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Cooperation in a Repeated Public Goods Game with a Probabilistic Endpoint

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by

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Introduction

In our experiment, we have a multiple-round public goods game but with a probabilistic endpoint. This changes the Nash equilibrium, such that cooperation is the new equilibrium strategy. The experiment consists of two treatments, one with a single round per session (called the intertemporal treatment), and the second with multiple rounds per session. Experimental results suggest that contribution was indeed positive and consistent provided a high enough probability of the game's continuation, but declined when probability fell.

Game theory suggests that players in public goods games will not cooperate under conditions when the endpoint is known. Rational agents do not assume other players would cooperate or otherwise reduce their payoffs to the benefit of other agents. This is most evident in typical prisoner dilemma games, as well as in applied prisoner dilemma games, such as public goods experiment, and the appropriