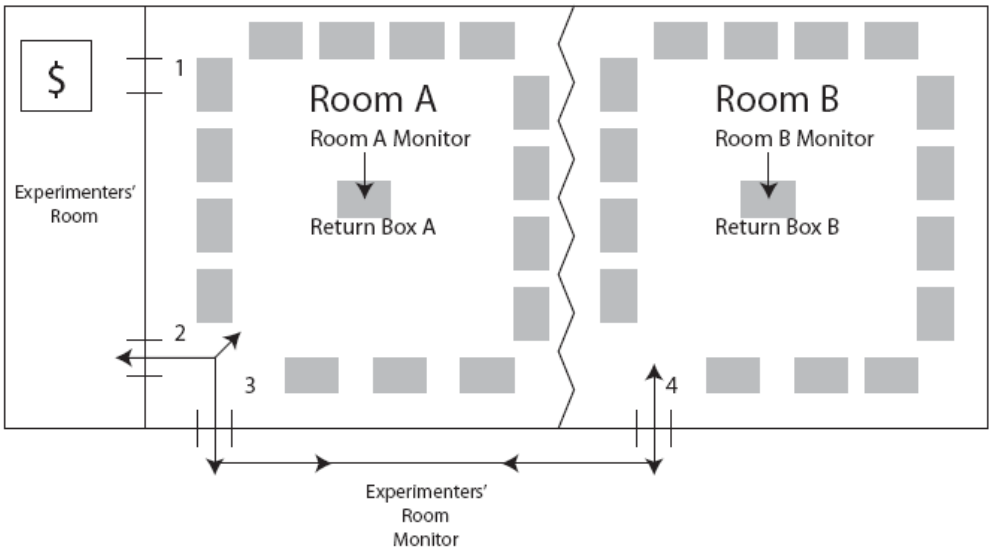

Figure 14. Layout of Rooms Used During the Experiment

Both treatments of this study follow a double-blind protocol. Subjects signed a form in advance to confirm that they will be paid at the end of the experiment. Before making any decisions, subjects are told that their decisions will remain private from the other participants, the room monitors[11], and the experimenters. They are told that anonymity is upheld by allowing them to randomly draw a numbered mailbox key where: (1) they use the number to identify their decisions; and (2) they use the key to collect their earnings related to their decisions.[12]

[11] Three subjects were randomly selected to be Experimenters' Room Monitor, Room A Monitor, and Room B monitor. All monitors were told that they would be paid $25 for participation. The Experimenters' Room Monitor was told that his or her job involved monitoring the procedures conducted by the experimenters and distributing materials to the two Rooms (his or her path is marked out in Figure 14). The Room A and Room B Monitors were told that their job consisted of collecting and distributing materials within their assigned rooms and enforcing the rule of silence in the rooms.

[12] Figure 15 shows how subjects collect their payoffs. Room A subjects collect their payoffs first for convenience, followed by Room B. Subjects were let into door 1 by an experimenter after they dropped off their questionnaire face down in a box. One subject was let in at a time, so subjects would have complete privacy when collecting their payoffs. Once the subject was in the Experimenters' Room she collected her payoff envelope using her key to open her mailbox in the double-blind payoff locker. She then exited door 2 and dropped her key in the box. An experimenter stood by door 2 to verify that she returned her key. Finally, the subject exits door 3 and the building.

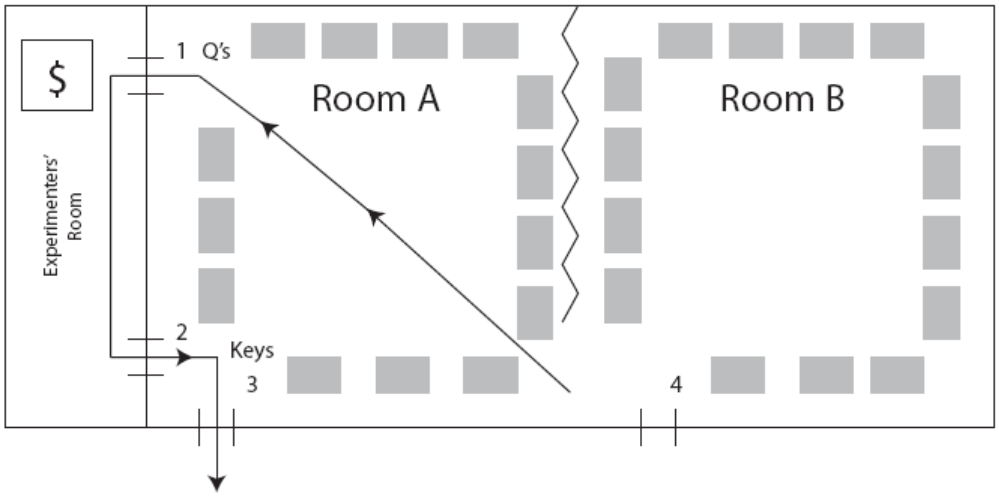

Figure 15. Layout of Rooms During the Payoff Procedure

Before entering Room A or Room B, subjects were asked to randomly draw a green card from a stack passed around by an experimenter. The green cards were used to select the three monitors used in the experiment and to determine which subjects go to Room A and Room B. The green cards in the stack include: 1 Experimenters' Room Monitor card, 1 Room A Monitor card, and 1 Room B Monitor card with the remaining cards divided equally into Room A and Room B cards. Subjects who drew Room A and Room B cards went to their respective rooms and sat at their own table facing the wall (see Figure 14). Although subjects in the same room do not play each other, this setup is designed to maintain privacy.

Instructions were handed to the subjects in each room to read silently.[13] After reading the instructions the experimenter explained the instructions verbally. If there were no further questions the wall divider between Room A and Room B was closed and the experimenters went into the Experimenters' Room and remained there until the end of

[13] The instructions as well as the short summary of instructions handed to subjects for each treatment are located in the Appendix.

the experiment (see Figure 14). Any questions asked by subjects after the experimenters left were relayed by the monitors.

Investment Game Control Treatment

In the IG control treatment each subject plays under the single-role protocol. Subjects who randomly selected Room A cards play the Senders, and the Room B subjects play the Receivers. Their decisions were called the Room A Decision Task and Room B Decision Task.

Subjects in Room A were given "Room A Decision Task" manila envelopes, which contained the materials subjects used to make their Sender role decision. Inside the manila envelope were three white envelopes and a short summary of instructions for handling the envelopes. The first envelope contains their mailbox key, which subjects are instructed to keep for the remainder of the experiment. The second envelope contains their endowment of 10 one-dollar certificates. The third envelope is empty. A subject's Task X involves transferring none, some, or all certificates from the second envelope into the third. The number of certificates transferred to the third envelope states how much he intends to send to the paired person in the other room. The remaining certificates in the second envelope states how much he intends to keep. After the subject has made a decision, he places the second and third white envelopes back in the manila envelope and he drops the manila envelope in the return box. The Experimenters' Room Monitor collects the return boxes from the two rooms and brings them to the experimenters. Two of the experimenters record the decisions made by subjects, and the third experimenter triples each amount sent. They then prepare the manila envelopes for the Room B Decision Task.

The Room B subjects then randomly select a “Room B Decision Task” manila envelope from the return box. Inside are the materials necessary to make their Receiver role decision. The manila envelope contains four small white envelopes and a short summary of instructions on how to use the envelopes to make their decision. The first envelope contains their mailbox key, which subjects are instructed to keep for the remainder of the experiment. The second envelope contains 10 certificates that subjects are instructed to keep. The third envelope contains the tripled number of certificates sent by the person from Room A. The fourth envelope is empty. The Room B Decision Task involves transferring none, some, or all of their tripled certificates from the third envelope into the fourth. The number of certificates transferred to the fourth envelope states how much she intends to return to the paired person in the other room. The number of certificates left in the third envelope states how many of the certificates she intends to keep. After the subject makes a decision, she places the second, third, and fourth white envelopes back in the manila envelope and she drops the manila envelope in the return box. The experimenters’ room monitor collects the return boxes from the two rooms and brings them to the experimenters. The experimenters record the decisions made by subjects, and prepare the payoff envelopes by exchanging paper certificates for U.S. dollars.

Role-Reversal Treatment

In this treatment subjects play Sender and Receiver in two independent IG repetitions under the role-reversal protocol. Subjects know in advance that only one game will be selected for payoff, and their Sender and Receiver role will have an equal chance of determining their payoff. Since every subject played both roles in this treatment it did

not matter which room they selected into. The “Room A Decision Task” (Sender role) from the IG control became “Task X” for all subjects, and the “Room B Decision Task” (Receiver role) became “Task Y” for all subjects. Subjects were paired with another subject from each room for each decision. There was a small chance (1 out of 15) a subject could be paired with the same person for both decisions, but the double-blind procedure prevented either subject from knowing if this occurred. Similar to the procedure of Burks et al. (2003), all subjects made their Sender decision (Task X decision) first, and the tripled certificates were sent to opposite rooms for subjects to make their Receiver decision (Task Y decision).

Since only one decision was randomly selected for payoff one-dollar certificates were printed in two colors to separate each role a subject plays. RED certificates were used for one IG and BLUE certificates were used for the other IG. Subjects in Room A used BLUE certificates for Task X and RED certificates for Task Y. Subjects in Room B used RED certificates for Task X and BLUE certificates for Task Y. After all decisions were made only one of each subject’s decisions, one of the two games, were randomly selected for payoff by the experimenters’ room monitor. This was done by the experimenters’ room monitor, who opened the room divider and flipped a coin to determine which color certificates were exchanged for U.S. Dollars and which color certificates became worthless. The room monitors confirmed the result of the coin flip.

Results and Analysis

In the IG control there were two sessions with 32 subjects in the first session and 28 subjects in the second (excluding monitors). The two sessions combined generated 30 IG pairs, 30 observations of amounts sent, and 30 observations of amounts returned. In the role-reversal treatment there were 30 subjects making Sender and Receiver decisions, thereby generating 30 IG pairs. A detailed summary of the decisions made in both treatments is found in Table 7. From the summary table it is clear that decisions made in the IG control treatment and role-reversal treatment are not significantly different from one another.

Table 7. Detailed Summary of Sender and Receiver Decisions by Treatment

	IG Control Treatment		Role-Reversal Treatment	
Summary Stats	Sender Sent	Receiver Returned	Sender Sent	Receiver Returned
# of obs.	30	30	30	30
Mean	4.10	3.07	4.23	3.47
Median	3.00	1.50	3.50	3.00
Std. Dev.	3.49	4.33	2.81	3.44
Variance	12.16	18.75	7.91	11.84
Skewness	0.42	2.24	0.50	1.13
Kurtosis	1.89	8.94	2.72	4.24

Sender Role Decisions

The send mean was $4.10 (standard deviation of $3.49) in the IG control, and the send mean was $4.23 (standard deviation of $2.81) in the role-reversal treatment. In Table 8, standard t-tests and Mann-Whitney tests confirm that the amount sent in the role-reversal protocol is not significantly different from the single-role protocol. The send distributions do appear slightly different in Figure 16 with slightly more weight in the

middle for the role-reversal treatment. The distribution of amounts sent is bimodal ($3 and $5) for the role-reversal treatment, whereas there are four modes ($0, $3, $5, and $10) for the IG control treatment. The Kolmogorov-Smirnov test has an exact p-value of 0.808, so the null hypothesis that the distributions are equal cannot be rejected. The two-sample variance comparison test shows that the variance is lower under role-reversal but at an insignificant level (one-sided p-value = 0.1262).

Table 8. Statistical Tests for Treatment Differences in Amounts Sent

Treatment Comparison	Means Test (t-test)	Mann-Whitney Test (rank sum test)	Variance Test	K-S Distribution Test
IG Control versus Role-Reversal	t = -0.163 Pr(T>t) = 0.565	z = -0.442 Prob>|z| = 0.659	f = 1.5377 d.f. = 29,29 Pr(F>f) = 0.1262	Exact p-value = 0.808

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

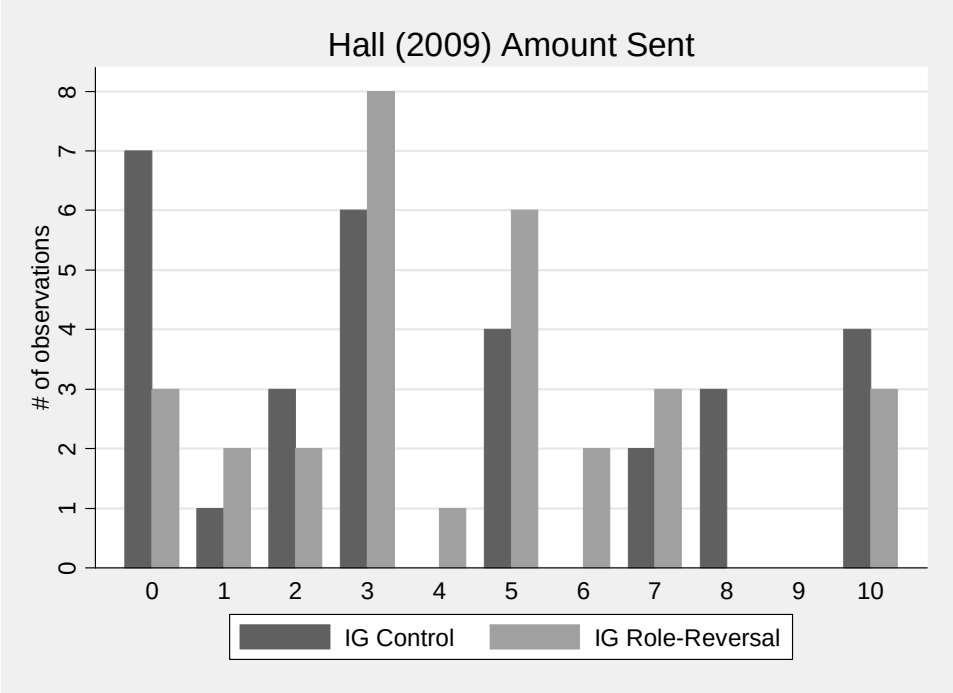

Figure 16. Amount Sent by Treatment

Receiver Role Decisions

Table 7 shows that the mean amount returned was $3.07 (standard deviation of $4.33) in the IG control, and $3.47 (standard deviation of $3.44)[14] in the role-reversal treatment. The standard deviation for Receiver decisions is also lower in the role-reversal treatment, but this is likely driven by the lower variance in the amount received. The returns are higher in the role-reversal treatment on average, but more was also sent on average in the role-reversal treatment. Does the difference hold after controlling for the amount received? Table 9, shows the results from the tobit regression on the amount returned. The tobit regression uses data from both treatments. An intercept dummy for role-reversal protocol data is needed to see if a changing the environment from a single-role protocol leads to different Receiver decisions. A slope dummy for the role-reversal protocol, which is equal to the amount sent to the Receiver for role-reversal data and 0 otherwise, could be used because the role-reversal effect may be stronger for larger amounts sent, but the slope dummy was found to be negative, near zero, and highly insignificant, so it was dropped from the tobit regression. The role-reversal intercept dummy is negative, but it appears to have no significant effect on the amount returned.

[14] In the role-reversal treatment the sum of all amounts received does not match the tripled sum of all amounts sent due to an experimenter error. $3 was reported as sent by a subject in Room B, and was tripled to $9 and stuffed into the tripled number of certificates envelope. The error occurred when the original $3 sent was also stuffed into the tripled number of certificates envelope, meaning that a subject in Room A received $3 + $9 = $12 when they should have only received $9. Had the subject in Room A actually sent $4, the send mean would have been $4.26 instead. The subject in Room B who received $12 thought that the other subject sent $4, and had no knowledge of the error, so the Receiver data is analyzed as if the Sender from Room A sent $4 instead of $3.

Table 9. Tobit Regression of Return Decisions

Constant	Role-Reversal Intercept Dummy	Received/3	Heteroscedasticity Term = Received	Sigma: Disturbance Std. Dev.
1.056	-0.692	0.594	0.061	1.425
(0.302)	(0.416)	(0.017)*	(0.003)**	(0.006)**

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Figure 17 shows a scatter plot[15] with linear prediction lines based on the amounts returned for each amount received in both treatments. Although the role-reversal protocol intercept dummy is insignificant in the tobit regression, the slope of the linear prediction line is steeper for the role-reversal treatment. The average return decision of those who received between $3 and $15 was $3.71 (14 observations) in the IG control and $2.89 (19 observations) in the role-reversal treatment. The average return decision of those who received between $18 and $30 was $4.44 (9 observations) in the IG control and $6.13 (8 observations) in the role-reversal treatment. Receivers experiencing salient role-reversal appear to return less (return more) when the amount received is low (high) relative to Receivers in the IG control but more data is needed to see if this result holds. Figure 18 plots the same data, except the amount returned divided by the amount sent to the subject is mapped on the y-axis. The IG control trend line shows that it does not pay to send more than $5 on average. The role-reversal trend line shows that on average you get slightly less than your money back for all amounts sent.

[15] The scatter plot points were displaced so plot points from both treatments can be seen using the "jitter" option for scatter plots in Stata. Subjects made decisions over whole-dollar amounts, so points near each other are in fact the same decision.

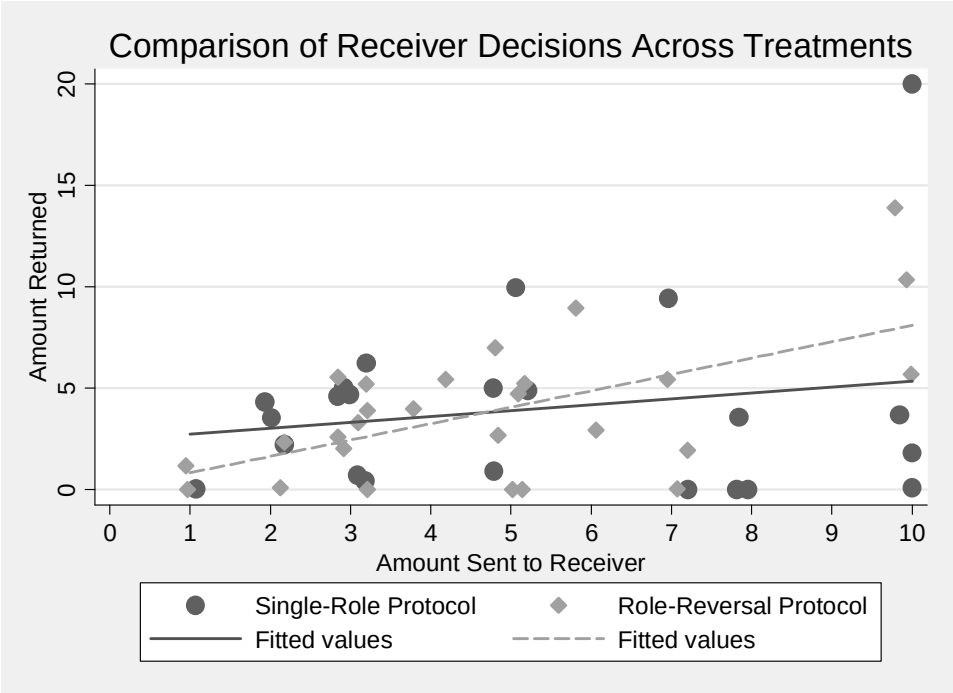

Figure 17. Amount Returned by Treatment

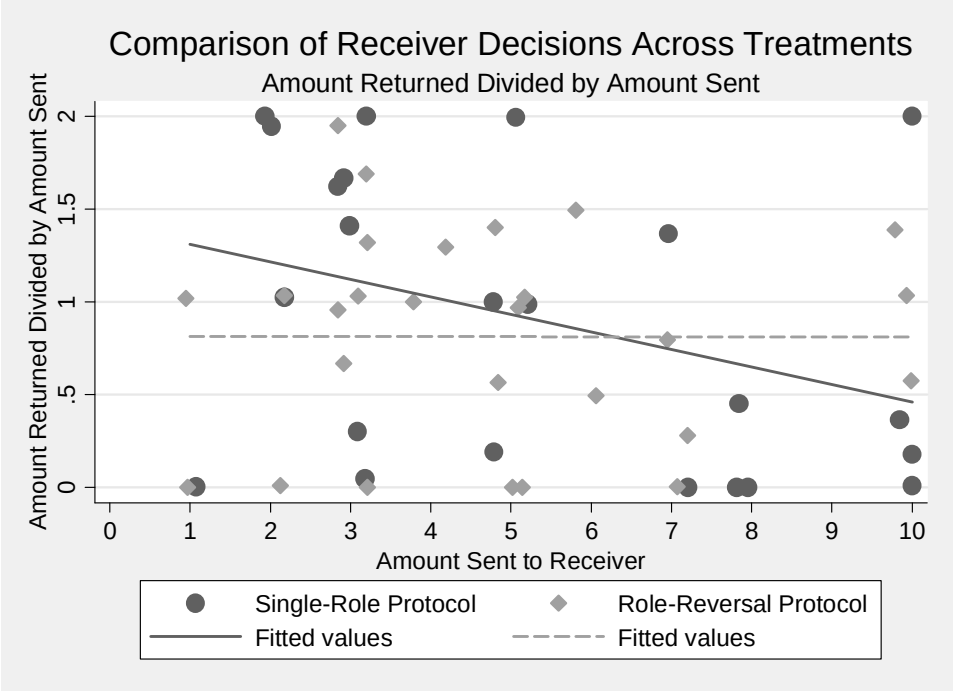

Figure 18. Amount Returned Divided by Amount Partner Sent by Treatment

What Else Can be Learned From the Role-Reversal Protocol?

What can be learned from using the role-reversal protocol in trust games? First, we can see if Sender behavior can explain Receiver behavior and *vice versa*. Here, only individuals who received positive amounts can be analyzed. Can the amounts returned by individuals explain the amounts they have sent. Table 10 shows tobit regression results with the amount sent as the dependent variable and the ratio of the amount returned over the amount the Sender was sent in the Receiver role as the independent variable. The sign of the coefficient is positive, meaning those who returned more were likely to have sent more, but is only near significant (p-value = 0.080).

Table 10. Tobit Regression of Amount Sent Under Role-Reversal

Constant	Returned/ (Received/3)	Heteroscedasticity Term = Received	Sigma: Disturbance Std. Dev.
2.794	1.776	-0.004	3.717
(0.009)	(0.080)	(0.883)	(0.017)

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Table 11 shows tobit regression results which tests if the amounts sent by individuals can explain the amounts they have returned. The results show the expected positive sign: on average higher amounts sent lead to higher returns, but the coefficient is insignificant (p-value = 0.152). There is little evidence to suggest that trust predicts trustworthiness in the role-reversal protocol we use. Afterall, individuals can rationally decide to send more and return less if they believe that on average people will return more than they would in the same role. This does not mean that these individuals' decisions are inconsistent, but rather evidence of opportunistic sending. Role-reversal reveals some of these Senders as opportunistic when the amount sent to them as Receiver

is greater than or equal to the amount they sent in their Sender role, yet they return less than they had originally sent. Table 12 shows how subjects in the role-reversal treatment are classified as opportunistic senders. In this experiment 1 out of 14 (7%) subjects tested for opportunistic sending are revealed to be opportunistic senders.

Table 11. Tobit Regression of Amount Returned Under Role-Reversal

Constant	Receiver Sent	Received/3	Heteroscedsticity Term = Received	Sigma: Disturbance Std. Dev.
-1.186	0.231	0.781	0.045	1.479
(0.400)	(0.152)	(0.017)*	(0.186)	(0.065)

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Table 12. Role-Reversal and Opportunistic Senders

Study	% Revealed Opportunistic Senders	Revealed Opportunistic Senders	Received/3 Greater than Amount Sent	Received Certificates	Sender Decisions
Hall	7.1%	1	14	27	30
BCV(2003)	36.4%	8	22	33	40
CG (2007)	26.9%	14	52	82	100

Survey Analysis

Subjects in both surveys were told, after learning their payoff, that they would receive an additional $5 for filling out a short survey. The general survey questions can be found in the Appendix. Table 13 shows the simple correlations of survey responses to Sender role decisions. Survey questions related to general trust of strangers have higher correlations with the amounts sent in the control treatment. Questions related to the stakes of the trusting action have higher correlations in the control treatment. Questions related to how often subjects loan money and personal items to friends have higher correlations

in the role-reversal treatment. This suggests that Sender decisions made under role-reversal are more correlated with survey questions that ask about subjects actual practice and experience with trust. Survey responses are weakly correlated to the amount returned in both treatments, with the exception of it being positively correlated and significant in the helpful question for IG data.

One of the survey questions in the Investment Game control treatment asked subjects to hypothetically play the other role. Receivers were asked what they would have sent if they played the Sender role instead. Receivers in the control treatment claimed that they "would have sent" an average of $5.67 if they were Senders instead. An average of $4.23 was sent in role-reversal treatment. Senders "received" the same distribution of amounts received by Receivers in the role-reversal treatment and asked how much they would return if they played the Receiver role instead. Senders in the control treatment claimed that they "would have returned" an average of $6.60 if they were receivers instead. This is much higher than the average of $3.85 returned in the role-reversal treatment. This disparity provides some evidence of the positive self-image effect Senders have about what they would do as Receivers in the control treatment. The positive self-image effect could have affected Sender's decisions in both treatments or not at all, since the average amounts sent are not different across treatments.

Table 13. Survey Correlations with the Amounts Sent and Returned in both Treatments

	Amount Sent		Proportion Returned	
Decisions/Responses	IG Control	Role-Reversal	IG Control	Role-Reversal
sent	1.000	1.000	…	0.259
proportion returned	…	0.259	1.000	1.000
female	-0.102	0.015	-0.009	-0.004
mother's education	-0.071	0.230	0.009	-0.050
father's education	0.179	0.313	0.211	-0.033
marital status	0.256	0.160	…	-0.281
family income	-0.111	0.050	-0.206	0.069
fair	0.330*	0.105	-0.166	-0.014
helpful	0.198	0.026	0.456**	-0.080
general trust	0.361*	0.111	-0.067	0.077
stranger trust	-0.524***	-0.147	0.124	-0.195
small stakes trust	0.239	-0.127	0.215	-0.122
high stakes trust	0.467***	0.150	0.082	0.087
loan money to friends	0.120	-0.248	0.128	-0.195
loan stuff to friends	-0.120	-0.378*	0.265	-0.210
number of friends	0.270	-0.007	-0.149	0.144

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

Conclusion

We conducted an experiment to test the effect of role-reversal in the Investment Game and found no significant effect. The lack of a role-reversal effect is likely due to selecting one decision for payment in the role-reversal protocol. Support for this conjecture comes from the previous test of role-reversal effects on the trust game conducted by Burks et al. (2003). Paying for both roles in their role-reversal treatment they found significant reductions in the amounts sent and returned. Their results are important because they show the joint effects of portfolio effects, income effects, endowment effects[16], and role-reversal effects. These extra effects can still be considered

[16] The following role-reversal trust games remove the $10 endowment for the Receiver: Burks et al. (2003), Vyrastekova and Onderstal (2005), and Chaduri and Gangahardan (2007). If their results are compared to Investment Game data, then a joint test of endowment and role-reversal effects are tested.

part of role-reversal for some environments, but they inhibit testing for a fundamental role-reversal effect.

There is also some evidence that paying for both roles can encourage opportunistic sending. Table 12 compares the proportion of opportunistic senders in the role-reversal data of this experiment to role-reversal data where subjects are paid for both roles. In our role-reversal treatment only 1 out of 14 testable subjects were revealed as an opportunitic sender (7.1%). More subjects were revealed to be opportunistic senders when they are paid for both roles: 8 out of 22 (36.4%) subjects in Burks et al. (2004) and 14 out of 52 (26.9%) subjects in Chaduri and Gangahardan (2007).

After selecting one decision for payoff we found no role-reversal effect, which is good news for researchers whose purpose of using a role-reversal protocol is to learn more about behavior in the game under its traditional single-role protocol. Paying for multiple roles introduce features that limit comparability between the role-reversal and single-role protocols. Research opportunities continue for those who wish to test for a role-reversal effect in other domains.

CHAPTER IV

TRUST IN PRIVATE AND COMMON PROPERTY EXPERIMENTS: EFFECTS OF STRONGER PROPERTY RIGHT ENTITLEMENTS

Introduction

This paper builds on the Cox, Ostrom, and Walker et al. (2009) study titled, "Trust in Private and Common Property Experiments." Cox, Ostrom, and Walker et al. (2009), henceforth COW, addressed notions that common property is fundamentally over extracted, neglected, and abused. Predictions of such "tragedies of the commons" are often based on reasoning that confuses common property with open access (Hardin 1968). Cox, Ostrom, and Walker have an ongoing research project designed to look for behavioral differences in assigning endowments as private and common property. In the COW study, they look at two payoff equivalent 2-person sequential games: the Private Property Trust Game and the Common Property Trust Game. Both games are designed to measure the generosity of the first mover (denoted as the Type X person) and the cooperative response of the second mover (denoted as the Type Y Person). The games differ in the initial property right assignments defined by the endowments. They find marginally greater first mover generosity and second mover cooperation in the Common Property Trust Game; however the game behavior was significantly different (Cox et al. 2009).

We investigate whether these results are robust to assigning stronger property right entitlements. We assign stronger property right entitlements by requiring subjects to meet a performance quota in a real effort task to earn their private or common property endowments. This simple change reveals some insights about behavior in private and common property environments previously unseen. Although the levels of generosity and

cooperative responses are similar across regimes, first mover decisions move to the extremes of zero and full generosity in the Common Property Trust Game.

The COW study and our initial investigation had subjects make decisions using the sequential move protocol. Under this protocol, a Type Y (second mover) decision was elicited after the Type Y person learned their paired Type X (first mover) player's decision. This protocol made Type Y responses to some possible (but not observed) Type X choices unknowable. To measure Type Y decisions in this domain we employ the strategy method protocol. This protocol elicits responses from a Type Y player for each potential decision made by the Type X player. Using this protocol, we find significant evidence of lower Type Y cooperation in the Common Property Trust Game than in the Private Property Trust Game.

Another important finding is that differences in cooperative behavior between private and common property environments are quite sensitive to multiple dimensions of saliency. The two dimensions of saliency present are the saliency of property ownership and the saliency of monetary payoffs. A reduction in the saliency of property ownership can be accomplished by reducing the performance quota subjects have to meet to earn their private or common property endowments or by removing the real effort task entirely. A reduction in the saliency of monetary payoffs for the Type Y player can be accomplished by switching from the sequential move protocol to the strategy method protocol. These reductions in saliency led to more generous Type X decisions in both games. However, the increase in generosity is only significant when the switch to the strategy method is made, although the switch to the strategy method does not affect the Type X player's saliency of payoffs. Tobit analysis of Type Y decisions reveal that either

reduction in saliency leads to more Type Y generosity in both games after controlling for the amount received, although the finding is not statistically significant. The saliency of property ownership has a strong effect on Type Y players' sensitivity to Type X deviations from the status quo in these trust games. It is sensitivity to the status quo which makes these games different behaviorally. Our findings have implications for how we model social preferences and how we test them empirically.

The essay is organized as follows. The first section introduces the Private and Common Property Trust Games and the related theoretical predictions. The second section discusses the COW study design and results. The third section discusses the potential impact of stronger property right entitlements. The fourth section employs stronger property rights by adding a real effort task to the COW design. Design of the real effort task, and results from employing it are discussed. The fifth section discusses the advantages and disadvantages of using the strategy method protocol as opposed to the sequential move protocol. The sixth section adds the strategy method protocol to the design, and results from adding it are discussed. The seventh section discusses the impact saliency has on behavior across the two games. The eighth section analyzes the survey data, and the last section concludes.

Theory and Hypotheses

The Private Property Trust Game and the Common Property Trust Game are both derived from the Investment Game (Berg, Dickhaut, and McCabe 1995). In the Investment Game, there is a first mover and a second mover who interact in a one-shot game. Both start with an endowment of $10 as their private fund. The second mover is constrained to keep his/her $10 endowment, while the first mover must decide whether to

send none, some, or all of his/her $10 in $1 units to the second mover. Each dollar sent by the first mover is tripled by the experimenters and added to the private fund of the second mover. Sending money creates a surplus which the second mover must then decide whether or not to share. A maximum surplus of $20 is generated when the first mover sends his entire endowment of $10. The second mover can return to the first mover any amount (in whole dollars) less than or equal to the tripled amount added to her private fund (Berg, Dickhaut, and McCabe 1995).

The amount sent by the first mover is traditionally interpreted as the level of trust in the second mover. The amount returned is traditionally interpreted as the second mover's willingness to reward that trust, or positive reciprocity. However, Cox (2004) showed that first mover and second mover actions can be partially motivated by unconditional altruism by using first mover and second mover dictator controls for the Investment Game. Still, we use the traditional interpretations for simplicity when we refer to decisions of "full trust" and "no trust" made in the Common Property Trust Game.

The 2-person Private Property Trust Game is different from the Investment Game in only one aspect: the second mover can return none, some, or all of his $10 endowment in addition to the tripled amount received if he/she wishes to do so. This change is necessary to make comparisons with the Common Property Trust Game possible because common property is shared jointly and the second mover is allowed to leave all of the common property for the first mover if he/she wishes to do so. The 2-person Common Property Trust Game is the inverse version of the Private Property Trust Game. In the Common Property Trust Game $40, the maximum amount that can be generated for subject pairs in the Investment Game and 2-person Private Property Trust Game, is

assigned as the amount of the common property endowment. The common property is described as a “joint decision fund” both subjects can withdraw from. The first mover can withdraw up to $10, in whole dollar units, from the joint fund and place it into his/her private fund. However, each dollar withdrawn by the first mover reduces the joint fund by $3. The second mover’s decision is on how to divide the remaining joint fund between his/her private fund and the paired first mover’s private fund after the first mover’s decision.

The 2-person Private Property Trust Game and the 2-person Common Property Trust Game are theoretically isomorphic games according to “economic man” game theory. This means the decisions should not be different in the two games according to that theory. The subgame-perfect Nash equilibrium is the same for both games: the second mover will return none (divide none) of her private fund (remaining joint fund) to the first mover, and the first mover, expecting this, will send nothing to the second mover (withdraw $10 from the joint fund).

The Investment Game (Berg, Dickhaut, and McCabe 1995), has the same subgame-perfect Nash equilibrium as the Private and Common Property Trust Games. However, repeated deviations from this prediction have been generated in laboratory experiments (Berg, Dickhaut, and McCabe 1995; Cox 2004). New theory has been developed to model social preferences in order to account for deviations from the standard theory of selfish preferences (Bolton and Ockenfels 2000; Charness and Rabin 2002; Fehr and Schmidt 1999). These theories also predict that the Private and Common Property Trust Games are isomorphic game pairs because they model unconditional preferences over the final distributions of wealth amongst the set of distributions

available. The two games are payoff equivalent and are isomorphic under these theories because the set of final distributions are identical across games.

Revealed Altruism Theory implies that these games are not isomorphic (Cox, Friedman, and Sadiraj 2008). Revealed Altruism Theory was developed to model second mover behavior in sequential games involving social dilemmas. First, the theory allows for individual preferences to include other-players' earnings as well as their own earnings. An individual's preferences are characterized as more altruistic than another if he/she is willing to pay more of his/her own income to increase the income of another. Second, the theory provides a partial ordering of the generosity of opportunity sets that the first mover can offer the second mover.

Revealed Altruism Theory states that individual preferences can become more or less altruistic depending on the actions of another agent. Reciprocity, denoted as Axiom R, states that if a first mover engages in an intentional action that provides a more generous opportunity set to the second mover, then the second mover will have preferences that are more altruistic towards the first mover <u>because</u> of the first mover's generosity (Cox, Friedman, and Sadiraj 2008).

Evidence of Axiom R in the Investment Game comes from comparing second mover decisions in Treatments A and C of Cox (2004). Treatment A is the Investment Game. Treatment C takes the opportunity sets offered by first movers in Treatment A, and randomly allocates them to second movers. Second movers in Treatment A know that they received a more generous opportunity set <u>because</u> the first mover was generous, while second movers in Treatment C know their paired first movers had no part in determining their opportunity set. Evidence of Axiom R comes from significantly greater

returns by second movers in Treatment A than in Treatment C after taking into account the income effects of more generous opportunity sets (Cox, Friedman, and Sadiraj 2008).[17] Following evidence from Investment Game data, the similar Private and Common Property Trust Games should follow Axiom R.

Axiom S is the section of Revealed Altruism Theory which says that the two games are not isomorphic. Axiom S distinguishes between acts of commission, which overturn the status quo, and acts of omission which uphold the status quo. The status quo is defined by the opportunity set determined by the initial endowments. A first mover upholds the status quo by offering the second mover the opportunity set defined by the initial opportunity set and overturns the status quo by offering any other opportunity set. Axiom S states that if the decision made by a first mover overturns the status quo then the reciprocal response, for individuals with preferences consistent with Axiom R, will be stronger than when the status quo is upheld.

A feasible set is defined as a convex set of (m, y) ordered pairs where m represents the second mover's money payoff and y represents the first mover's money payoff. For both the Private and Common Property Trust Games the opportunity sets the first mover can offer to the second mover are defined by downward sloping lines in 2-dimensional m x y space. The lists of opportunity sets are identical across games, but the status quo set E determined by the endowments is different. E^P is the least generous opportunity set a first mover can offer in the Private Property Trust Game and is defined as $E^P = \{(m, y) \in R^2_+ : m + y = 20, m \in [0,10]\}$. E^c is the most generous opportunity

[17] It is not easy to disentangle increases in second mover cooperativeness into increases in reciprocity due to first mover generosity and increases in unconditional altruism due to increases in income. Cox, Friedman, and Sadiraj (2008) achieved this by controlling for the size of the opportunity set in a Stackelburg mini-game (Cox, Friedman, and Sadiraj 2008).

set a first mover can offer in the Common Property Trust Game and is defined as $E^c = \{(m, y) \in R_+^2 : m + y = 40, m \in [0,40]\}$. To uphold the status quo set the first mover must send nothing to the second mover in the Private Property Trust Game and withdraw nothing from the joint fund in the Common Property Trust Game. A first mover overturns the status quo set in the Private Property Trust Game by sending tokens. A first mover overturns the status quo set in the Common Property Trust Game by withdrawing tokens.

Second movers with preferences consistent with Axiom R and Axiom S will care about how the opportunity set compares to the entire list of opportunity sets the first mover could have chosen and also how the opportunity set chosen by first movers compares to the status quo opportunity set. Second movers will respond more altruistically towards first movers who overturn the status quo in the Private Property Trust Game by sending 1, 2, 3, …, or 10 tokens than first movers who equivalently withdraw 9, 8, 7, …, or 0 tokens in the Common Property Trust Game. Also, second movers will respond less altruistically towards first movers who overturn the status quo in the Common Property Trust Game by withdrawing 1, 2, 3, …, or 10 tokens than first movers who equivalently send 9, 8, 7, …, or 0 tokens in the Private Property Trust Game. The net effect is that second mover generosity is predicted to be relatively lower in the Common Property Trust Game.

How can Axiom S be tested in these two games? After controlling for the first mover's decision by matching the tokens left (not withdrawn) to the tokens sent, second movers should leave less for the first movers in the Common Property Trust Game than second movers return to first movers in the Private Property Trust Game.

How would knowledge of second movers with Axiom S preferences affect first mover decisions? If a first mover is not comfortable with fully trusting the second mover, then he may wish to send or not withdraw tokens but not at the maximum of 10. Sending a number of tokens less than ten may disappoint the second mover, but they are still happy because the status quo was less generous. Withdrawing any tokens may disappoint the second mover but also may anger them because the status quo was more generous. At the extreme, the second mover may decide to punish the first mover for withdrawing anything by leaving none of the remaining joint fund to the first mover. If the first mover partially trusts the second mover, but is afraid the second mover may also punish him for withdrawing then he may respond optimally by withdrawing the maximum of 10 tokens or none at all. These extremes are traditionally interpreted as “no trust” and “full trust” although actual levels of trust by first mover may be less extreme. Evidence of first movers anticipating strong Axiom S preferences will be confirmed by a greater proportion of first movers making decisions at the extremes of withdrawing none or all 10 tokens in the Common Property Trust Game.

The Cox, Ostrom, and Walker et al. (2009) Study

The COW study supports Axiom R but not Axiom S. Trust, reciprocity, cooperation, and generosity can be found in both games, but decisions are not significantly different across games. First movers are denoted as Type X players and second movers as Type Y players. Table 14 shows the summary data for the COW paper in the Private Property Trust Game. Their Private Property Trust Game sessions generated 34 Type X decisions (an average of $5.65 was sent) and 34 Type Y decisions (an average of $6.65 was returned). Figure 19 shows a histogram of the Type X decisions

made in Treatment COW1, their Private Property Trust Game treatment. There are modes at 0, 7, and 10 tokens sent.

Table 14. Subject Decisions for COW1 Treatment

2-Person Private Property Trust Game

N-Pairs = 34	Mean	Std. Dev.	Minimum	Maximum
Tokens X Sent	5.65	3.83	0	10
$ Y Returned	6.65	6.43	0	20
$ X Earned	11.00	5.08	0	20
$Y Earned	20.29	9.09	0	40

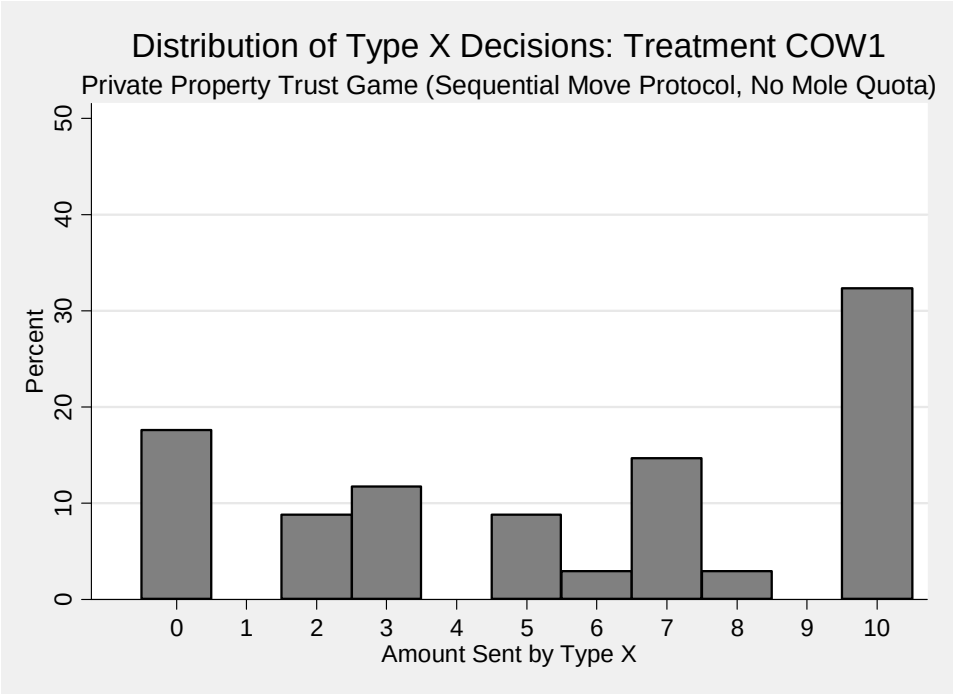

Figure 19. Type X Decisions: COW1, Private Property Trust Game

Table 15 shows the summary data for the Cox et al. (2009) paper in the Common Property Trust Game. Their Common Property Trust Game sessions generated 34 Type X decisions (an average of $6.71 was left) and 34 Type Y decisions (an average of $8.76 was returned). Figure 20 shows a histogram of the Type X decisions made in Treatment

COW2, their Common Property Trust Game treatment. There are modes at 0, 4, and 10 tokens left.

Table 15. Subject Decisions for COW2 Treatment

2-Person Common Property Trust Game

N-Pairs = 34	Mean	Std. Dev.	Minimum	Maximum
Tokens X Left	6.71	3.88	0	10
$ Y Returned	8.76	8.20	0	20
$ X Earned	12.12	6.60	0	20
$Y Earned	20.85	9.76	5	40

How do the two games compare? Figure 21 compares the distributions of Type X decisions for the Private and Common Property Trust Games. The shapes of both distributions are quite similar, with slightly more weight to the right for the Common Property Trust Game. Table 16 shows the results of some standard parametric and non-parametric tests on Type X decisions. The number of tokens left (not withdrawn) in the Common Property Trust Game is greater than the number of dollars sent in the Private Property Trust Game, but the difference is insignificant.

Table 16. Parametric and Non-Parametric Tests of Type X Decisions

	Parametric Tests		Nonparametric Tests	
Test	Means Test (t-test)	Variance Test (F-test)	Mann-Whitney Test (Rank Sum Test)	K-S Test
Null Hypothesis	COW1 = COW2	S.D.(COW1)/ S.D.(COW2) =1	COW1 = COW2	Distributions are Equal
Test Statistic	t = -1.1325 Pr(|T| > |t|) = 0.2614 Pr(T<t) = 0.1307	f = 0.9733 2*Pr(F < f) = 0.9384 Pr(F < f) = 0.4692	z = -1.212 Pr > |z| = 0.2255 Pr(COW1>COW2) = 0.418	D = 0.2059 Exact p-value = 0.473

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

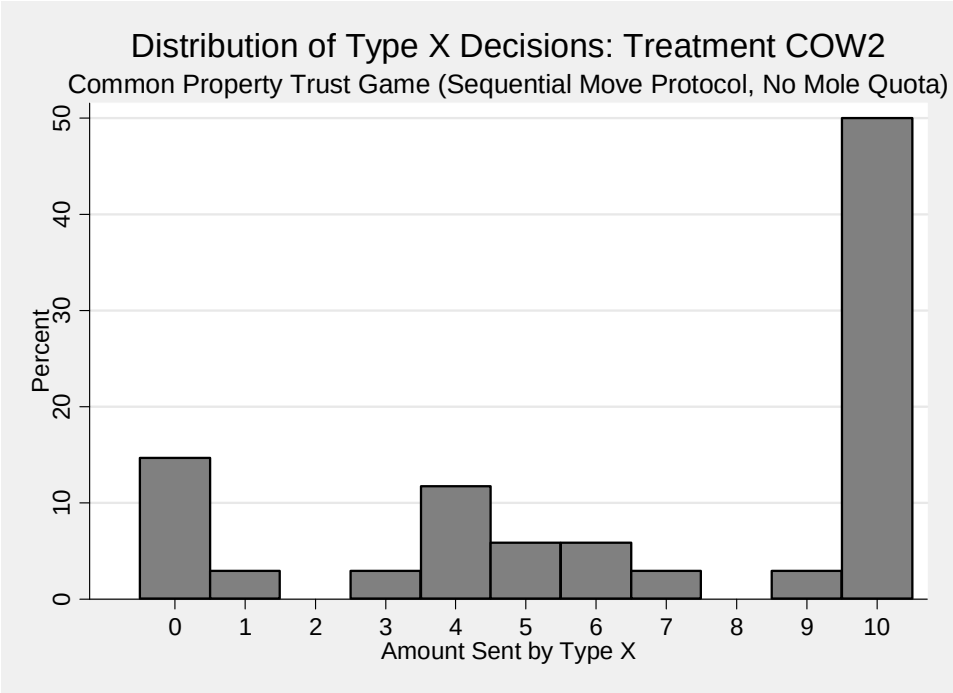

Figure 20. Type X Decisions: COW2, Common Property Trust Game

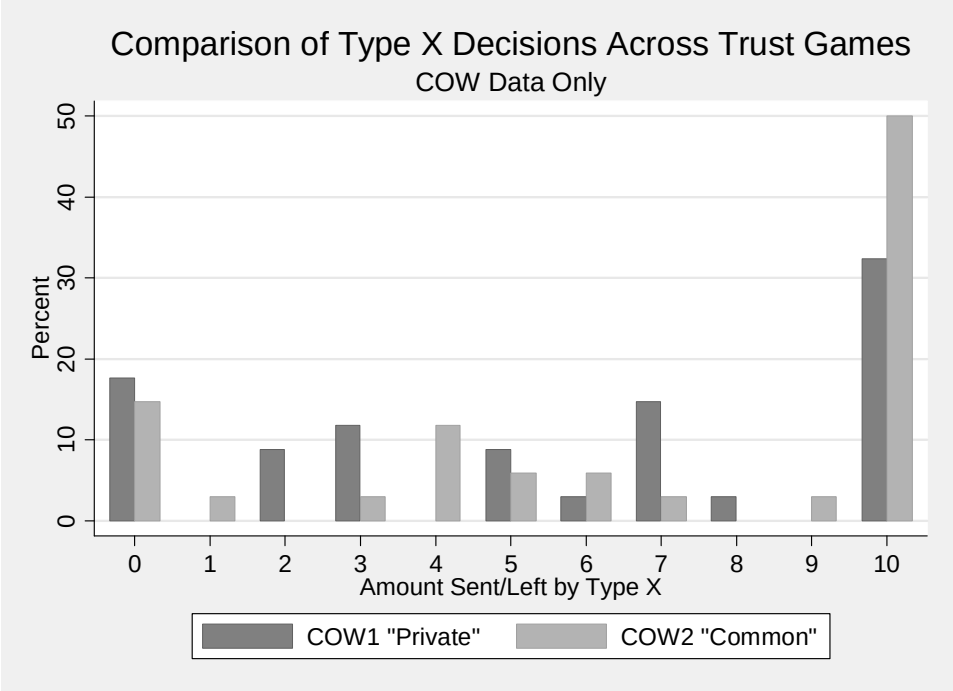

Figure 21. Comparison of Type X Decisions for COW1 and COW2

Figure 22 compares Type Y decisions graphically across games using quadratic line fits with 95% confidence intervals. Comparing the lines more reciprocity or trustworthiness by Type Y players is found in the Common Property Trust Game. However, the 95% confidence intervals overlap significantly. This graphical representation is good for an initial comparison because it shows the general trend of Type Y responses as well as the degree of variability of Type Y responses around each Type X decision. However, the graph implies that negative or Type Y returns larger than upper bound of the opportunity set are feasible when they are not. The quadratic best fit lines and corresponding 95% confidence intervals are formed using the least squares method and do not take into account that the dependent variable (Type Y decisions) is bounded. Type Y decisions lie in a cone: decisions on the amount in dollars and cents to return (leave) are bounded from below at $0.00 and from above by the dollar value of the Type Y Fund (joint fund) determined by the Type X decision. To get a more accurate comparison of Type Y decisions across games we use a tobit regression which recognizes that the data lies in a cone and restricts the error term to the bounds set by the cone.

Table 17 shows a tobit regression of the amount returned by a Type Y player. Our primary variable of interest is the Common Property Trust Game intercept dummy variable. Revealed Altruism Theory, Axiom S predicts that a switch from the Private Property Trust Game to the Common Property Trust Game results in a regime change because the status quo switches from the least generous opportunity set to the most generous opportunity set. Therefore an intercept dummy for the Common Property Trust Game is included in the analysis to test for this effect.[18]

[18] The effect of Axiom S could also be stronger when larger opportunity sets are offered or when greater deviations from the status quo are made. This could be tested by including a slope dummy for returns on

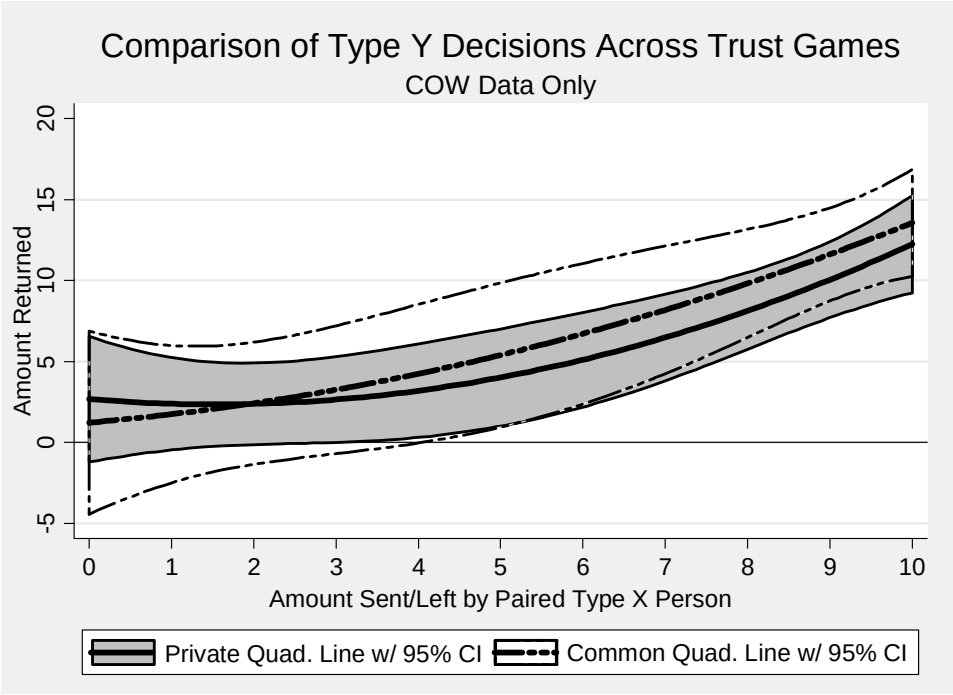

Figure 22. Type Y Decisions for COW1 and COW2

Table 17. Tobit Analysis of Type Y Decisions in COW Study

Number of Observations	67
Constant Term	-2.192
	(.2668)
Type X Sent (Left)	1.37
	(.0000)***
Common Property Dummy	.128
	(.9486)
Heteroscedasticity Term	0.013
	(.1993)
Sigma Disturbance of Std. Dev.	5.04
	(.0006)***

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

the Common Property Trust Game. The Common Property slope dummy variable was dropped from all tobit regressions for the following reason: Revealed Altruism Theory predicts that more generous opportunity sets generate more altruistic responses from second movers but makes no prediction as to whether this effect is increasing, constant, or diminishing as the opportunity sets offered become more generous. Similarly, Axiom S states that deviation from the status quo set may invoke stronger Axiom R preferences, but it makes no prediction on how the location or size of the deviation affects the strength of Axiom S. Also, earlier tobit analysis found the Common Property slope dummy insignificant in all tobit regressions, so it was dropped. This suggests that responses to deviations from a status quo are more sensitive to the act than the magnitude of the deviation towards the opportunity set offered instead.

Hypothesis tests reported by Cox et al. (2009) do not reject the hypothesis that the two games are isomorphic. The differences, albeit insignificant, point in the opposite direction of the prediction following Axiom S. One of two possibilities can explain their results: (1) Subjects have preferences consistent with Axiom R, but not Axiom S fundamentally; or (2) this particular environment and institution did not invoke latent preferences consistent with Axiom S. In the spirit of the second explanation we change the environment by adding saliency to private and common property ownership. Specifically, we ask if strengthening property entitlements will reveal preferences consistent with Axiom S and lead to behavioral differences between the Private and Common Property Trust Games.

Adding Stronger Property Right Entitlements

In experimental economics, monetary incentives are used to 'mimic' the preferences we want to study or to dominate outside preferences subjects may bring to the lab that we do not wish to study. Monetary endowments are also used as resources or property which subjects use to make purchases, transfers, and other decisions. Money is used to induce preferences, create incentives, and present opportunity costs to decision-makers (Smith 1982).

More often than not, monetary endowments are given to subjects simply for participating in the experiment. In other words they receive "house money" from the experimenter's research budget and are asked to make decisions with that money. Subjects could treat this "house money" differently than if the same money came from their regular income (Clark 2002). Milton Friedman's permanent income (PI) hypothesis states that subjects who prefer to smooth lifetime consumption will have a lower

marginal propensity to consume a one-time gain in income (Clark 2002; Friedman 1957). Although some subjects participate in multiple experiments, experimental house money is not a regular source of income. Some studies have found that unexpected one-time gains encourage risk taking with the new money (Arkes et al. 1994; Battalio, Kagel, and Jiranyakul 1990; Keasey and Moon 1996; Thaler 1990; Thaler and Johnson 1990). However, Clark (2002) looked for "house money" effects in the voluntary-contributions mechanism (VCM) public goods game and found none, so the "house money" effect is not a completely robust phenomenon (Clark 2002).

Why may property right entitlements not be strong enough already? If subjects treat their endowments as house money, then they may not be treating the endowment as their private or common property. If this is true, then property ownership is not salient to the subject. One way to strengthen entitlements and make property ownership salient is to have subjects earn their private or common property endowments.

How might earning endowments create stronger senses of entitlement? Subjects must bear more effort costs in obtaining the property right than the usual costs of showing-up and devoting time to the experiment, which can develop a stronger attachment to the property. This could motivate subjects' selfish tendencies to ensure they get the most out of the effort they invested in the game. It could also strengthen subjects' preferences for fairness or their risk preferences could change. Once the property right has been earned all costs to obtain it should be considered sunk costs. Whether or not subjects ignore this sunk cost is an empirical question. Daniel Friedman (2007) tested to see if subjects commit the sunk cost fallacy under a variety of different settings, but surprisingly found very few cases where they did (Friedman 2007).

Another convention is to randomly assign subjects to roles with symmetric entitlements. Cherry et al. (2002) compared decisions made with unearned endowments in a Dictator Game baseline (Hoffman, McCabe, and Smith 1996) to a treatment with earned endowments. Low-stakes (high-stakes) endowments of $10 ($40) were earned by dictators answering less than 10 (10 or more) questions correctly on a quiz. Non-dictators had $0, and had no opportunity to take the quiz so entitlements were asymmetric. The percentage of dictators who transferred $0 to the non-dictator increased from 19% (15%) in the low-stakes (high-stakes) baseline to 79% (70%) in the earned endowments treatment (Cherry, Frykblom, and Shogren 2002). Fahr and Irlenbusch (2000) looked at the effect of the relative strength of property rights between the first mover and second mover of the trust game. There were three treatments defined by whether the first mover, second mover, or both had to crack walnuts to play the trust game. If required to crack walnuts, subjects had to collect 150g of walnut kernels in about a half an hour to earn the right to play. They found that the second movers were more generous towards first movers when the first movers worked and even more generous when the first movers worked and they did not work. First mover decisions were similar across treatments (Fahr and Irlenbusch 2000). Hoffman et al. (1994) tested the effects of allowing subjects to earn the right of playing first mover in the Ultimatum Game by scoring high on a general knowledge quiz. They found that first movers offered smaller splits to the second movers who were also less likely to reject compared to players who were randomly assigned to play first or second mover (Hoffman et al. 1994).

Since there is evidence that adding earned entitlements to endowments or player roles has an effect in games similar to the COW study, we want to test if adding stronger private and common property entitlements affects behavior differently in the common and private property treatments. Entitlements will be symmetric, and this will be done by having all players perform the same effort task.

Cox and Hall Sequential Move Protocol Treatments

The experimental design and procedures for the Cox and Hall (CH) Sequential Move Protocol Treatments are identical to the COW treatments with one exception: subjects earn their private or common property endowments by meeting a performance quota in a real effort task. The treatments are called the sequential move protocol treatments because decisions are made in sequence as described in the COW Private and Common Property Trust Games. The experiment procedure was changed from hand-run to computer-run to save time needed for subjects to perform the real effort task.[19] The computerized decision forms and questionnaires are identical in appearance to those in the COW study. Decision forms, questionnaires, and instructions used in this experiment are located in the Appendix. Undergraduate students at Georgia State University were recruited by email using the Experimental Economics Center (ExCEN) recruiter software. The experiment was run using a double-blind procedure which prevents the subjects and experimenters from being able to personally identify any subject's decisions and payments. After signing in subjects entered the ExCEN computer lab and began reading instructions for the real effort task.

[19] The computer-run procedure was programmed using the Visual Basic Express 2008 Edition software.

The real effort task was intended to give subjects a stronger sense of entitlement to their private property or common property endowment. Subjects had to meet a performance quota to earn their endowment, which they were told would be used in the next game. Subjects were also told in advance that if their quota was not met then they would be paid their show-up fee of $5 and asked to leave the experiment without participating in the decision task.

The real effort task was called "The Whack-A-Mole Game." The Whack-A-Mole game is similar to a mole-whacking game that can be played at an arcade, except the moles do not disappear until they are "whacked." This change was made to make the game easy so most subjects could earn their endowments for the decision task provided they put in the effort. Figure 23 shows a typical screen subjects would see during the game. There is a 6 by 4 grid of moles and holes on the field. Each time the subject mouse-clicked a mole picture the picture box would show a hole picture. If the subject clicked on a hole picture nothing would happen. The object of the game is to mouse-click all of the moles until the field is clear of moles (there is only a field of holes). Once a field is cleared the computer generates a new field of moles for the subject to whack. Each picture box has an equal probability of being a mole picture or a hole picture, so fields are halfway full of moles on average.[20] The performance quota required the subject to clear a pre-specified number of fields within an announced time limit. Subjects had to meet the quota to earn the tokens that were used in the Private Property or Common

[20] Each subject faced the same fields in the same sequence because all subjects start with the same probability generating seed.

Property Trust Game. After the time ran out for the Whack-A-Mole game anyone who did not meet the quota was paid $5 and asked to leave.[21]

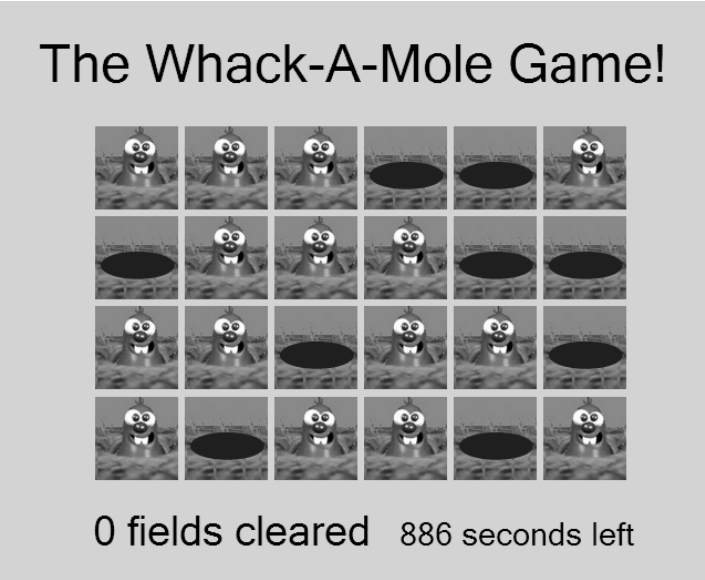

Figure 23. Computer Screenshot of the Real Effort Task

In the first treatment, Treatment CH1, subjects had to clear 120 mole fields in 15 minutes (900 seconds) to earn 10 tokens, worth $1 each. These tokens became their private fund in the Private Property Trust Game. In Treatment CH2, subjects had to clear 120 mole fields in 15 minutes (900 seconds) to earn 20 tokens, worth $1 each, which were combined with the 20 tokens of another subject who met the quota and placed into a joint decision fund totaling 40 tokens worth $40 dollars to be used in the Common Property Trust Game. In Treatment CH3, subjects had to clear 240 mole fields in 30 minutes (1800 seconds) to earn the tokens necessary to play the same Common Property Trust Game played in Treatment CH2. Treatment CH3 was conducted to set the mole-whacking wage equal to Treatment CH1, although the potential final earnings are

[21] If only an odd number of subjects met the performance quota, then the subject who was closest to meeting the performance quota was allowed to participate in the decision task.

identical in all three treatments. Once the subjects who did not fulfill the quota left, the remaining subjects were handed instructions to the Private Property Trust Game if in Treatment CH1 or were handed Common Property Trust Game instructions if in Treatment CH2 or CH3.

After the Whack-A-Mole stage is over the procedures of the Cox et al. (2009) study are followed hereafter. The procedures reported herein were only different in that they were computerized whereas the procedures used by Cox et al. (2009) were hand-run. The instructions and wording of decision-making forms were identical: paper instructions were handed to subjects but decision-forms appeared on subjects' computer screens instead of on paper.

For both games subjects were randomly paired as "Type X" and "Type Y" players. After reading the instructions and listening to a scripted explanation, each subject chose a sealed envelope containing a numbered mailbox key from a box containing identical envelopes. Subjects were told that the number on the mailbox key was their private identification number. They were told the numbered key would open a numbered mailbox containing their earnings from the decision making game plus their show-up fee of $5. After all Type X and Type Y subjects made their decisions, they filled out a Type X or Type Y questionnaire (the Type X and Type Y questionnaires for the Common Property Trust Game are located in the Appendix). While subjects were filling out questionnaires, their payments were stuffed in a locker containing the numbered mailboxes. Subjects collected their earnings one at a time to protect privacy, and then left the experiment.

184 undergraduate students from Georgia State University participated in treatments CH1, CH2, and CH3 run in seven sessions from April until July in 2009.[22] 56 subjects participated in Treatment CH1, the Private Property Trust Game with a 120 mole field task. Table 18 displays the summary statistics for Treatment CH1, which generated 28 Type X decisions (an average of $4.75 was sent) and 28 Type Y decisions (an average of $6.59 was returned). Figure 24 shows a histogram of the Type X decisions in Treatment CH1, the Private Property Trust Game. The distribution is W-shaped with modes at 0, 4, 5, and 10.

Table 19 shows the summary statistics for Treatment CH2, the Common Property Trust Game with a 120 mole-field quota. Treatment CH2 data was collected in three sessions and generated 32 Type X decisions (an average of $5.28 was sent) and 32 Type Y decisions (an average of $6.14 was returned). Figure 25 shows a histogram of Type X decisions. The distribution is U-shaped with high modes at 0 and 10.

Table 20 shows the summary statistics for Treatment CH3, the Common Property Trust Game with a 240 mole-field quota. Treatment CH3 data was collected in two sessions, which generated 32 Type X decisions (an average of $4.84 was sent) and 32 Type Y decisions (an average of $5.70 was returned). Figure 26 shows a histogram of Type X decisions. The distribution is even more sharply U-shaped with high modes at 0 and 10.

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[22] 188 subjects actually participated in the experiment. However 4 subjects were asked to leave because they were unable to meet the mole quota in the last session of Treatment CH2. 3 of the 4 subjects could not meet the mole quota because their computer software shutdown during the middle of the task for these individuals. Since the subjects faced unusual circumstances, all 4 subjects were paid $15 in private for participation once they left the lab. Two subjects, in different sessions, did not meet the mole quota in time for Treatment CH1. These 2 subjects were allowed to play the Private Property Trust Game because an even number of subjects was needed to generate unique Type X and Type Y pairings. See the "Whack-A-Mole Game" Instructions for further details.

Table 18. Subject Decisions for CH1 Treatment

2-Person Private Property Trust Game

N-Pairs = 28	Mean	Std. Dev.	Minimum	Maximum
Tokens X Sent	4.75	3.45	0	10
$ Y Returned	6.59	7.43	0	20
$ X Earned	11.84	5.36	0	20
$Y Earned	17.66	6.86	10	40

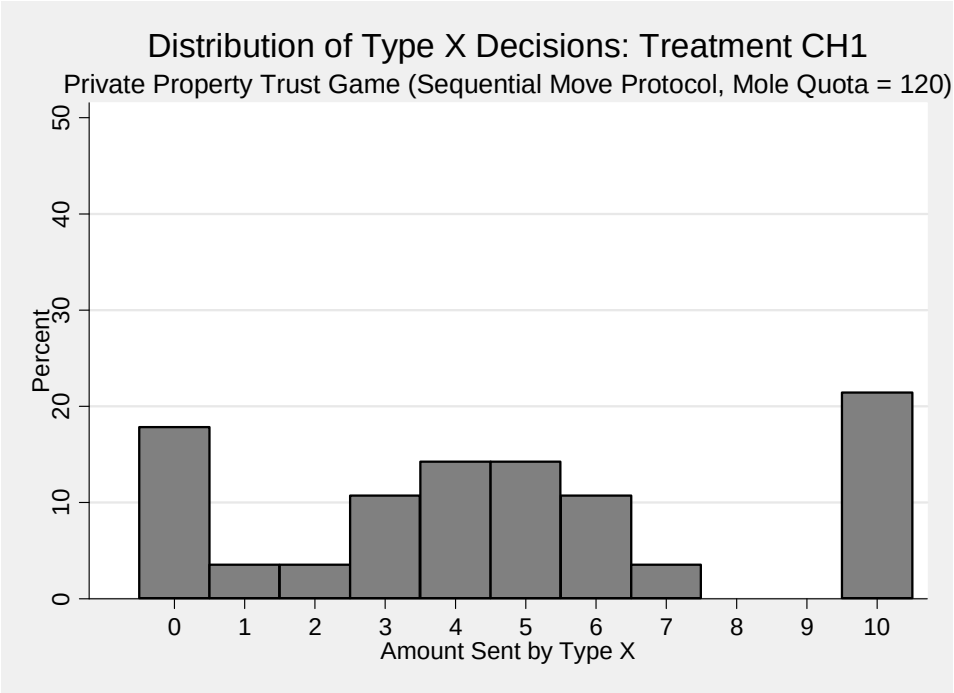


Figure 24. Type X Decisions: CH1, Private Property Trust Game

Table 19. Subject Decisions for CH2 Treatment

2-Person Common Property Trust Game

N-Pairs = 32	Mean	Std. Dev.	Minimum	Maximum
Tokens X Left	5.28	4.70	0	10
$ Y Returned	6.14	7.85	0	20
$ X Earned	10.87	5.86	0	20
$Y Earned	18.77	10.53	0	40

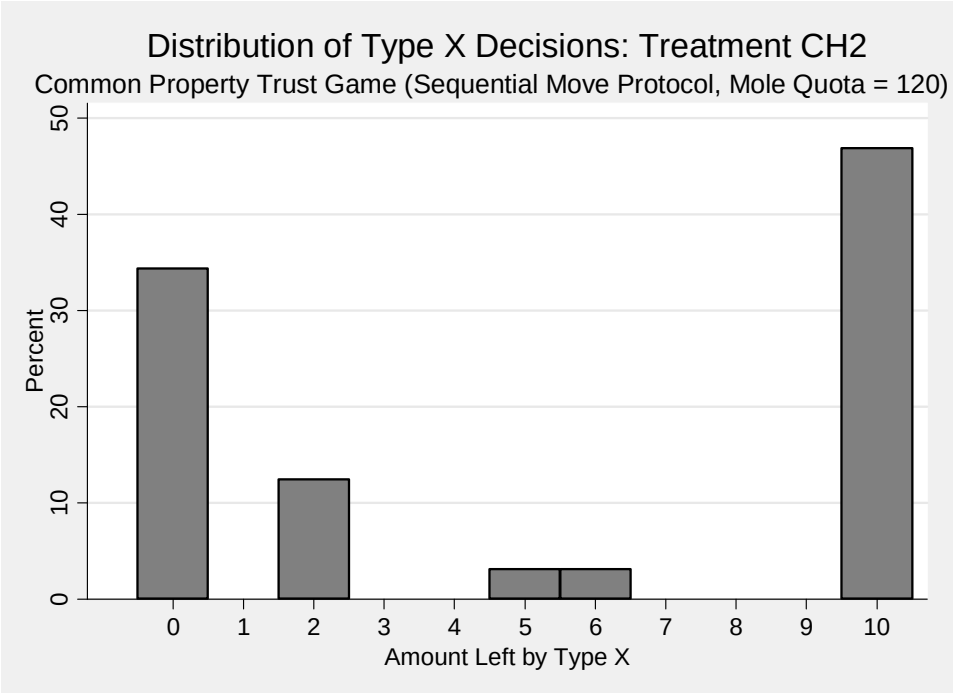


Figure 25. Type X Decisions for CH2, Common Property Trust Game

Table 20. Subject Decisions for CH3 Treatment

2-Person Common Property Trust Game

N-Pairs = 32	Mean	Std. Dev.	Minimum	Maximum
Tokens X Left	4.84	4.78	0	10
$ Y Returned	5.70	8.35	0	20
$ X Earned	10.86	5.99	0	20
$Y Earned	18.83	10.27	0	40

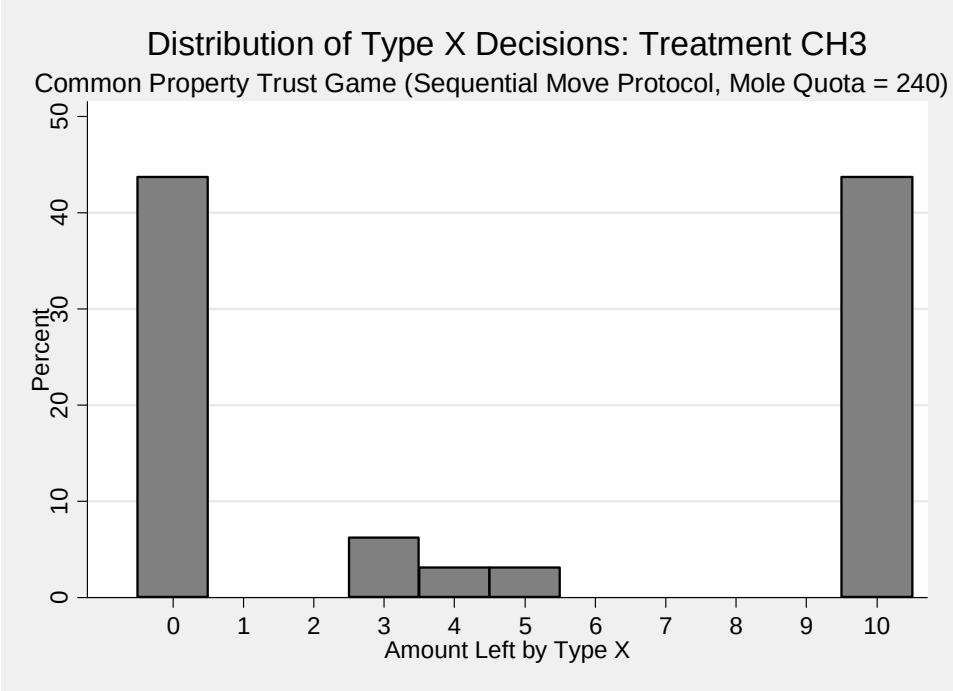


Figure 26. Type X Decisions: CH3, Common Property Trust Game

Table 21. Treatments II and III, Combined Common Property Trust Game Data

2-Person Common Property Trust Game

N-Pairs = 64	Mean	Std. Dev.	Minimum	Maximum
Tokens X Left	5.06	4.71	0	10
$ Y Returned	5.92	8.04	0	20
$ X Earned	10.86	5.88	0	20
$Y Earned	18.80	10.32	0	40

At a glance, the Treatment CH2 and Treatment CH3 generate similar decisions, so the Treatment CH2 and CH3 data is pooled for analysis. Table 21 summarizes the pooled Type X and Type Y decisions from Treatments CH2 and CH3, the Common Property Trust Game. 128 subjects participated in Treatments CH2 and CH3 combined, which generated 64 Type X decisions (an average of $5.06 was left) and 64 Type Y decisions (an average of $5.92 was returned). Figure 27 shows the distribution of Type X decisions in all three treatments. When stronger property right entitlements are added Private and

Common Property Type X decisions have different distributions: Type X decisions move to the extremes of “full trust” and “no trust” in the Common Property regime but not in the Private Property regime.

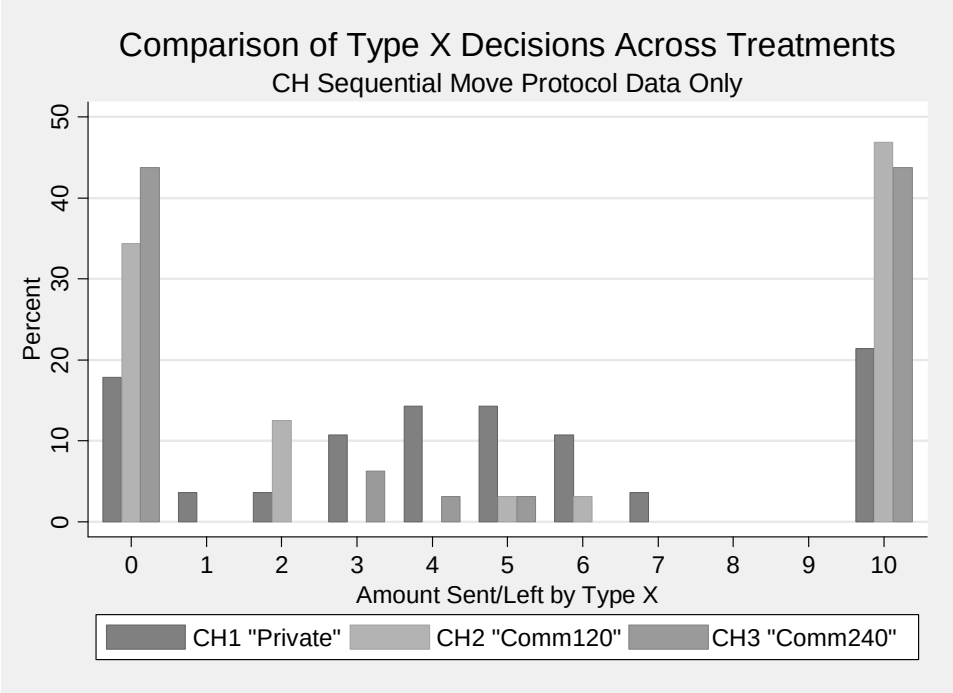


Figure 27. Comparison of Type X Decisions for CH1, CH2, and CH3

Table 22. Means and Distribution Tests for Treatment Differences in Type X Decisions

	Parametric Tests	Nonparametric Tests	
Null Hypothesis	Means Test (t-test)	Mann-Whitney Test (Rank Sum Test)	Distributions Test (K-S Test)
Treatment CH1 = Treatment CH3	t = -0.0878 Pr(|T| > |t|) = 0.9303 Pr(T>t) = 0.4652	z = 0.277 Pr > |z| = 0.7821 Pr(CH1>CH3) = 0.520	D = 0.2589 Exact p-value = 0.224
Treatment CH1 = Treatment CH2	t = -0.5031 Pr(|T| > |t|) = 0.6168 Pr(T<t) = 0.3084	z = -0.245 Pr > |z| = 0.8065 Pr(CH1>CH2) = 0.482	D = 0.2545 Exact p-value = 0.240
Treatment CH1 = Treatment CH2 & CH3 Pooled	t = 0.3559 Pr(|T| > |t|) = 0.7230 Pr(T>t) = 0.3615	z = -0.018 Pr > |z| = 0.9858 Pr(CH2<CH3) = 0.499	D = 0.2388 Exact p-value = 0.181
Treatment CH2 = Treatment CH3	t = -0.3692 Pr(|T| > |t|) = 0.7132 Pr(T<t) = 0.3566	z = -0.460 Pr > |z| = 0.6459 Pr(CH2<CH3) = 0.469	D = 0.938 Exact p-value = 0.999

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

How does the Private Property Trust Game compare to the Common Property Trust Game in the sequential move protocol treatments only? Table 22 conducts parametric and non-parametric means comparison tests to see if any of the means are significantly different. All tests fail to reject the null that all treatments have similar mean amounts sent (left). In Table 22, the Komolgorov-Smirnov test concludes that no distribution is significantly greater than another in any treatment comparison. There appears to be no difference in the central tendency of tokens sent (left) by Type X players.

Table 23 shows that the standard deviation of the Common Property Trust Game (4.71) is significantly greater than the standard deviation of the Private Property Trust Game (3.45) using a standard deviation ratio F-test (one-sided p-value = 0.0384). Table 24 shows the proportions tests of Type X decisions comparing Private Property Trust Game data and Common Property Trust Game data. There are significantly greater

proportions of 0 and 10 tokens left in the Common Property Trust Game. There are significantly greater proportions of 4, 5, and 6 sent (left) in the Private Property Trust Game, although there are fewer observations in the middle for comparison.

Table 23. Variance Tests for Treatment Differences in Type X Decisions

Null Hypothesis	Standard Deviation Ratio Test
S.D.(Treatment CH1)/S.D.(Treatment CH3) =1	f = 0.5208 2*Pr(F < f) = 0.0889 Pr(F < f) = 0.0444*
S.D.(Treatment CH1)/S.D.(Treatment CH2) =1	f = 0.5389 2*Pr(F < f) = 0.1065 Pr(F < f) = 0.0533
S.D.(Treatment CH1)/S.D.(Treatments CH2 and CH3 pooled) =1	f = 0.5370 2*Pr(F < f) = 0.0767 Pr(F < f) = 0.0384*
S.D.(Treatment CH2)/S.D.(Treatment CH3) =1	f = 1.0347 2*Pr(F > f) = 0.9250 Pr(F > f) = 0.5375

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Table 24. Proportions Tests of Type X Decisions

Type X sent (left)	Private Property Trust Game	N	Common Property Trust Game	one-sided p-value	N
0	0.1786	5	0.390625	0.0229*	25
1	0.0357	1	…	0.0642	0
2	0.0357	1	0.0625	0.301	4
3	0.1071	3	0.03125	0.0698	2
4	0.1429	4	0.015625	0.0066**	1
5	0.1429	4	0.03125	0.023*	2
6	0.1071	3	0.015625	0.0238*	1
7	0.0357	1	…	0.0642	0
8	...	0	…	…	0
9	…	0	…	…	0
10	0.2143	6	0.453125	0.015*	29

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

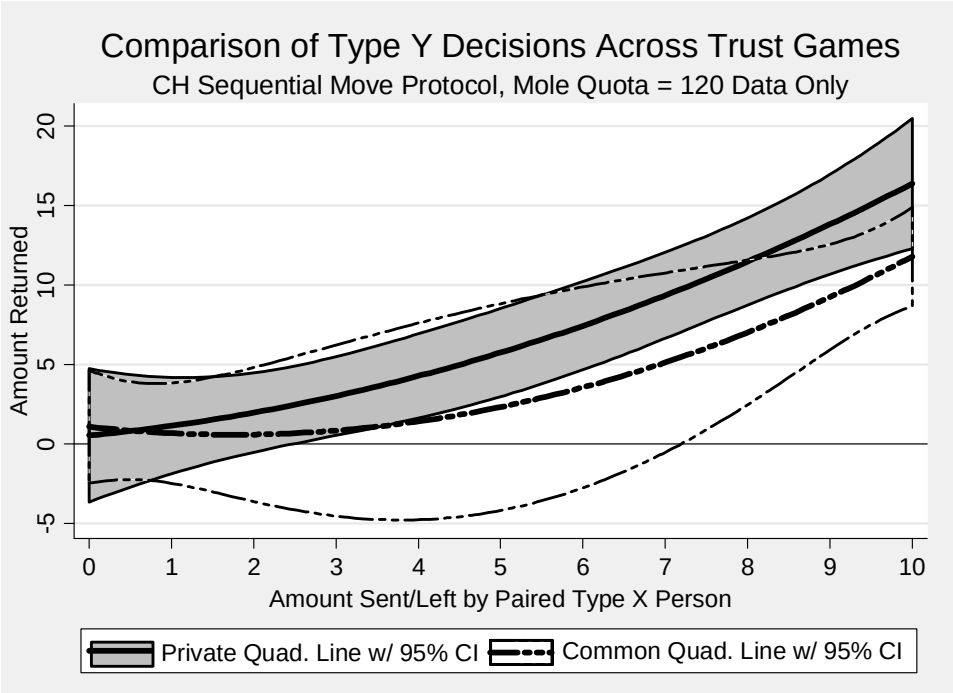


Figure 28. Type Y Decisions for CH1 and CH2

Figure 28 Compares Type Y Decisions across trust games when mole-field quota is the same at 120. Quadratic fit lines are mapped with a 95% confidence interval for each treatment. One can see that there is some evidence of less reciprocity in the Common Property Trust Game Treatment CH2. However the evidence is weak because the 95% confidence interval in the Common Property Trust Game overlaps the Private Property Trust Game to a large degree. A large portion of the bottom half of the confidence interval for CH2 lies below a Type Y player's feasible set (they cannot return a negative amount). The confidence interval bulges in the middle because there are so few Type Y choices due to the lack of Type X choices made in the middle. To get a better idea of how Type Y is responding to Type X decisions in the middle we pooled the data for CH2 and CH3 in Figure 29. The result is a similar story, but with a tighter confidence interval for the Common Property quadratic line.

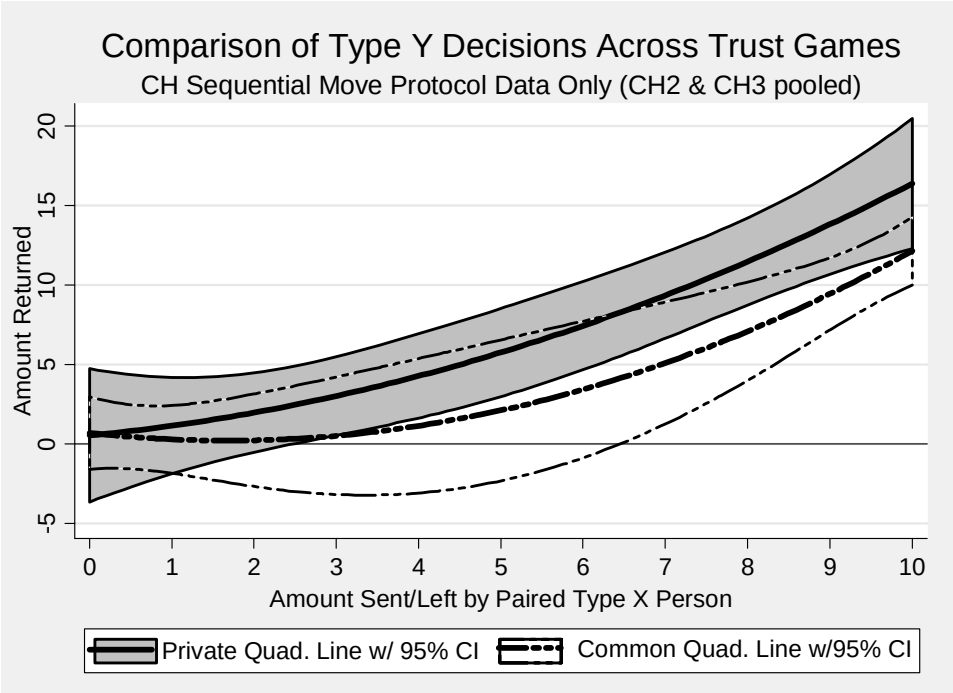


Figure 29. Type Y Decisions for CH1 and CH2 & CH3 (pooled)

Table 23 shows three Tobit regressions of Type Y return decisions as the dependent variable looking only at data from Treatments CH1, CH2, and CH3. The independent variables include the Type X Sent (Left) variable and Common Property Trust Game dummy variable. Using tobit analysis, we cannot say that subjects return significantly less in the Common Property Trust Game because the dummy variable comes up insignificant in all three regressions. The lack of significance is likely coming from differences in the distribution of Type X decisions between the Private and Common-Property Trust games. Roughly 1/3 of all Type X decisions lie between withdraw 0 and 10 tokens for the Common Property Trust Game compared to 2/3 of all Type X decisions that lie between send 0 and 10 tokens in the Private Property Trust Game. The modal Type Y response to 0 tokens sent (10 tokens withdrawn) is to return (leave) $0, resulting in $10 dollars for each player. Very few deviations from return

(leave) $0 occur in either game. When 10 tokens are sent in the Private Property Trust Game, the average return is $16.67 (8.16 standard deviation), and when 0 tokens are withdrawn in the Common Property Trust Game the average return is $12.14 (8.23 standard deviation). The mean in the Private Property Trust Game is not significantly different (t-test one-sided p-value = 0.1140), which is likely due to the small number of Type X decisions to send all 10, 6 cases, compared to 29 cases of 0 tokens withdrawn in the Common Property Trust Game. If the number of cases were increased from 6 to 14, holding the mean and standard deviation constant, the difference would become significant (t-test one-sided p-value = 0.0489).

Table 25. Tobit Regressions of Type Y Decisions

Regression	(1)	(2)	(3)
Data Set	Cox Hall Treatments CH1, CH2, and CH3 Data	Cox Hall Treatments CH1 and CH2 Data	Cox Hall Treatments CH1 and CH3 Data
N	92	60	60
Constant	-4.108	-3.861	-3.352
	(.0667)	(.1117)	(.0937)
Type X Tokens Sent (Left)	1.799	1.753	1.727
	(.0000)***	(.0000)***	(.0001)***
Common Property Trust	-1.869	-1.995	-2.380
Game Dummy	(.4914)	(.4469)	(.3592)
240 Mole Field-Quota	-1.396	…	…
	(.5266)		
Heteroscedasticity: Tripled	0.017	0.014	.0313
Amount Sent	(.0705)	(.1776)	(.0349)*
Sigma: Disturbance	4.650	4.913	2.766
standard deviation	(.0003)***	(.0007)***	(.0325)*

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

It is natural to ask why the effort task may lead to behavior more consistent with Axiom S. The real effort task creates a more emotional sense of entitlement to the endowments with which the Type X and Type Y subjects make decisions. When the Type X withdraws tokens in the Common Property Trust Game he/she destroys property that is not just jointly owned but now the Type X and Type Y players may have a stronger sense of partial entitlement to the joint fund. In other words the real effort task may create entitlements which make the property right assignments salient enough to bring Axiom S preferences out of latency. The entitlements are apparently salient enough for the Type X person to think about and recognize the possibility of Axiom S as well. As a result, the Type X person responds by withdrawing none or all 10 tokens from the joint decision fund based on the level of trust he/she has in the paired Type Y player. Due to lack of difference between Treatment CH3, which had a higher mole-field quota, and Treatment CH2, perhaps even lower effort tasks can be used to add saliency.

The strongest evidence that the common and private property game pairs are not isomorphic comes from the Type X decisions. In the common property treatments 2/3 of Type X decisions were to withdraw 0 or all 10 tokens, that is, to fully trust or completely withdraw from trusting the Type Y player. One possible interpretation is that the Type X person has some probability in his/her head that he/she is paired with a trustworthy Type Y player with reciprocal preferences consistent with Axiom R. If the Type X player is playing the Private Property Trust Game, he/she may choose to send an amount consistent with his/her level of trust (or perceived probability of being matched with a trustworthy Type Y player). In the Common Property Trust Game the Type X player must also wonder if the Type Y person has preferences consistent with Axiom R and

Axiom S. Suddenly, the act of withdrawing tokens out of partial trust is not a good strategy if the paired Type Y with strong Axiom S preferences may refuse to reward a less but not least generous opportunity set. In that case, the Type X person may decide to leave all (withdraw all) tokens if his or her trust is high (low). If the Type X player is playing the Common Property Trust Game, he/she may not be able to choose an amount consistent with his/her level of trust. One way to confirm that these Type X beliefs are credible is to elicit all potential Type Y responses using the strategy method protocol.

Adding the Strategy Method Protocol

The difficulty in testing Axiom S using the more traditional sequential move protocol is that only one Type Y decision is made, and the potential responses to other opportunity sets the Type X could have offered are not observed. This is not a big problem if Type X decisions are evenly distributed for each game, but Type X decisions are clustered at the extremes in the Common Property Trust Game. This makes a direct test of Axiom S under the sequential move protocol require a very large sample. The strategy method protocol offers the benefit of making all potential responses observable. It does this by asking a Type Y player to submit a planned response for each possible decision by a Type X player. This provides a cheaper means for testing Axiom S across the games.

There are some potential costs to using the strategy method protocol. First is the reduction in the saliency of payoffs for Type Y players. Type Y players now have to make multiple potentially binding responses, yet only one decision determines their payoffs in the end. Their decision-making costs increase, but their expected rewards do not. There is also a potential “hot” versus “cold” effect. A Type Y response in the

sequential move protocol is considered “hot” because it is potentially more emotional for the Type Y player to learn the Type X player’s decision, and how the decision affects their opportunities, before responding. The strategy method protocol is considered “cold” because Type Y is submitting a planned response and does not know the Type X decision beforehand. For sequential games designed to measure social behavior there is mixed evidence on the significance of hot versus cold responses. Three studies do not find a hot versus cold effect (Brandts and Charness 2000; Cason and Mui 1998; Oxoby and McLeish 2004), while two studies do find an effect (Brosig, Weimann, and Yang 2003; Casari and Cason 2009).

Casari and Cason (2009) do find that the strategy method protocol significantly lowers trustworthy behavior in a trust game. Their trust game differs from the Private Property Trust Game in that the multiplier is 5 instead of 3, and the Type X decision is binary to send all or none of the $10 endowment. Their design is ideal for testing for a hot versus cold effect in the trust game because it minimizes the reductions in saliency by reducing the number of Type Y decisions in the strategy method protocol to one. However, their design is not ideal for testing isomorphism between the Private and Common Property Trust Games because Type Y responses to Type X decisions in the middle help tell the story. Despite the potential drawbacks of employing the strategy method protocol, it is the only way we can cheaply check to see if Type X decisions are indeed an effect of Type X players correctly anticipating Type Y responses consistent with Axiom S.

Cox and Hall Strategy Method Protocol Treatments CH4 and CH5

Treatments CH1, CH2, and CH3 used the sequential move protocol to elicit decisions in the Private and Common Property Trust Games. For Treatments CH4 and CH5 we employ the strategy method protocol to elicit Type Y responses. The strategy method protocol differs from the sequential move protocol in these games that the Type Y players have to submit 11 potentially binding decisions before learning of the Type X player's actual decision. The Whack-A-Mole game is still played at the beginning with a 120 mole-field quota for both strategy method protocol treatments. Once a Type X and Type Y pair make their decisions, the Type X decision makes the associated Type Y response to the decision binding and the game is played out to calculate the final earnings. Figure 30 shows a screen shot of the Type Y player's decision sheet for the Private Property Trust Game with the strategy method protocol. The rows are organized by the Type X person's potential actions in Column A, with the first row representing the status quo. The decision sheet for the Common Property Trust Game is identical except the Type X player withdraws rather than contributing so the value of Column B decreases from $40 to $10.

Column A	Column B	Column C	Column D
If the Paired Type X Person Sends	Then the Total Fund Value is	Amount You Wish to Send to the Type X Person	Amount You Wish to Keep
0	$10.00	$X.XX	$Y.YY
1	$13.00	$X.XX	$Y.YY
2	$16.00	$X.XX	$Y.YY
3	$19.00	$X.XX	$Y.YY
4	$22.00	$X.XX	$Y.YY
5	$25.00	$X.XX	$Y.YY
6	$28.00	$X.XX	$Y.YY
7	$31.00	$X.XX	$Y.YY
8	$34.00	$X.XX	$Y.YY
9	$37.00	$X.XX	$Y.YY
10	$40.00	$X.XX	$Y.YY

Submit Decision Table

Figure 30. Type Y Decision Sheet for Eliciting Decisions Under the Strategy Method Protocol

126 undergraduate students from Georgia State University participated in treatments CH4 and CH5 in four sessions running from January to February in 2010. Table 26 shows the summary data for Treatment CH4, the Private Property Trust Game using the strategy method protocol. Two sessions included 32 Type X subjects and 32 Type Y subjects. In total, 32 Type X decisions were made (an average of $5.63 was sent) and 352 Type Y decisions were made (32 subjects made 11 decisions each and an average of $6.96 was returned).[23] Figure 31 shows a histogram of the Type X decisions made in Treatment CH4, the Private Property Trust Game treatment. There are modes at

[23] Note that the average Type Y decisions in Treatments CH4 and CH5 cannot be directly compared to the average Type Y decisions in the other treatments which use the sequential move protocol. First, Type Y players submit 11 decisions instead of one in the strategy method protocol. Second, Type Y decisions correspond to actual Type X decisions in the sequential move protocol but not in the strategy method protocol.

0, 3, and 10 tokens sent. Here we have a W-shaped distribution with a fat right tail at 10 tokens sent.

Table 26. Subject Decisions for CH4 Treatment

2-Person Private Property Trust Game

N-Pairs=32 (352 Y Decisions)	Mean	Std. Dev.	Minimum	Maximum
Tokens X Sent	5.63	3.94	0	10
$ Y Returned	6.96	6.25	0	20
$ X Earned	12.72	5.49	0	20
$Y Earned	18.53	8.87	0	40

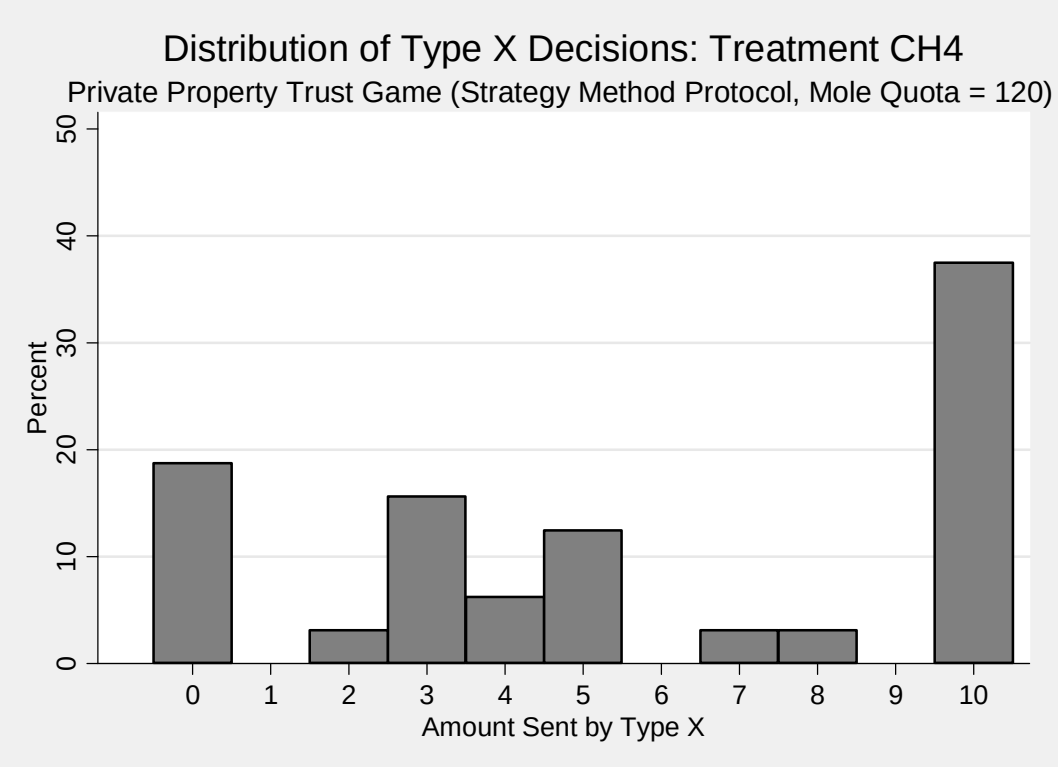


Figure 31. Type X Decisions for CH4, Private Property Trust Game

Table 27. Subject Decisions for CH5 Treatment

2-Person Common Property Trust Game

N-Pairs = 31 (341 Y Decisions)	Mean	Std. Dev.	Minimum	Maximum
Tokens X Left	7.26	3.64	0	10
$ Y Returned	5.82	6.70	0	25
$ X Earned	12.84	6.49	0	21
$Y Earned	21.68	7.56	10	40

Table 27 shows the summary data for the CH5 treatment in the Common Property Trust Game. The Common Property Trust Game sessions included 31 Type X subjects and 31 Type Y subjects. In total, 31 Type X decisions were made (an average of $7.26 was left) and 341 Type Y decisions were made (31 subjects made 11 decisions each and an average of $5.82 was returned).Figure 32 shows a histogram of the Type X decisions made in Treatment CH5, their Common Property Trust Game treatment. There are modes at 0, 5, and 10 tokens left. Here we have a J-shaped distribution with over half of the subjects chose to withdraw 0 tokens.

How do the two games compare? Figure 33 compares the distributions of Type X decisions for the Private and Common Property Trust Games. The shapes of both distributions are quite similar, but with more weight to the right for the Common Property Trust Game. Table 28 shows the results of some standard parametric and non-parametric tests on Type X decisions. The number of dollars left in the Common Property game is greater than the amount of number of dollars sent in the Private Property Trust Game, but the difference is insignificant.

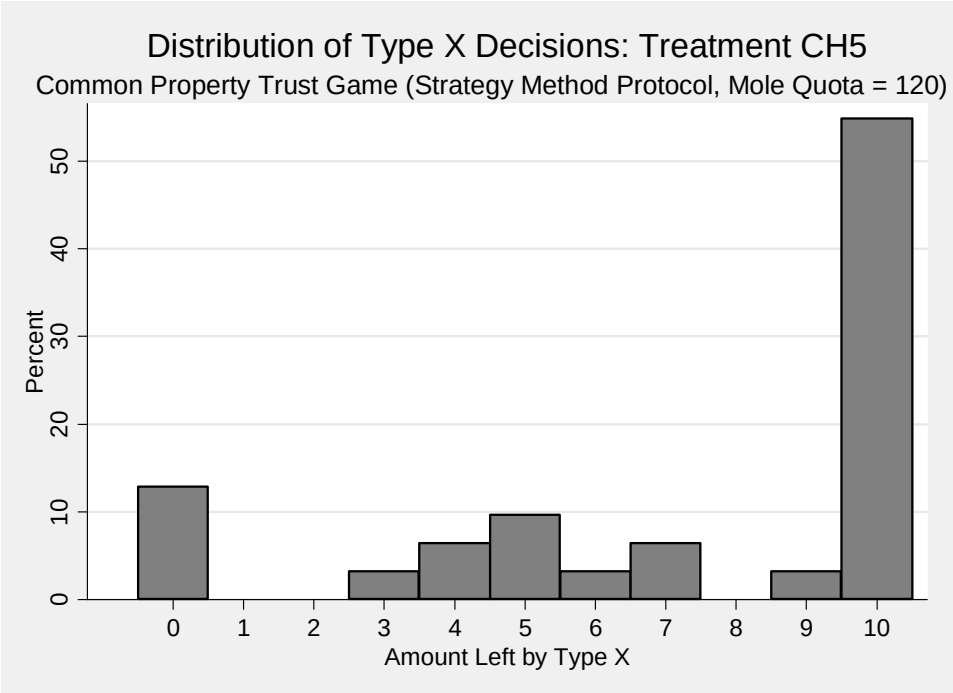


Figure 32. Type X Decisions for CH5, Common Property Trust Game

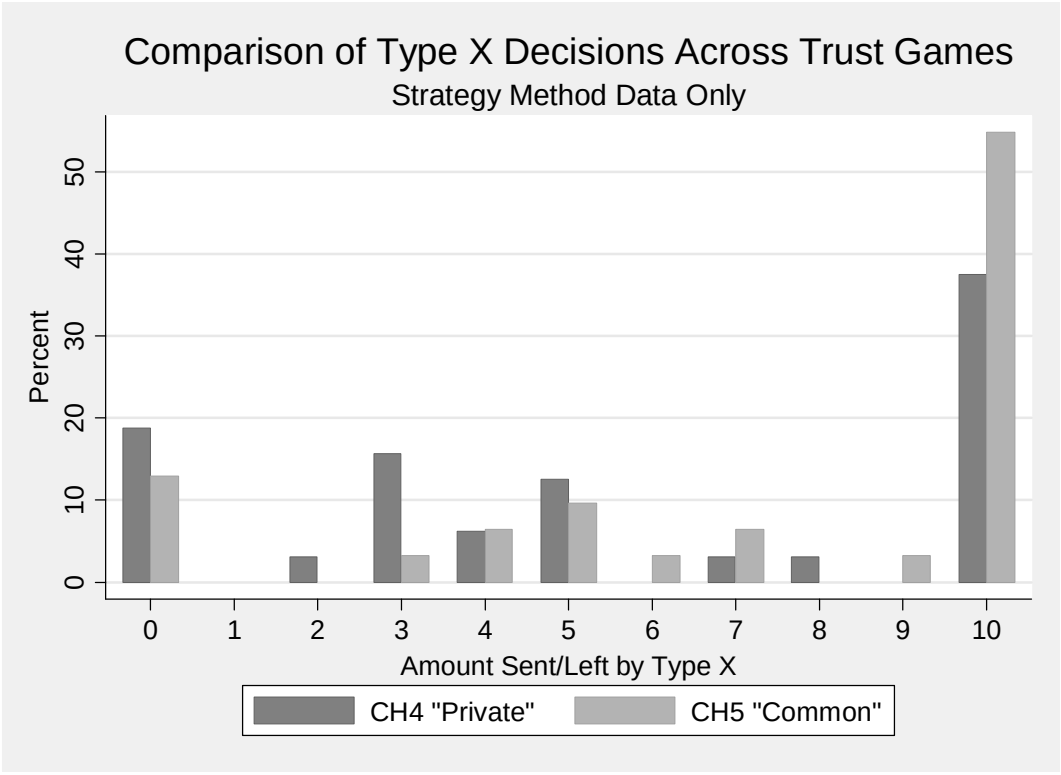


Figure 33. Comparison of Type X Decisions for CH4 and CH5

Table 28. Parametric and Non-Parametric Tests of Type X Decisions

	Parametric Tests		Nonparametric Tests	
Test	Means Test (t-test)	Variance Test (F-test)	Mann-Whitney Test (Rank Sum Test)	K-S Test
Null Hypothesis	CH4 = CH5	S.D.(CH4)/ S.D.(CH5) =1	CH4 = CH5	Distributions are Equal
Test Statistic	t = -1.7088 Pr(|T| > |t|) = 0.0924 Pr(T<t) = 0.0462*	f = 1.1710 2*Pr(F < f) = 0.6673 Pr(F <>f) = 0.3337	z = -1.692 Pr > |z| = 0.0906 Pr(CH4>CH5) = 0.383	D = 0.2399 Exact p-value = 0.256

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

There are 352 and 341 Type Y decisions made in Treatments CH4 and CH5 respectively. Figure 34 compares the Type Y decisions across trust games for the strategy method protocol. Here the quadratic fit line for Type Y decisions in the Common Property regime lie below the line of Private Property decisions. Even the top band of the 95% confidence interval of Common Property decisions lie below the bottom band of the 95% confidence interval of the Private Property decisions in the middle domain of potential Type X decisions. This is strong evidence of Axiom S leading to differences in reciprocity across Private and Common Property environments. Table 29 shows tobit regression analysis of Type Y decisions in the strategy method protocol treatments. The regression shows that there is a significant reduction in the amount returned for the Common Property Trust Game under the strategy method protocol.

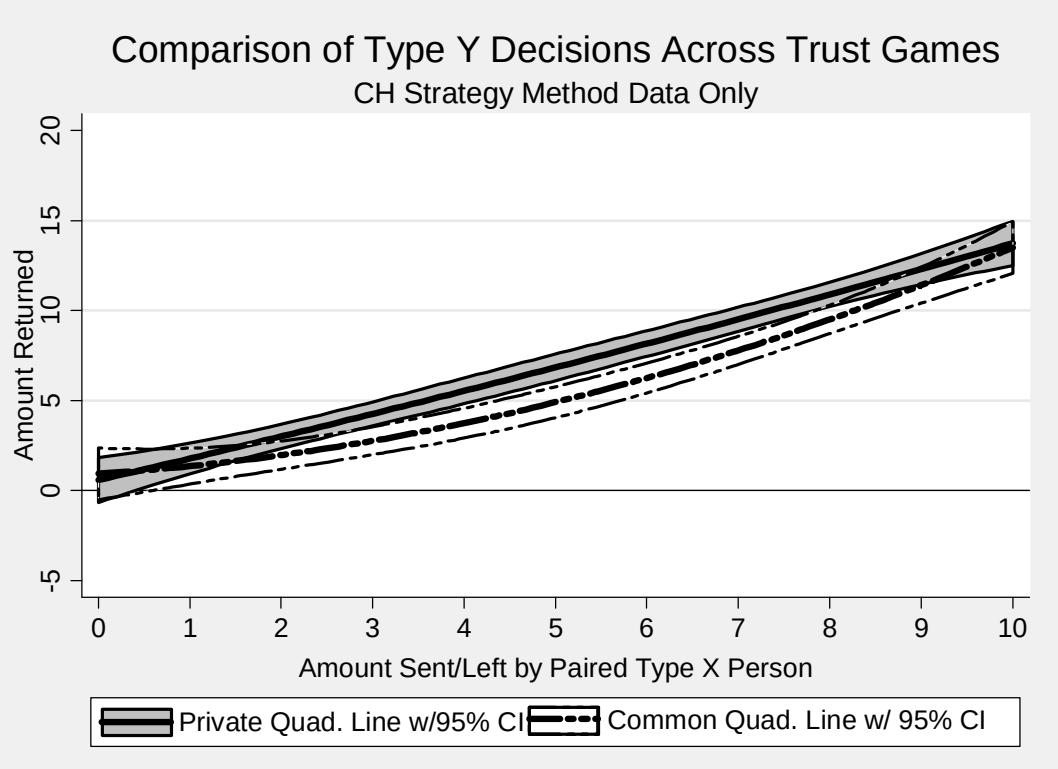


Figure 34. Type Y Decisions for CH4 and CH5

Since every Type Y player makes the same decision under the strategy method protocol a t-test can also be conducted on the amounts returned, and significantly less is returned under this test as well (p-value = 0.0104). Four Type Y subjects took Axiom S to the extreme in treatment CH5: They split equally the remaining joint decision fund if zero tokens were withdrawn and split $0.00 to the Type X player if they withdrew any tokens. If Type X players are risk averse enough to prepare for the possibility of facing Type Y players like these, then they are responding optimally by withdrawing 0 or the maximum of 10 tokens.

Table 29. Tobit Analysis of Type Y Decisions in Treatments CH4 and CH5

Number of Obs.	693
Constant Term	-0.789
	(.0634)
Type X Sent (Left)	1.384
	(.0000)***
Common Property Dummy	-1.847
	(.0001)***
Heteroscedasticity Term	0.0359
	(.0000)***
Sigma: Disturbance of standard deviation	2.360
	(.0000)***

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Testing for Saliency Effects

We define saliency in this experiment on two dimensions: saliency of property ownership and saliency of payoff outcomes. Reductions in saliency can be accomplished by removing the real effort task to reduce property right entitlements or by switching from the sequential move protocol to the strategy method protocol. Removing the real effort task may lead some subjects to interpret their private or common property endowments as the experimenter's money rather than their own. When does a subject feel ownership of the money they are making decisions with? Is it upon receiving the endowment or upon exiting the laboratory with the cash in hand? Having the subject earn their property before making decisions increases the saliency property ownership and allows for a more appropriate test of differences between private and common property. The strategy method protocol requires Type Y subjects to make 11 decisions instead of one, yet only one decision will determine earnings and it is less salient to the subject which decision will count towards their earnings.

What are the effects of changing the saliency within the Private Property Trust Game? Figure 35 compares Type X decisions in the Private Property Trust Game. The most salient treatment is CH1 because subjects have to earn the right to play and the sequential move protocol is used. Reducing saliency can be accomplished by removing the effort task as in COW1 or by switching to the strategy method protocol as in CH4. Either reduction in saliency results in more generous Type X Decisions, in particular more decisions to send all 10 tokens are made.

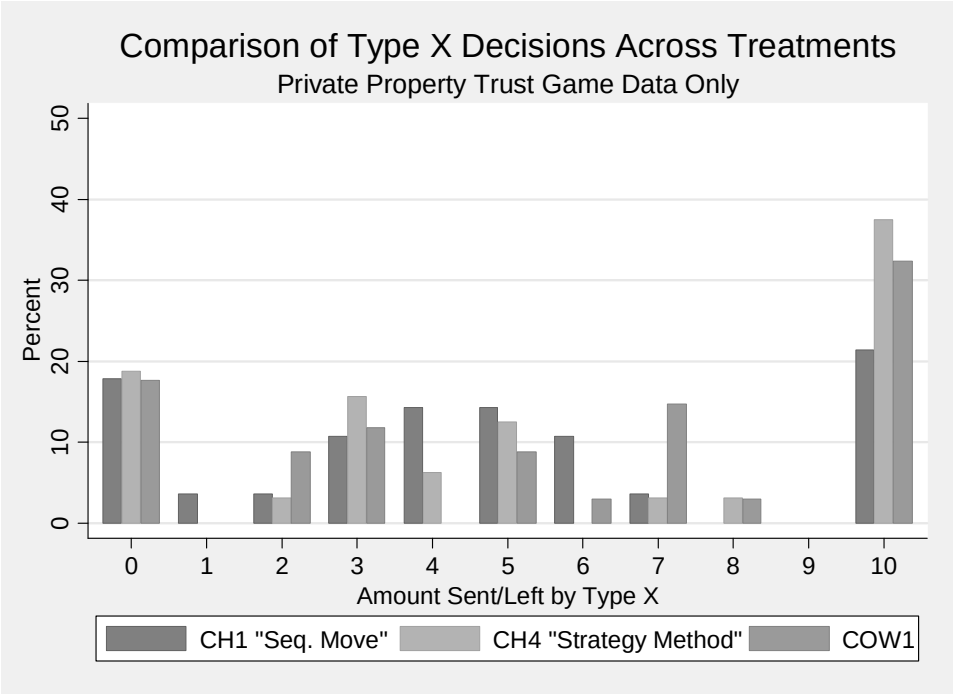


Figure 35. Comparison of Type X Decisions in the Private Property Trust Game

What are the effects of changing the saliency within the Common Property Trust Game? Figure 36 compares Type X decisions in the Common Property Trust Game. The most salient treatments are CH2 and CH3 because subjects have to earn the right to play and the traditional sequential move protocol is used. Reducing saliency can be

accomplished by removing the effort task as in COW2 or by switching to the strategy method protocol as in CH5. Either reduction in saliency results in more generous Type X Decisions, in particular more decisions to withdraw 0 tokens are made. What is also interesting is how the distributions of Type X decisions for Treatments CH2 and CH3 are U-shaped and are J-shaped distributions for Treatments CH5 and COW2.

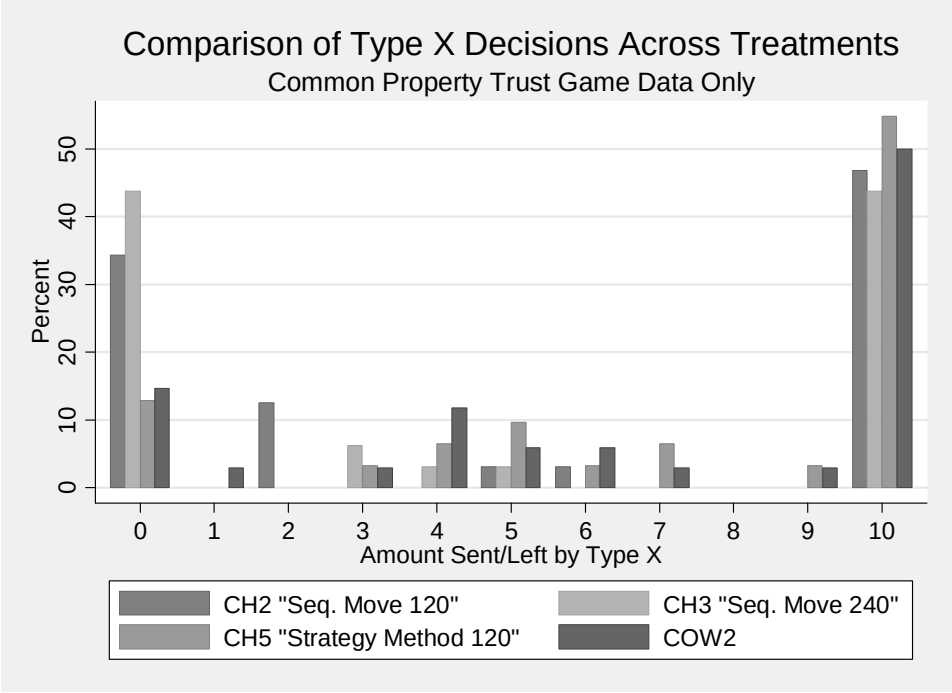


Figure 36. Comparison of Type X Decisions in the Common Property Trust Game

Comparing the COW1 and COW2 treatments with the CH1, CH2, and CH3 treatments the lower amount sent and left in this study for both Private and Common Property data could come from subject-pool effects, weaker entitlement effects, or a combination thereof. The COW sessions were conducted at the University of Indiana, where 54% of the participants were male. The sessions for this study were conducted at Georgia State University, where only 38% of the participants were male. Several studies

have shown that men send more than women in similar trust games but under a private property setting, either because they trust more or are more risk loving (Chaudhuri and Gangadharan 2007; Vyrastekova and Onderstal 2005). Still evidence of people becoming more risk loving with house money can also explain the lower amounts sent (left) in this study (Arkes et al. 1994; Battalio, Kagel, and Jiranyakul 1990; Keasey and Moon 1996; Thaler 1990; Thaler and Johnson 1990). Since these are different studies, we shall make no guess as to which effect is stronger in lowering the average amount sent (left) in the sequential move treatments for this experiment. However, lower saliency through lack of entitlements is a likely culprit because we get a similar effect when saliency is reduced by switching to the strategy method protocol.

Figures 37 and 38 compare the effects of saliency on Type Y decisions in the Private Property Trust Game. Less generosity appears to be found in the COW1 treatment when compared with the CH1 treatment. The effect of switching to the strategy method protocol is less clear because there appears to be relatively greater (lower) returns when the amounts sent are low (high) for treatment CH4. Figures 39 and 40 compare the effects of saliency on Type Y decisions in the Common Property Trust Game. There appears to be more generosity in the COW2 treatment compared to the CH2 treatment. The effect of switching to the strategy method protocol also results in greater amounts left to the Type X player.

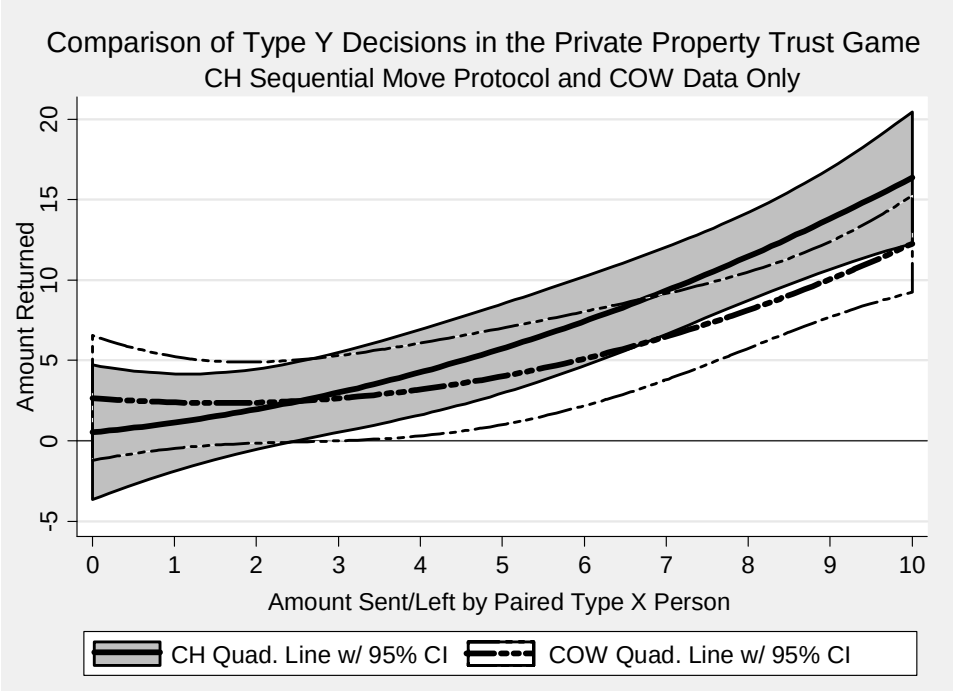


Figure 37. Effect of Real Effort Task on Type Y Decisions in the Private Property Trust Game

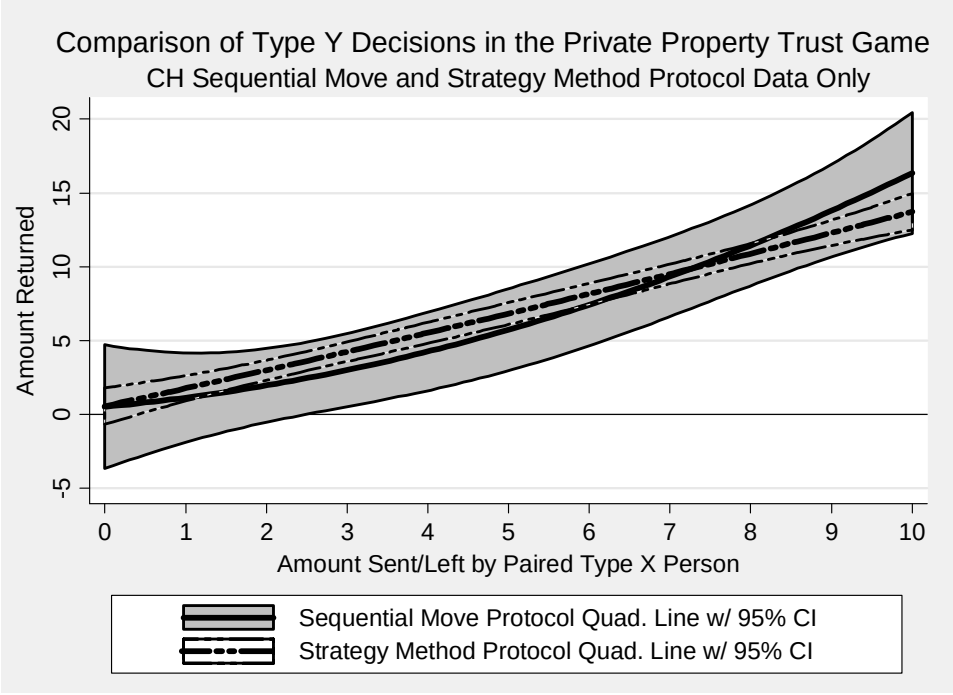


Figure 38. Effect of Strategy Method Protocol on Type Y Decisions for Private Property Trust Game

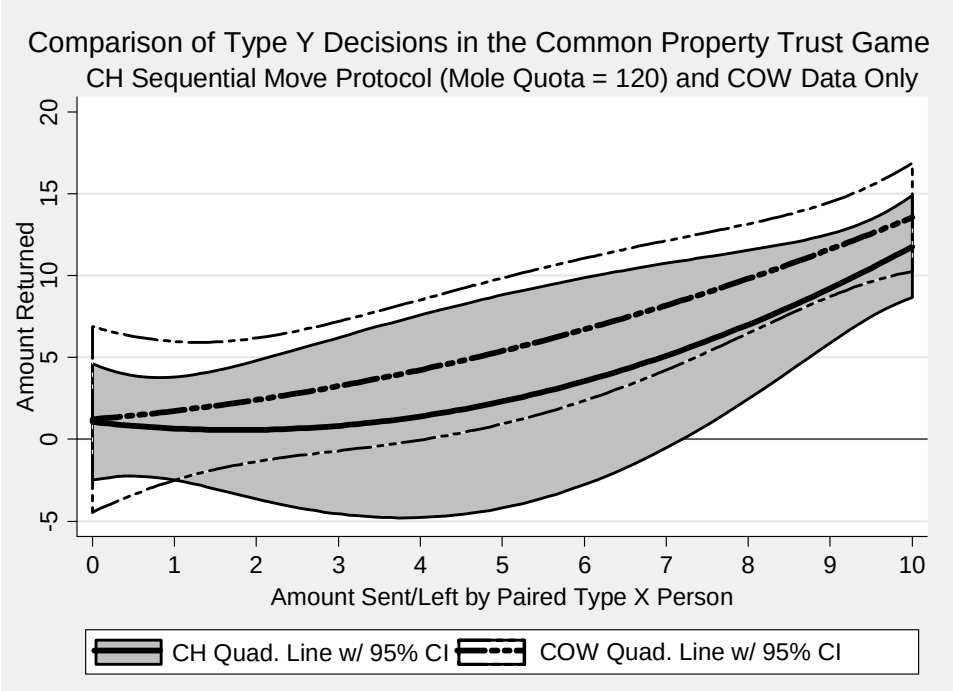


Figure 39. Effect of Real Effort Task on Type Y Decisions for Common Property Trust Game

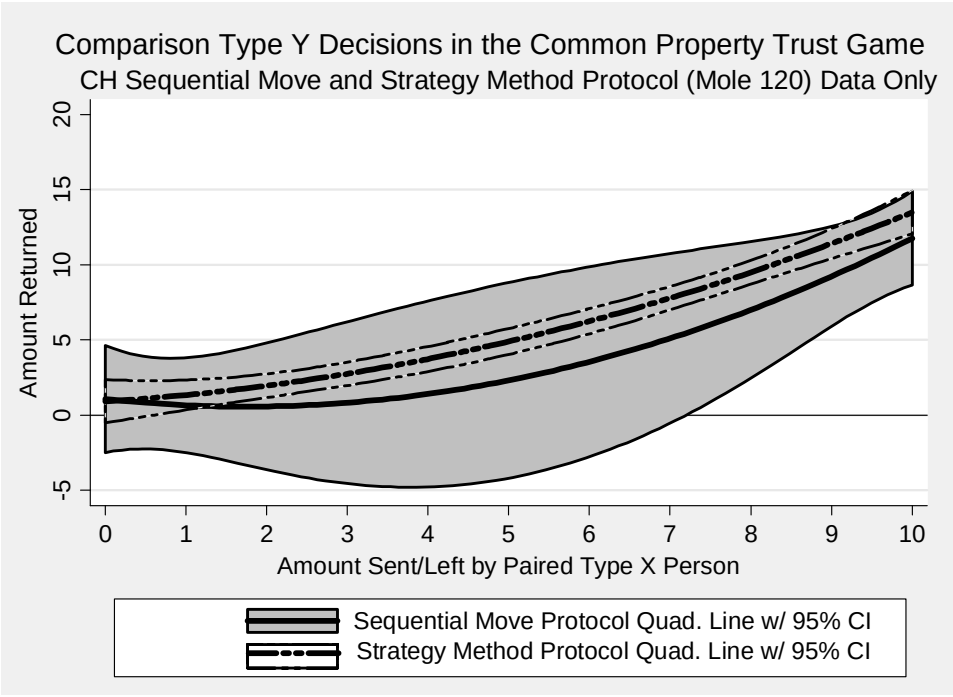


Figure 40. Effect of Strategy Method Protocol on Type Y Decisions for Common Property Trust Game

Tobit analysis is conducted using Type Y decisions as the dependent variable. The independent variables include the Type X Sent (Left), a dummy variable for the COW study and a dummy variable for the strategy method protocol treatments. Table 30 shows five tobit regressions used to test for saliency effects. The first regression pools all of the data and finds that reducing saliency increases Type Y generosity whether by removing stronger entitlements in the COW study or by switching to the strategy method protocol in Treatments CH4 and CH5. The COW and Strategy method protocol dummy variables are only near-significant however. The results hold when we only look at non-strategy data in the second regression, CH data only in the third, Private Property data only in the fourth, and Common Property data in the fifth. These findings run contrary to Casari and Cason (2009), who found that the strategy method protocol reduces Type Y decisions, but we may be comparing apples to oranges. They analyzed Type Y responses to being offered the most generous opportunity set so we will as well.

Table 31 shows the Type Y average responses to 10 tokens sent (0 withdrawn) by a Type X player. Here we get Type Y responses similar to Casari and Cason (2009) in the Private Property Trust Game but responses go in the opposite direction for the Common Property Trust Game. These effects are not significant, which is likely due to the small sample size. These effects however do show how saliency may affect the strength of Axiom S in each trust game.

Table 30. Tobit Regressions of Type Y Decisions

Regression	(1)	(2)	(3)	(4)	(5)
Data Set	All Data	Cox Hall Sequential move protocol and COW Data	Cox Hall Data	Private Property Trust Game Data Only	Common Property Trust Game Data Only
N	852	159	60	414	438
Constant	-2.118 (.0099)	-3.922 (.0227)	-2.031 (.0127)	-1.843 (.0858)	-5.115 (.0008)
Type X Tokens Sent (Left)	1.412 (.0000)***	1.612 (.0000)***	1.422 (.0000)***	1.396 (.0000)***	1.506 (.0000)
Common Property Dummy	-1.807 (.0000)***	-1.441 (.2965)	-1.872 (.0001)***	…	…
COW Dummy	1.247 (.1755)	1.233 (.3535)	…	0.823 (.5178)	2.237 (.2383)
Strategy Method Dummy	1.108 (.1307)	…	1.058 (.1453)	1.164 (.2583)	1.276 (.3296)
Heterosceda sticity Term	0.031 (.0000)***	0.017 (.0112)**	.0332 (.0000)***	.0346 (.0000)***	.0252 (.0000)***
Sigma: Disturbance Std. Dev.	2.730 (.0000)***	4.660 (.0000)***	2.576 (.0000)***	2.118 (.0000)***	3.828 (.0000)***

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Table 31. Type Y Average Responses to 10 Sent (0 Withdrawn)

Private Property	Avg. Return if 10 Sent	N	t-test one-sided p-value	Common Property	Avg. Return if 0 Withdrawn	N	t-test one-sided p-value
CH1	16.67	6	base	CH2	11.80	15	base
CH4	13.80	32	0.2247	CH3	12.50	14	0.4121
COW1	12.73	11	0.1661	CH5	14.65	31	0.1370
				COW2	14.06	16	0.2165

Survey Analysis

Subjects in all treatments were paid $5 to fill out a survey, which can be found in Appendix F. The survey provides demographic information such as the age and gender of the decision maker. The questionnaire also asks three questions related to subjects' social attitudes. The three questions are:

1. *Generally speaking, would you say that most people can be trusted or that you can't be too careful dealing with people?*

   Answers: Can't be too careful, Most people can be trusted, Don't know

2. *Would you say that most of the time people try to be helpful or that they are mostly just looking out for themselves?*

   Answers: Just look out for themselves, Try to be helpful, Don't know

3. *Do you think that most people would take advantage of you if they got a chance or would they try to be fair?*

   Answers: Most people would take advantage, They would try to be fair, Don't know

From each question two dummy variables are created as seen in Tables 30 and 31. The "Trust Pos" dummy variable codes "Most people can be trusted" as 1, and the "Trust Null" Dummy variable codes "Don't know" as 1.

Table 32 displays tobit regressions of Type X decisions using the survey variables described above. The tobit lower and upper limits were set at 0 and 10. The first regression looks at all of the data. The Common Property Trust Game Dummy has a positive and significant effect on Type X decisions. In terms of saliency, the strategy method protocol dummy variable also has a positive and significant effect on the amounts sent. The COW dummy variable has a positive yet only near significant effect. It is fairly

intuitive that a non-negative response to the trust and fairness question has a positive and significant effect on the amount sent or left. Regressions (2) and (3) look at only one trust game at a time to see if the results are robust to each game. The effect of the strategy method protocol on Type X decisions is stronger in the Common Property Trust Game. Interestingly a non-negative response to the trust question has a stronger effect on the amount left in the Common Property Trust Game than on the amount sent in the Private Property Trust Game. Responses to the fairness and helpful questions did a relatively better job than trust in explaining Type X decisions in the Private Property Trust Game. This finding suggests that there is a greater need for trust in the Common Property Trust Game, and that perceptions of fairness are relatively more important in the Private Property Trust Game.

Table 32. Tobit Regressions of Type X Decision with Questionnaire Analysis

Regression	(1)	(2)	(3)
Data Set	All Data	Private Property Trust Game Data	Common Property Trust Game Data
N	220	92	128
Constant	-12.042	-24.018	-11.275
	(.0566)	(.0102)	(.2447)
Common Property	2.725	…	…
Trust Game Dummy	(.0489)*		
COW Study	2.187	-0.190	4.185
Dummy	(.1801)	(.9006)	(.2098)
Strategy Method	5.153	2.043	11.002
Dummy	(.0027)**	(.2012)	(.0022)**
Male Gender	1.798	2.391	2.340
Dummy	(.1916)	(.0628)	(.3932)
Age	0.520	1.279	0.426
	(.0792)	(.0053)**	(.3287)
Trust Pos Dummy	6.831	2.590	12.996
	(.0006)***	(.1272)	(.0043)**
Trust Null Dummy	3.638	-0.083	9.703
	(.1068)	(.9650)	(.0658)
Helpful Pos Dummy	0.767	-1.223	3.738
	(.6422)	(.4135)	(.2721)
Helpful Null	-4.465	-4.424	-4.790
Dummy	(.0782)*	(.0518)*	(.3744)
Fair Pos Dummy	7.117	7.828	6.951
	(.0001)***	(.0000)***	(.0638)
Fair Nul Dummy	3.940	1.953	5.199
	(.0412)**	(.3146)	(.1513)
Sigma Disturbance:	8.595	5.217	12.209
Standard Deviation	(.0000)***	(.0000)***	(.0000)***

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Table 33 displays tobit regressions of Type Y decisions using the questionnaire variables described above.[24] The tobit lower and upper limits were set at 0 and the Type Y Fund (remaining Joint Fund). The first regression looks at all of the data. The Common Property Trust Game Dummy still has a negative and significant effect on Type Y decisions. In terms of saliency, the strategy method protocol dummy variable still has a positive effect on the amounts returned and becomes significant when the questionnaire variables are added. A positive response to the trust and fairness question has a positive and significant effect on the amount sent or left. All three regressions show that men are significantly less trustworthy than women. Regressions (2) and (3) look at only one trust game at a time to see if the results are robust to each game. Age has an economically and statistically significant negative effect on Type Y Decisions in the Common Property Trust Game. This suggests that older students may respond more strongly to deviations from the status quo. The ages of the four Type Y players who returned 20 if 0 is withdrawn and returned 0 otherwise are is 21, 21, 27, and 32 while the average age is 20.93 for Type Y players in treatment CH5. In the Common Property Trust Game regression a non-negative response to the Helpful question has a strong effect on increasing Type Y decisions. Perhaps people who believe others "try to be helpful" are extra generous to those who confirm their beliefs and decide not to destroy their commonly owned property. From the Trust question we find that the Trust Pos Dummy variable has a stronger effect in the Private Property Trust Game, and the Trust Null Dummy variable has a weaker effect in the Common Property Trust Game.

[24] The Fair Pos Dummy and Fair Null Dummy variables were dropped because the LimDep tobit regression limits the number of explanatory variables to 13 and they had less explanatory power than the Trust and Helpful Variables.

Table 33. Tobit Regressions of Type Y Decision with Questionnaire Analysis

Regression	(1)	(2)	(3)
Data Set	All Data	Private Property Trust Game Data	Common Property Trust Game Data
N	838	403	435
Constant	1.241	0.703	14.637
	(.4624)	(.7435)	(.0007)
Tokens Type X Sent	1.435	(1.3790)	1.644
	(.0000)***	(.0000)***	(.0000)***
COW Study	1.704	1.258	1.145
	(.0919)	(.3028)	(.5005)
Common Property	-2.21	…	…
	(.0000)***		
Strategy method protocol	1.885	0.986	2.571
	(.0282)*	(.3349)	(.0598)
Male	-2.538	-2.954	-1.876
	(.0000)***	(.0000)***	(.0284)*
Age	-0.167	-0.068	-1.081
	(.0096)**	(.4289)	(.0000)***
Trust Pos Dummy	1.898	2.360	1.402
	(.0073)**	(.0002)***	(.3914)
Trust Null Dummy	-1.050	0.452	-4.192
	(.1730)	(.5345)	(.0313)*
Helpful Pos Dummy	0.654	-0.046	5.259
	(.2651)	(.9289)	(.0017)**
Helpful Null Dummy	3.075	-1.024	5.742
	(.0009)***	(.5919)	(.0001)***
Heteroscedasticity Term	0.029	0.037	0.016
	(.0000)***	(.0000)***	(.0013)***
Sigma Disturbance Std. Dev.	2.758	1.872	4.520
	(.0000)***	(.0000)***	(.0000)***

*$p < 0.05$ **$p < 0.01$ ***$p < 0.001$

Conclusion

We added a real effort task where subjects had to meet a performance quota to earn the right to play in the Private Property Trust Game or the Common Property Trust Game. This was done to give the subjects a stronger sense of entitlement to their private or common property endowments, and increase the saliency of property ownership

Treatments CH1, CH2, and CH3 employ the sequential move protocol as did the COW study. Looking only at the means and tobit analysis, the Private Property Trust Game and the Common Property Trust Game seem to remain isomorphic under our design: the means comparison tests do not reject the null that the means are the same, and the tobit analysis finds the Common Property Trust Game dummy variable to be insignificant. However isomorphism breaks down when we look at the distributions of Type X decisions: Type X decisions move to the extremes of “full trust” and “no trust” in the Common Property Trust Game but not in the Private Property Trust Game. There is some weak evidence that reciprocity is lower in the Common Property Trust Game, but we observe very few Type X decisions that fall between withdrawing 0 and 10 tokens so that inferences on Type Y decisions are limited.

To reveal Type Y decisions less frequently observed in the sequential move protocol we employ the strategy method protocol in Treatments CH4 and CH5. The strategy method protocol asks Type Y subjects to submit a planned response to each possible Type X decision. The strategy method protocol treatments reveal that reciprocity is lower in the Common Property Trust Game. This finding supports Axiom S from Revealed Altruism Theory (Cox, Friedman, and Sadiraj 2008), and it justifies the extreme decisions of Type X players in the Common Property Trust Game.

We generate new evidence that suggests that the games are not isomorphic under stronger entitlements. Outside the lab, private and common property rights are more often acquired by money generated from real effort. We find evidence of Type Y preferences consistent with Axiom S; which provides some insights into the differences in the need for trust and cooperation between private property environments and common property environments. We also provide further evidence that having subjects earn their endowments is an important experimental design consideration in testing theory.

One notable finding is that Type X players move towards the extremes when stronger entitlements are added and particularly when the sequential move protocol is employed. If Type X players are responding optimally to the possibility that Type Y players have preferences consistent with Axiom R and Axiom S, then Revealed Altruism Theory may provide a useful foundation on which to build new theory to explain first mover behavior in sequential games that involve social dilemmas.

Another interesting observation is the possible hot versus cold effect appears but not in the expected direction for trust games. A Type Y player responds to generous decisions made by a Type X player, therefore the “hot” sequential move protocol should generate a more positive emotional response and more generous return. Casari and Cason (2009) find that the strategy method protocol has no effect on Type X decisions and reduces generosity in Type Y decisions (Casari and Cason 2009). Their results are in the expected direction for Type Y decisions. However, we find that the strategy method protocol has a positive and significant effect on Type Y returns when the questionnaire variables are added to the tobit regression. In the strategy method protocol the Type X players know that the Type Y players will not know their decision before making the

Type Y decision, hence they cannot directly influence a positive emotional response and therefore as high returns from the Type Y player. If a Type X player believes this then he/she should send or leave fewer tokens under the strategy method protocol. Contrary to this we find that the strategy method protocol also increases generosity in Type X decisions.

Two explanations are offered why Type X generosity increased in the strategy method treatment. However, these explanations are speculative and not testable with the data from this experiment. The first explanation is that betrayal by Type Y players may be less emotional and personal under the strategy method protocol for Type X players, thus betrayal aversion is reduced. However, this argument also implies that Type Y players would also feel less guilty about betraying Type X players and as a result would return less under the strategy method protocol. This explanation is only plausible for the Type X players.

The second explanation is that it is more difficult for the Type Y player to ignore or neglect all of the opportunity sets the Type X player could have chosen under the strategy method protocol. If the Type X offers the most generous opportunity set to the Type Y player, he/she could have offered one of ten less generous opportunity sets instead. If the Type Y player fails to recognize this, he/she may return less because they do not fully appreciate the generosity of the Type X player. We go to great efforts to make sure subjects fully understand the game, the choice set of the Type X player, and how each Type X choice affects the choice set of the Type Y player. We delay Type X or Type Y role assignment until after the instructions are read silently by the subjects, explained verbally by the experimenter, and all remaining subject questions about the

trust game are answered. We do this because we want the subjects to pay attention to each role in the game. Under the strategy method protocol, Type Y subjects must fill out a table so they cannot ignore the alternative Type X choices and must compare them. This is more likely to bring reciprocal preferences out of latency for Type Y subjects. Type X players may be more confident that Type Y players will reciprocate and, as a result, be more generous.

Cox, Ostrom, and Walker have extended their COW study of behavioral differences in private and common property regimes to include more than two players and to include multiple rounds. We extend their previous study into earned entitlements and the strategy method protocol. The earned entitlements were symmetric and independently earned. We plan on investigating whether our results are robust to asymmetric entitlements and team-earned entitlements. The strategy method protocol revealed more information about how subjects viewed the game, leaving open the investigation for other elicitation and decision making protocols.

A potential criticism of the differences between Private and Common Property experiments is that the status quo is generating the behavioral differences and that the framing of the endowment as private or common and the Type X action as sending or withdrawing has little effect. We welcome anyone interested to test this by removing the framing, or by keeping the framing and choosing payoff equivalent status quo opportunity sets.

CHAPTER V

CONCLUSION

This dissertation comprises three essays on experiments with redistribution, trust, and entitlements. The first essay reports experiments designed to assess the labor supply effects of increasing redistribution by the public sector for the purpose of providing charitable goods and services. Subjects perform a real effort task to give them a stronger sense of entitlement to their labor earnings under redistribution. The second essay looks at previous experiments to see if behavior in some standard cooperative games is robust under a role-reversal protocol that asks each subject to play all roles in the game. A new experiment is reported that addresses the question whether behavioral results in the Investment Game (a trust game) are robust to applying a role-reversal protocol. The third essay reports a theory-testing experiment designed to test for differences in trust-related behavior under different private and common property environments. Subjects perform a real effort task to give them a stronger sense of entitlement to their private or common property.

The first essay sets up an experiment designed to test how different levels of redistribution affect the size and distribution of income. Income redistribution, by transferring income from the rich to the poor, will decrease income inequality but may also reduce total income, leading to an efficiency-equality tradeoff. Focus is placed on the source of the efficiency-equality tradeoff attributed to increases in the redistribution level lowering incentives for work. Subjects are allowed to freely allocate time between labor and leisure to make responses to work incentives more observable. Income redistribution is implemented by proportionately taxing individual labor earnings and

dividing the revenue equally amongst the group members. Subjects receive feedback each period which tells them whether they are net taxpayers into redistribution or net recipients from it. Low and high tax rates are implemented in separate treatments to measure effects on efficiency, equality, and labor supply. We find a significant efficiency-equality tradeoff that can be explained by lowered work incentives. Labor supply decisions become signals of cooperation and strategic and cooperative behavior similar to behavior found in public goods experiments. The efficiency-equality tradeoff calls for a reconsideration of increasing dependence on the public sector for charity provision and reducing inequality.

The second essay looks for effects of applying a role-reversal protocol to cooperative games such as bargaining games and trust games. Under a role-reversal protocol players know in advance they will be playing all possible roles in repetitions of the same game. Some researchers use role-reversal protocols to gather more information about how decisions are made in the single-role protocol. This makes role-reversal a valuable empirical tool as long as behavior in the single-role protocol is robust to the application of role-reversal. We survey the current literature to determine the effects of role-reversal and find the evidence is mixed. Mixed evidence is due to the lack of within-study tests of role-reversal, and the lack of consensus on how to design a role-reversal protocol. We conduct an experiment comparing behavior in the Investment Game (a trust game) to behavior in its role-reversal protocol. Only one role decision is selected for payoff in the role-reversal protocol. We find evidence that paying subjects for one role instead of all roles in the role-reversal treatment leads to no significant role-reversal effect.

The third essay follows a previous study which tested behavior in the Private Property Trust Game and the Common Property Trust Game. This study looks at the effects of assigning stronger property rights to these games. Subjects' property rights are strengthened by requiring that subjects meet a performance quota in an effort task to earn their private or common property endowment. Stronger property rights lead to a divergence between decisions made in the Private and Common Property trust games not found in the previous study. In first mover behavior we find similar levels of generosity in both games and a significantly higher variance of generosity in the Common Property Trust Game. First mover decisions in the Common Property Trust Game move to the extreme decisions of "no trust" and "full trust" towards the second mover. First mover decisions in the Private Property Trust Game are similar to decisions made in the previous study. When a strategy method protocol is added to make all second mover responses observable significantly less generous behavior is observed in the Common Property Trust Game. These results provide support for Revealed Altruism Theory, which states that first mover deviations from the status quo can generate stronger reciprocal behavior in second movers (Cox, Friedman, and Sadiraj 2008). We find that stronger property right entitlements add saliency to property ownership which results in behavior consistent with Revealed Altruism Theory.

APPENDIX A

EXPERIMENT INSTRUCTIONS FOR CHAPTER II

These are the instructions used in the 80% Tax Treatment. The 20% tax treatment instructions are identical after the "20% of Your Labor Earnings are Transferred to the Group Account" replacement is made.

<INSTRUCTIONS HANDED OUT BEFORE THE DOUBLE PAY PERIOD>

No Talking Allowed

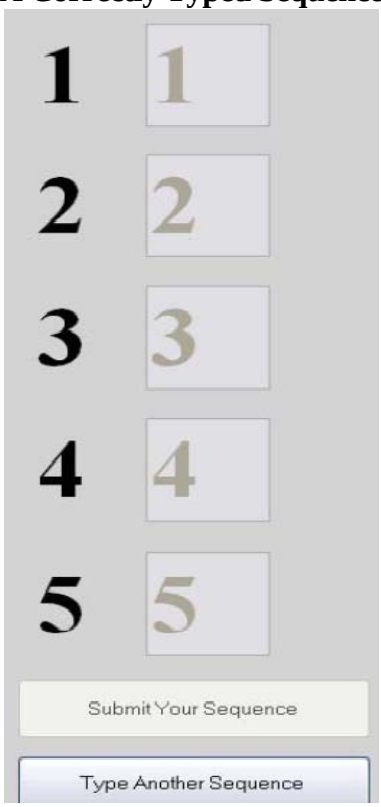
Now that the experiment has begun, we ask that you do not talk. We ask that you remain in your seat for the entire time. If you have any questions, please raise your hand and the experimenter will approach you and answer your question in private.

Your Labor Task

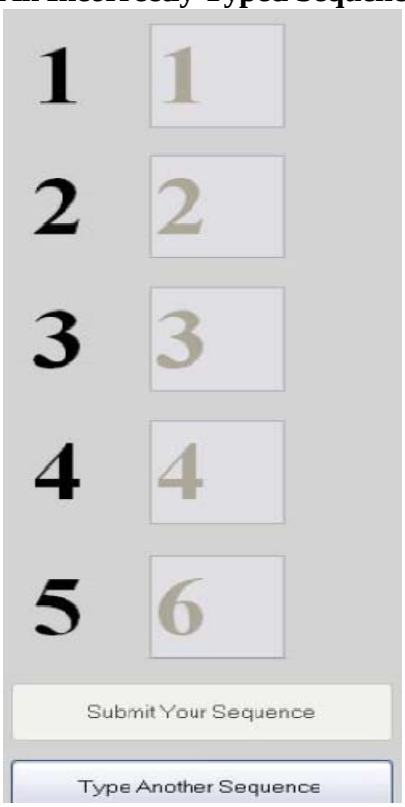
You are going to participate in a labor task. Your earnings will be determined by how many times you complete the task correctly.

Your labor task is to type a sequence of single-digit numbers exactly as they are displayed on a computer screen. Each sequence consists of five numbers, which are determined randomly. Once you have copied your entire sequence of numbers you will click the **Submit Your Sequence** button.

A Correctly Typed Sequence



An Incorrectly Typed Sequence



The figure on the left shows a correctly typed sequence, and the figure on the right shows an incorrectly typed sequence. Notice that there is no room for error as the incorrect sequence has only one incorrect number (6 is typed in the last box instead of 5).

Every time you submit a sequence correctly your earnings will increase by 6 cents.
If you enter an incorrect sequence, you will not be penalized but your earnings will not increase when you perform the task incorrectly.

After you have viewed your results, click the **Type Another Sequence** button to repeat the labor task.

Periods
There are 13 periods in this experiment. Each period lasts 5 minutes and 30 seconds. For the last 30 seconds of each period, you cannot type any sequences. This is done to give you time to review your earnings information.

The first period is called the Double Pay Period. You will be paid 6 cents for each correct task during this period only. This is twice the amount you will be paid for each correct task for the 12 remaining Regular Pay Periods, so take advantage. Your earnings will be displayed in the **Total Earnings** box when the Double Pay Period Summary is displayed.

<INSTRUCTIONS HANDED OUT AFTER THE DOUBLE PAY PERIOD>

The Regular Pay Periods
You will keep your earnings from the Double Pay Period. You will have the exact same labor task in the next 12 Regular Pay Periods, but your earnings will be determined differently.

Groups of Five
All participants in this session have been assigned to a group with 5 members. You will stay in the same group for all of the Regular Pay Periods. Please note that you will not be interacting with any of the other groups in the experiment.

Earnings Calculation for the Regular Pay Periods
The third page shows a sample screen shot of your earnings information at the end of a Regular Pay Period. Use this picture as a guide while the remaining instructions describe the types of earnings information displayed. When you are finished reading instructions, the experimenter will demonstrate how your earnings are calculated each period with examples.

<----Your Labor Earnings

You will be paid 3 cents for each correct task in the Regular Pay Periods. You will receive continuous feedback on your labor earnings during each period.

<--- 80% of Your Labor Earnings are Transferred to the Group Account

80% of your labor earnings will be automatically transferred into the Group Account. This happens to every member of your group. You will receive continuous feedback on your transfers to the Group Account during each period.

<---Your 20% Share of the Group Account

Your 20% Share of the Group Account will be revealed at the end of each period once all transfers to the Group Account are summed together. The Group Account is then split evenly such that each member of the group receives 20% of the Group Account.

<---Your Leisure Earnings

You may want to take a leisure break after completing the labor task a number of times. If you take a leisure break, you will receive payment for your leisure activities: 1 cent for every 2 seconds of leisure time spent. These earnings are private to you, and none of your leisure earnings will be transferred to the Group Account.
Taking a leisure break is easy. Click on the tab labeled **Leisure** to start your leisure break. A blank screen will open up and you will immediately begin to collect leisure earnings. When you are ready to complete more labor tasks click the tab labeled **Labor**.

<---Your Total Earnings

Your total earnings will be revealed at the end of each period. Your total earnings are equal to your labor earnings MINUS your transfers to the Group Account PLUS your 20% share of the Group Account PLUS your leisure earnings.

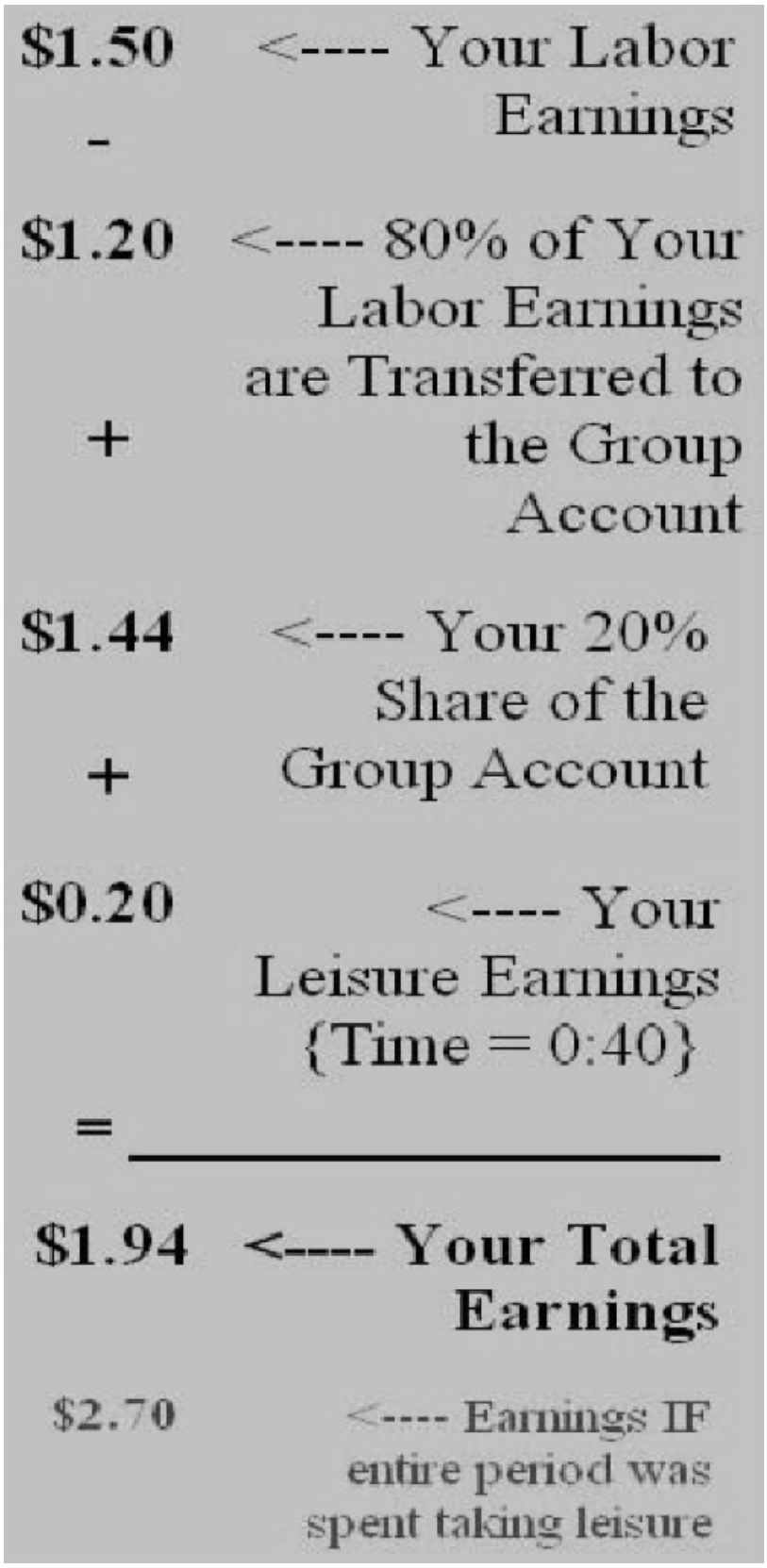
<---- Earnings if entire period was spent taking leisure

This shows information on what your hypothetical earnings would have been if you spent the entire period with the **Leisure Tab** active. This information is revealed at the end of each period.

Reviewing Your Earnings Information

For the last 30 seconds of each period, you will be prohibited from making any labor or leisure earnings. You should take this time as an opportunity to review and interpret what has happened to your earnings this period. Your cumulative earnings for this experiment can also be viewed during this time. Once the thirty seconds are over, a new period will start and you may continue typing sequences or making leisure earnings.

SAMPLE SCREEN SHOT of EARNINGS INFORMATION



APPENDIX B

EXPERIMENT SURVEY FOR CHAPTER II

Exit Questionnaire – You will be paid an additional $5 for filling out this questionnaire.
ID Code _____

1. Are you male or female?

2. What is your class (freshman, sophomore, junior, or senior)?

3. What is your major?

4. What level of education did your mother complete?
   A. Didn't finish high school
   B. High school diploma or equivalent
   C. Some college
   D. Four year college degree
   E. Some graduate or professional schoolwork
   F. Graduate or professional degree

5. What level of education did your father complete?
   A. Didn't finish high school
   B. High school diploma or equivalent
   C. Some college
   D. Four year college degree
   E. Some graduate or professional schooling
   F. Graduate or professional degree

6. What is your current marital status?
   A. Single (never married)
   B. Married or in a marriage-type relationship
   C. Divorced or separated

7. What is your family's household income?
   A. $0-$20,000
   B. $20,000-$40,000
   C. $40,000-$60,000
   D. $60,000-$100,000
   E. $100,000-$200,000
   F. more than $200,000

8. How often do you lend money to friends?

   Number of times per month

   -0 1 2 3 4 5 6 7 8 9 10+

More questions on the following page…

9. How often do you lend personal possessions to friends?

   Number of times per month

-0	1	2	3	4	5	6	7	8	9	10+

For the next set of questions, please circle 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10. Selecting a lower number indicates how strongly you agree with the answer on the left, and higher numbers indicate how strongly you agree with the answer on the right.

10. Do you think most people would try to take advantage of you if they got the chance, or would they try to be fair?

Would take advantage of you										Would try to be fair
0	1	2	3	4	5	6	7	8	9	10

11. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?

Just look out for themselves										Try to be helpful
0	1	2	3	4	5	6	7	8	9	10

12. Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?

Need to be very careful										Most people can be trusted
0	1	2	3	4	5	6	7	8	9	10

13. How much do you tend to trust others when you have a <u>little</u> at stake?

Not at all										A lot
0	1	2	3	4	5	6	7	8	9	10

14. How much do you tend to trust others when you have a <u>lot</u> at stake?

Not at all										A lot
0	1	2	3	4	5	6	7	8	9	10

15. In the past, when you trusted someone, was your trust usually rewarded or usually exploited?

Usually exploited										Usually rewarded
0	1	2	3	4	5	6	7	8	9	10

16. How satisfied are you with your <u>life as a whole</u>?

Very Unsatisfied										Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

More questions on the following page…

17. How satisfied are you with your family life?

Very Unsatisfied									Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

18. How satisfied are you with your social life?

Very Unsatisfied									Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

19. How satisfied are you with your love (romantic) life?

Very Unsatisfied									Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

20. How satisfied are you with your spiritual/philosophical life?

Very Unsatisfied									Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

21. How satisfied are you with your income/financial status?

Very Unsatisfied									Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

22. How satisfied are you with your current job and/or career path?

Very Unsatisfied									Very Satisfied
0	1	2	3	4	5	6	7	8	9	10

The next sets of questions propose a statement. Please circle 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10 to indicate how strongly you agree or disagree with each statement.

23. It is not fair if the results (or allocations) are not fair.

Agree strongly									Disagree Strongly
0	1	2	3	4	5	6	7	8	9	10

24. It is not fair if the rules (or institutions) are not fair.

Agree strongly									Disagree Strongly
0	1	2	3	4	5	6	7	8	9	10

25. If I earn my money I have a right to keep it.

Agree strongly									Disagree Strongly
0	1	2	3	4	5	6	7	8	9	10

26. People should give up some of their earnings to those less fortunate.

Agree strongly									Disagree Strongly
0	1	2	3	4	5	6	7	8	9	10

More questions on the following page…

For the remaining questions, a <u>hypothetical</u> amount has been generously been allocated to you and another person whom you will never meet. You have been randomly selected to choose between Plan A, Plan B, or Plan C, which would determine your earnings and the other person's earnings. The other person has no decision to make. Please indicate your decision by circling the Plan you prefer.

27. Suppose you are **<u>Person 1</u>**. Please circle the Plan you prefer.

	Plan A	**Plan B**	**Plan C**
Person 1 gets	**$300**	**$250**	**$200**
Person 2 gets	$100	$150	$200
Total	***$400***	***$400***	***$400***

28. Suppose you are **<u>Person 1</u>**. Please circle the Plan you prefer.

	Plan A	**Plan B**	**Plan C**
Person 1 gets	**$300**	**$233**	**$167**
Person 2 gets	$100	$133	$167
Total	***$400***	***$366***	***$333***

29. Suppose you are **<u>Person 2</u>**. Please circle the Plan you prefer.

	Plan A	**Plan B**	**Plan C**
Person 1 gets	$300	$250	$200
Person 2 gets	**$100**	**$150**	**$200**
Total	***$400***	***$400***	***$400***

30. Suppose you are **<u>Person 2</u>**. Please circle the Plan you prefer.

	Plan A	**Plan B**	**Plan C**
Person 1 gets	$300	$233	$167
Person 2 gets	**$100**	**$133**	**$167**
Total	***$400***	***$366***	***$333***

APPENDIX C

EXPERIMENT INSTRUCTIONS FOR CHAPTER III

Investment Game Control Instructions

No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Three Monitors and Two Rooms
Three monitors have been randomly selected from among those of you who came here today. The rest of you have been randomly divided into two rooms, called Room A and Room B.

Anonymity
Each person in Room A will be randomly paired with a person in Room B. You will never play with another person in the same room as you. No one will learn the identity of the person she/he is paired with.

Initial Certificates for Room B
Each person in Room B will be given 10 one-dollar certificates and asked to simply keep them.

Initial Certificates for Room A
Each person in Room A will be given 10 one-dollar certificates. As explained below, each Room A person will have a decision to make about what to do with her/his 10 one-dollar certificates.

Complete Privacy
This experiment is structured so that no one, including the experimenters, the monitors, and the other people participating in the experiment will ever know the personal decision of anyone else in the experiment. This is accomplished by a procedure in which you collect your money payoff, contained in a sealed envelope, from a numbered mailbox that only you have the key for. Your privacy is guaranteed because neither your name nor your student ID number will appear on any form that records your decisions in this experiment. The only identifying mark on the decision forms will be a number known only to you. You will be able to collect your money payoffs with privacy by using a key, which opens a mailbox. The key and mailbox will be labeled with the same number you will mark on your decision-reporting forms. But you will be the only person who knows your personal number. **It is your responsibility to mark all decision forms with your personal number. If you fail to do this correctly, then the experimenters may not be able to pay you for your decisions.**

Room A Decision Task
Each person in Room A will decide whether to keep all 10 of his/her one-dollar certificates or give some or all of them to his/her paired person in Room B who will then complete his/her Room B Decision Task. Every certificate given by a person in Room A to a person in Room B will be tripled by the experimenters. The following table shows how this works.

If the Room A Person Gives	The Experimenters Triple the Amount	And the Room B Person Receives
0	3 * 0	0
1	3 * 1	3
2	3 * 2	6
3	3 * 3	9
4	3 * 4	12
5	3 * 5	15
6	3 * 6	18
7	3 * 7	21
8	3 * 8	24
9	3 * 9	27
10	3 * 10	30

Room B Decision Task
In addition to the initial one-dollar certificates, each person in Room B will receive another envelope containing the tripled number of one-dollar certificates given to him/her by the paired person in Room A. He/she will then decide whether to return some, all, or none of the tripled number of one-dollar certificates to the same person in Room A who gave them.

The Role of the Monitors
The monitors have been randomly chosen from among the students who volunteered for today's experiment. The monitors will be in charge of distributing and collecting the large manila envelopes containing mailbox keys and one-dollar certificates. One monitor will be asked to watch and make sure that the experimenters actually follow the procedures that have been explained here. The other two monitors are room monitors and will make sure silence is maintained during the experiment.

The Payoff Procedure
You will be using one-dollar certificates printed on green while making your decision task. After all decisions have been recorded by the experimenters the one-dollar certificates will be exchanged for real U.S. dollars. You may then use your key to obtain your earnings and take your earnings home.

Investment Game Control Summary of Instructions
Room A: A short summary of instructions for the Room A Decision Task
Enclosed in this manila envelope are three small white envelopes:

- The first envelope is labeled "KEEP THIS KEY" and contains a numbered mailbox key. You will keep this key for the remainder of the experiment. This key

will be the only way the experimenters can keep up with your decisions and earnings, **so please make sure the number on your key matches the "key #___" written on the first line of any envelope you receive.**

- The second envelope is labeled "key #___" on the first line and "my certificates" on the following line. Inside the envelope are 10 one-dollar certificates.
- The third envelope is labeled "key #___" on the first line and "certificates given to a person in Room B" on the following line. The envelope is currently empty.

The Room A Decision Task involves you transferring none, some, or all of the certificates in the second envelope into the third envelope.

- The amount left in the second envelope represents the certificates you wish to keep.
- The amount you put in the third envelope indicates the amount you wish to send to the person in Room B you are paired with. The amount will be tripled before reaching the person in Room B.

After making your transfers, please close (but do not seal) the second and third envelopes and place them back in the manila folder. Please close (but do not seal) the manila envelope, place it in the return box, and return to your seat.

Room B: A short summary of instructions for the Room B Decision Task
Enclosed in this manila envelope are four small white envelopes:

- The first envelope is labeled "KEEP THIS KEY" and contains a numbered mailbox key. You will keep this key for the remainder of the experiment. This key # will be the only way the experimenters can keep up with your decisions and earnings, **so please make sure the number on your key matches the "key #___" written on the first line of any envelope you receive.**
- The second envelope is labeled "key #___" on the first line and "my initial certificates" on the following line. Inside the envelope are 10 one-dollar certificates. You may count the certificates if you like, but you will do nothing with these certificates because they are yours to keep. Please leave these certificates in the second envelope.
- The third envelope is labeled "key #___" on the first line and "tripled number of certificates sent by person holding key #___ in Room A" on the following lines. Inside is the tripled number of certificates sent from the other room.
- The fourth envelope is labeled "key #___" on the first line and "certificates returned to the person holding key #___ in Room A" on the following lines. The envelope is currently empty.

The Room B Decision Task involves you transferring none, some, or all of the tripled certificates in the third envelope into the fourth envelope.

- The amount left in the third envelope represents the certificates you wish to keep.
- The amount you put in the fourth envelope indicates the amount you wish to return to the person in Room A who sent you the tripled number of certificates.

After making your transfers, please close (but do not seal) the second, third, and fourth envelopesand place them back in the manila folder. Please close (but do not seal) the manila envelope, place it in the return box, and return to your seat.

Role-Reversal Instructions

No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Three Monitors and Two Rooms
Three monitors have been randomly selected from among those of you who came here today. The rest of you have been randomly divided into two rooms, called Room A and Room B.

Anonymity
Each person in Room A will be randomly paired with a person in Room B. You will never play with another person in the same room as you. No one will learn the identity of the person she/he is paired with.

Initial Certificates for Task Y
Each person completing Task Y will be given 10 one-dollar certificates and asked to simply keep them.

Initial Certificates for Task X
Each person completing Task X will be given 10 one-dollar certificates. As explained below, each person completing Task X will have a decision to make about what to do with her/his 10 one-dollar certificates.

Complete Privacy
This experiment is structured so that no one, including the experimenters, the monitors, and the other people participating in the experiment will ever know the personal decision of anyone else in the experiment. This is accomplished by a procedure in which you collect your money payoff, contained in a sealed envelope, from a numbered mailbox that only you have the key for. Your privacy is guaranteed because neither your name nor your student ID number will appear on any form that records your decisions in this experiment. The only identifying mark on the decision forms will be a number known only to you. You will be able to collect your money payoffs with privacy by using a key, which opens a mailbox. The key and mailbox will be labeled with the same number you will mark on your decision-reporting forms. But you will be the only person who knows your personal number. **It is your responsibility to mark all decision forms with your personal number. If you fail to do this correctly, then the experimenters may not be able to pay you for your decisions.**

Task X

Each person completing Task X will decide whether to keep all 10 of his/her one-dollar certificates or give some or all of them to his/her paired person in the other room who will then complete Task Y. Every certificate given by a person completing Task X to a person completing Task Y will be tripled by the experimenters. The following table shows how this works.

If the Person Completing Task X Gives	The Experimenters Triple the Amount	And the Person Completing Task Y Receives
0	3 * 0	0
1	3 * 1	3
2	3 * 2	6
3	3 * 3	9
4	3 * 4	12
5	3 * 5	15
6	3 * 6	18
7	3 * 7	21
8	3 * 8	24
9	3 * 9	27
10	3 * 10	30

Task Y

In addition to the initial one-dollar certificates, a person completing Task Y will receive another envelope containing the tripled number of one-dollar certificates given to him/her by the paired person in Task X. He/she will then decide whether to return some, all, or none of the tripled number of one-dollar certificates to the same person who gave them in Task X.

You will be completing Task X and Task Y

People in both rooms will complete Task X first and then Task Y. This is done by the following procedure:

1. Each person will select a large manila envelope from a box carried around by his/her room monitor.
2. Each person will complete Task X, reseal his/her large manila envelope, and return it to the box.
3. The experimenters will record the Task X decisions and prepare new large manila envelopes for Task Y.
4. Once again, each person will select a new manila envelope from a box carried around by his/her room monitor.
5. Each person will complete Task Y, reseal his/her manila envelope, and return it to the box.
6. The experimenters will record the Task Y decisions and prepare the payoffs for the end of the experiment.

The Role of the Monitors

The monitors have been randomly chosen from among the students who volunteered for today's experiment. The monitors will be in charge of distributing and collecting the large manila envelopes containing mailbox keys and one-dollar certificates. One monitor will be asked to watch and make sure that the experimenters actually follow the procedures that have been explained here. The other two monitors are room monitors and will make sure silence is maintained during the experiment.

The Payoff Procedure

You will be using one-dollar certificates printed on either BLUE or RED paper for each task:

- BLUE Certificates: Used by people in Room A completing Task X and people in Room B completing Task Y.
- RED Certificates: Used by people in Room B completing Task X and people in Room A completing Task Y.

You will know the outcome of both of your tasks, but **only one of your tasks will be randomly selected for payoff**: After both rooms have completed Task Y, the monitor will flip a coin to determine if the BLUE (heads) or the RED (tails) one-dollar certificates will be exchanged for real U.S. dollars. You may then use your key and take your earnings home.

Role-Reversal Summary of Instructions

Room A: A short summary of instructions for Task X

Enclosed in this manila envelope are three small white envelopes:

- The first envelope is labeled "KEEP THIS KEY" and contains a numbered mailbox key. You will keep this key for the remainder of the experiment. This key # will be the only way the experimenters can keep up with your decisions and earnings, **so please write down your key # each place you see "key #___" on an envelope or form.**
- The second envelope is labeled "key #___" on the first line and "my certificates" on the following line. Inside the envelope are 10 BLUE one-dollar certificates.
- The third envelope is labeled "key #___" on the first line and "certificates given to a person in Room B" on the following line. The envelope is currently empty.

Task X involves you transferring none, some, or all of the certificates in the second envelope into the third envelope.

- The amount left in the second envelope represents the certificates you wish to keep.
- The amount you put in the third envelope indicates the amount you wish to send to the person in Room B you are paired with. The amount will be tripled before reaching the person in Room B.

After making your transfers, please close but do not seal the second and third envelopes and place them back in the manila folder. Please close but do not seal the manila envelope, place it in the return box, and return to your seat.

Room A: A short summary of instructions for Task Y

Enclosed in this manila envelope are three small white envelopes:

- The first envelope is labeled "key #___" on the first line and "my initial certificates" on the following line. Inside the envelope are 10 RED one-dollar certificates. You may count the certificates if you like, but you will do nothing with these certificates because they are yours to keep.
- The second envelope is labeled "key #___" on the first line and "tripled number of certificates sent by person holding key #___ in Room B" on the following lines. Inside is the tripled number of certificates sent from the other room.
- The third envelope is labeled "key #___" on the first line and "certificates returned to the person holding key #___ in Room B" on the following lines. The envelope is currently empty.

Please record your key # on all three envelopes.

Task Y involves you transferring none, some, or all of the certificates in the second envelope into the third envelope.

- The amount left in the second envelope represents the certificates you wish to keep.
- The amount you put in the third envelope indicates the amount you wish to return to the person in Room B who sent you the tripled number of certificates.

After making your transfers, please close (but do not seal) all three envelopes and place them back in the manila folder. Please close (but do not seal) the manila envelope, place it in the return box, and return to your seat.

APPENDIX D

EXPERIMENT SURVEY FOR CHAPTER III

Survey Questions

1. What is your key number?

2. Are you male or female?

3. What is your class (freshman, sophomore, junior, or senior)?

4. What is your major?

5. What level of education did your mother complete?
   A. Didn't finish high school
   B. High school diploma or equivalent
   C. Some college
   D. Four year college degree
   E. Some graduate or professional schoolwork
   F. Graduate or professional degree

6. What level of education did your father complete?
   A. Didn't finish high school
   B. High school diploma or equivalent
   C. Some college
   D. Four year college degree
   E. Some graduate or professional schooling
   F. Graduate or professional degree

7. What is your current marital status?
   A. Single (never married)
   B. Married or in a marriage-type relationship
   C. Divorced or separated

8. What is your family's household income?
   A. $0-$20,000
   B. $20,000-$40,000
   C. $40,000-$60,000
   D. $60,000-$100,000
   E. $100,000-$200,000
   F. more than $200,000

Task X Questions [*Room A Questions for IG Control*]

9. Did you send part of your initial certificates to the paired person completing Task Y?

10. If you did send some, how many certificates did you send?

11. What were your reasons for making the decision that you made?

Room A Hypothetical Role-Reversal Question for IG Control

Suppose you were placed into Room B instead of Room A and you must complete the Room B Decision Task. A person from Room A has sent you ____ one-dollar certificates which are tripled by the experimenter resulting in you receiving ____ one-dollar certificates. How many of the tripled one-dollar certificates would you have returned to this person from Room A?

Task Y Questions [*Room B Questions for IG Control*]

12. Did you receive some "additional certificates"?

13. If you did receive some "additional certificates," how many did you receive?

14. If you did receive "additional certificates," did you return any of them to your paired person completing Task X?

15. How many certificates did you return, if any?

16. What were your reasons for making the decision that you made?

Room B Hypothetical Role-Reversal Question for IG Control

Suppose you were a member of Room A instead of Room B and you must complete the Room A Decision Task. How many of your one-dollar certificates would you have sent?
Please circle only one value.

$0 $1 $2 $3 $4 $5 $6 $7 $8 $9 $10

Other Questions

17. There was a 50% chance that the decision you made in each Task would affect the real payoffs of you and your paired person for each Task. Did knowing this influence your decisions? Please explain your answer.

18. Do you think most people would try to take advantage of you if they got the chance, or would they try to be fair?
    A. Would take advantage of you
    B. Would try to be fair
    C. Depends
    D. Don't know

19. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?
    A. Try to be helpful
    B. Just look out for themselves
    C. Depends
    D. Don't know

20. Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?
    A. Most people can be trusted
    B. Need to be very careful
    C. Depends
    D. Don't know

21. When dealing with strangers, one is better off using caution before trusting them.
    A. Disagree strongly
    B. Disagree somewhat
    C. Disagree slightly
    D. Agree slightly
    E. Agree somewhat
    F. Agree strongly

22. How much do you tend to trust others when you have a little at stake?
    A. Not at all
    B. A little
    C. Some
    D. A lot

23. How much do you tend to trust others when you have a lot at stake?
    A. Not at all
    B. A little
    C. Some
    D. A lot

24. How often do you lend money to friends?
    A. More than once a week
    B. About once a week
    C. About once a month
    D. Once a year or less

25. How often do you lend personal possessions to friends?
    A. More than once a week
    B. About once a week
    C. About once a month
    D. Once a year or less

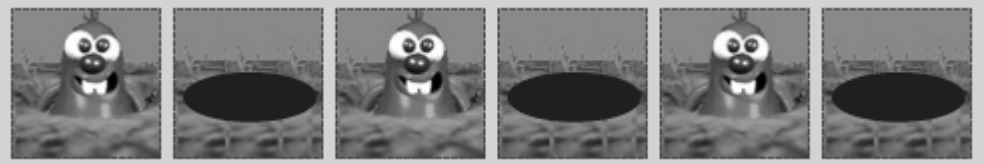
26. How many friends do you have whom you would consider close?

APPENDIX E

EXPERIMENT INSTRUCTIONS FOR CHAPTER IV

Used in Treatments CH1 and CH4 (Private Property Trust Game)

Instructions for the Whack-A-Mole Game



No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

The Whack-A-Mole Game Description
You will be playing the Whack-A-Mole Game for the next 15 minutes. The Whack-A-Mole Game is similar to the arcade version you may be familiar with, but the rules and scoring are a bit different.

- The object of the game is to hit the moles as they pop out of their holes by clicking on the picture squares with moles in them.
- All of the moles pop out of their holes at once. The number of moles that pop up is random: at any given time there is a 50% chance that each mole will pop out of his/her hole.
- You do not have to hit the moles right away. They will stay up until you hit them.
- You will earn points for each field of moles you clear. Each time you clear a field your score will increase and a new bunch of moles will pop up for the next field you have to clear.

Field Clearing Requirements
If you clear a minimum of 120 fields within the allotted time of 15 minutes you will earn 10 tokens to use in the next game. The 10 tokens have a starting value of one dollar each, for a total value of $10. Once you clear 120 fields, you do not have to clear any more fields, and you will not be paid extra for clearing more fields.

If you do not clear enough fields, then you will be paid your show-up fee of $5 and will be asked to leave before the next game begins.

The next game is played in pairs, so the number of participants must be even. If an odd number of participants clear 120 fields then the one participant who was closest to clearing 120 fields will also be allowed to play to make the number of participants even.

Decision Game Instructions

No Talking Allowed
Now that the decision game has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Two Types
The participants in today's experiment will be randomly divided into two types, referred to as Type X and Type Y.

Random Pairing and Anonymity
Each person of Type X will be randomly paired with a person of Type Y. No one will learn the identity of the person with whom he/she is paired. As discussed below, Type X persons will make their decisions anonymously in one room, and Type Y persons in another room.

Starting Balances
Each person in a pair of decision-makers will start with his/her token fund of 10 tokens earned in the Whack-A-Mole game. The 10 tokens have a starting value of one dollar each, for a total value of $10.

Complete Privacy
This experiment is structured so that no one, including the experimenters or the other participants, will ever know the personal decision or money earnings of anyone in the experiment. This is accomplished by a procedure in which you collect your earnings, contained in a sealed envelope, from a numbered mailbox that only you have the key for. Your privacy is guaranteed because neither your name nor your student ID number will appear on any form that records your decisions in this experiment. The only identifying mark on the decision forms will be an identification number known only to you. You will be able to collect your money payoffs with privacy by using a key, which opens a mailbox located in a room adjacent to this room. The key and mailbox will be labeled with the same number as your decision-reporting forms. But you will be the only person who knows your personal number.

The Type X Decision Task
Each person of Type X will decide whether or not to send any of his/her tokens to the paired person of Type Y. Each token that a Type X person sends reduces the value of his/her token fund by $1 but increases the value of the token fund of the paired Type Y person by $3. A Type X person cannot send more than his initial 10 token starting balance. Four examples illustrate how the values of the tokens that may be sent by Type X are related to the value of the Type Y token fund.

- If the Type X person sends 0 tokens, that reduces the value of his/her fund by $0 and adds $0 to the value of the fund held by the paired Type Y person.

- If the Type X person sends 1 token, that reduces the value of his/her fund by $1 and adds $3 to the value of the fund held by the paired Type Y person.
- If the Type X person sends 5 tokens, that reduces the value of his/her fund by $5 and adds $15 to the value of the fund held by the paired Type Y person.
- If the Type X person sends 10 tokens, that reduces the value of his/her fund by $10 and adds $30 to the value of the fund held by the paired Type Y person.

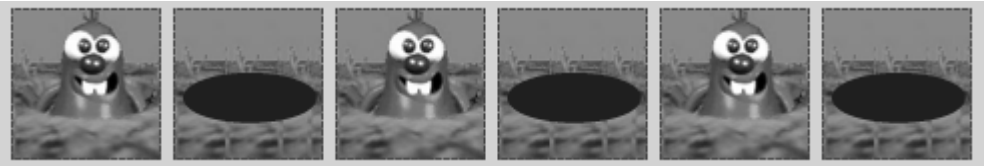
The Type Y Decision Task – *Treatment CH1 Version (Sequential Move Protocol)*
After the Type X person in a pair makes his/her decision, the Type Y person in that pair makes his/her decision. The Type Y person's decision is to divide the value of the token fund he/she holds between his/her self and the paired person of Type X. That is, the Type Y person decides how much of the fund to keep for his/her self and how much to send back to the Type X person.

The Type Y Decision Task – *Treatment CH4 Version (Strategy Method Protocol)*
The Type Y person's decision task is to divide the value of the token fund he/she holds between his/her self and the paired person of Type X. That is, the Type Y person decides how much of the fund to keep for his/her self and how much to send back to the Type X person.

The Type Y person will not know the Type X person's decision until after he/she finishes the Type Y Decision Task. Therefore, the Type Y person will have to submit a binding decision for each possible Type X decision.

Used in Treatments CH2, CH3, and CH5 (Common Property Trust Game)[25]

Instructions for the
Whack-A-Mole Game



No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

The Whack-A-Mole Game Description
You will be playing the Whack-A-Mole Game for the next 15 minutes. The Whack-A-Mole Game is similar to the arcade version you may be familiar with, but the rules and scoring are a bit different.

- The object of the game is to hit the moles as they pop out of their holes by clicking on the picture squares with moles in them.
- All of the moles pop out of their holes at once. The number of moles that pop up is random: at any given time there is a 50% chance that each mole will pop out of his/her hole.
- You do not have to hit the moles right away. They will stay up until you hit them.
- You will earn points for each field of moles you clear. Each time you clear a field your score will increase and a new bunch of moles will pop up for the next field you have to clear.

Field Clearing Requirements
If you clear a minimum of 120 fields within the allotted time of 15 minutes you will earn 20 tokens to use in the next game. Your 20 tokens will be placed in a joint decision fund together with the 20 tokens of another person who cleared 120 fields. The 40 tokens in the joint decision fund will have a value of one dollar each, for a total value of $40.

If you do not clear enough fields, then you will be paid your show-up fee of $5 and will be asked to leave before the next game begins.

The next game is played in pairs, so the number of participants must be even. If an odd number of participants clear 120 fields then the one participant who was closest to clearing 120 fields will also be allowed to play to make the number of participants even.

[25] Treatment CH3 differs only in the size of the Mole Field Quota =240 and allotted time of 30 Minutes to meet the quota.

Decision Game Instructions

No Talking Allowed
Now that the decision game has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Two Types
The participants in today's experiment will be randomly divided into two types, referred to as Type X and Type Y.

Random Pairing and Anonymity
Each person of Type X will be randomly paired with a person of Type Y. No one will learn the identity of the person with whom he/she is paired. As discussed below, Type X persons will make their decisions anonymously in one room, and Type Y persons in another room.

Starting Balances
Each pair of decision-makers will start with balance of 40 tokens in their joint decision fund earned in the Whack-A-Mole game. The 40 tokens have a starting value of one dollar each, for a total value of the joint decision fund of $40.

Complete Privacy
This experiment is structured so that no one, including the experimenters or the other participants, will ever know the personal decision or money earnings of anyone in the experiment. This is accomplished by a procedure in which you collect your earnings, contained in a sealed envelope, from a numbered mailbox that only you have the key for. Your privacy is guaranteed because neither your name nor your student ID number will appear on any form that records your decisions in this experiment. The only identifying mark on the decision forms will be an identification number known only to you. You will be able to collect your money payoffs with privacy by using a key, which opens a mailbox located in a room adjacent to this room. The key and mailbox will be labeled with the same number as your decision-reporting forms. But you will be the only person who knows your personal number.

The Type X Decision Task
Each person in Type X will decide whether or not to withdraw tokens from the joint decision fund. Each token that a Type X person withdraws has a value to that person of $1. Each token withdrawn reduces the value of the joint decision fund by $3. A Type X person cannot withdraw more than 10 tokens. Four examples illustrate how the values of the tokens remaining in the decision fund are related to the values of the tokens that may be withdrawn:

- If the Type X person removes 0 tokens, that adds $0 to his/her earnings and does not change the value of the joint decision fund.
- If the Type X person removes 1 token, that adds $1 to his/her earnings and reduces the value of the joint decision fund by $3.

- If the Type X person removes 5 tokens, that adds $5 to his/her earnings and reduces the value of the joint decision fund by $15.
- If the Type X person removes 10 tokens, that adds $10 to his/her earnings and reduces the value of the joint decision fund by $30.

The Type Y Decision Task – *Treatment CH2 and CH3 Version (Sequential Move Protocol)*

After the Type X person in a pair makes his/her decision, the Type Y person in that pair makes his/her decision. The Type Y person's decision is to divide between his/her self and the paired person of Type X the value of the joint decision fund after the Type X person's withdrawal decision. That is, the Type Y person decides how much of the remaining value of the fund to keep for his/her self and how much to leave for the Type X person.

The Type Y Decision Task – *Treatment CH5 Version (Strategy Method Protocol)*

The Type Y person's decision task is to divide between his/her self and the paired person of Type X the value of the joint decision fund after the Type X person's withdrawal decision. That is, the Type Y person decides how much of the remaining value of the fund to keep for his/her self and how much to leave for the Type X person.

The Type Y person will not know the Type X person's decision until after he/she finishes the Type Y Decision Task. Therefore, the Type Y person will have to submit a binding decision for each possible Type X decision.

APPENDIX F

EXPERIMENT SURVEY FOR CHAPTER IV

Type X Questionnaire

Thank you very much for participating in our decision experiment. We would like to ask you a few questions about your experience in this experiment and about you. **What is Your Key Number? _____**

Questions about the experiment:

1. Is this the first decision experiment in which you have participated? Yes ____ No ____
2. If your answer to #1 is "No", approximately how many other experiments have you been in? _____
3. Were the experiment instructions clear? Yes___ No ____
4. Did you have any questions you wanted to ask us? If yes, please briefly write them in the space provided

_____
_____
_____

5. Did you send all or some of your tokens to the Type Y person with whom you were paired? Yes ____ No _____
   If yes, how many tokens did you send? _____
   What were your reasons for making the decision that you made?

_____
_____
_____

[For the Common Property Trust Game Treatments the Type X Person was asked:
5. Did you withdraw any tokens from the joint decision fund?
Yes ____ No _____
If yes, how many tokens did you withdraw? _____]

More general questions:

1. Generally speaking, would you say that most people can be trusted or that you can't be too careful dealing with people?

   Most people can be trusted _____ Can't be too careful _____ Don't know ____

2. Would you say that most of the time people try to be helpful or that they are mostly just looking out for themselves?

   Try to be helpful _____ Just look out for themselves_____ Don't know ____

3. Do you think that most people would take advantage of you if they got a chance or would they try to be fair?

   Most people would take advantage ___They would try to be fair___ Don't know____

Information about you:

What year are you in school? Freshman ____ Sophomore ____ Junior ____ Senior ____

What is your intended or declared major ? _____

What is your age? _____ What is your gender? Female ____ Male _____

Type Y Questionnaire

Thank you very much for participating in our decision experiment. We would like to ask you a few questions about your experience in this experiment and about you. **What is Your Key Number? _____**

Questions about the experiment:

1. Is this the first decision experiment in which you have participated? Yes ____ No ____
2. If your answer to #1 is "No", approximately how many other experiments have you been in? _____
3. Were the experiment instructions clear? Yes___ No ____
4. Did you have any questions you wanted to ask us? If yes, please briefly write them in the space provided

_____
_____
_____

5. Did the Type X person with whom you were paired send all or some of their token fund to you?
Yes ___ No __
If yes, how much money from their fund did they send you? _____
How much did you send to the Type X person with whom you were paired? _____
What were your reasons for making the decision that you made?

_____
_____
_____

[For Common Property Trust Game Treatments Type Y was asked:

5. Did the Type X person withdraw any tokens from the joint decision fund?
Yes ___ No __
If yes, how many tokens did they withdraw? _____
How much did you send to the Type X person with whom you were paired from the remaining joint decision fund? _____
What were your reasons for making the decision that you made?]
More general questions:

1. Generally speaking, would you say that most people can be trusted or that you can't be too careful dealing with people?

   Most people can be trusted _____ Can't be too careful _____ Don't know ____

2. Would you say that most of the time people try to be helpful or that they are mostly just looking out for themselves?

   Try to be helpful _____ Just look out for themselves_____ Don't know ____

3. Do you think that most people would take advantage of you if they got a chance or would they try to be fair?

   Most people would take advantage ___They would try to be fair___ Don't know____

Information about you:

What year are you in school? Freshman ____ Sophomore ____ Junior ____ Senior ____
What is your intended or declared major ? _____
What is your age? _____ What is your gender? Female ____ Male _____

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VITA

Daniel Thomas Hall was born on August 13, 1980 in Snellville, GA. He holds a Bachelor of Science in economics from Georgia College & State University (Milledgeville, GA) and a Master of Arts in economics from Georgia State University (Atlanta, GA).

In the fall of 2004, Daniel joined Georgia State University to pursue a doctoral degree in economics. Throughout his time at Georgia State, has obtained several awards. Daniel has won the Andrew Young School of Policy Studies Dean's Fellowship Award. Daniel also obtained a dissertation grant from Georgia State University.

Daniel has received support from the Department of Economics and Experimental Economics Center (E*x*CEN) at the Andrew Young School of Policy Studies of Georgia State University. In 2008, he received a travel grant from the Department of Economics to present *An Experimental Test of the Efficiency Equality Tradeoff: Income Redistribution and its Effect on Real Labor-Leisure Choices* at the Southern Economic Association Meetings held in Washington, D.C. The title of the paper was later changed to *The Efficiency-Equality Tradeoff in Public Sector Charity Provision* and is now published in Volume 13 of Research in Experimental Economics. The volume is titled *Charity with Choice*. In 2009, he received another travel grant from the Department of Economics to present *Using Role-Reversal in Laboratory Experiments* at the Southern Economic Association Meetings held in San Antonio, TX. Also in 2009, he received a travel grant from the Experimental Economics Center (E*x*CEN) to present the paper "*Trust in Private and Common Property Experiments: Effects of Stronger Property Right Entitlements"* at the Annual North American Economic Science Association Meetings

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While at Georgia State, Daniel was involved with many activities at Georgia State. Internally, he served as a graduate research assistant to labor and innovation economist Dr. Paula Stephan and later for the Experimental Economics Center. He also served a term as Vice President of Graduate Student Association of the Andrew Young School of Policy Studies. He has also presented multiple times in the Georgia State Economics Department brown bag seminar series.

Daniel demonstrated proficiency in teaching in 2007, where he taught multiple courses in principles of microeconomics and macroeconomics as an Adjunct Instructor at the Atlanta Campus of Georgia Military College, a Junior Liberal Arts College.

Daniel received his doctorate in economics from the Andrew Young School of Policy Studies at Georgia State University in May 2010. Daniel plans to pursue a long-term career in research and academia, and he will be starting as an Assistant Professor of Economics at High Point University in High Point, NC. He can be reached by email at danhall13@hotmail.com or via his homepage, http://sites.google.com/site/danhall813. His permanent mailing address is 500 Capri Point, Lavonia, GA 30553.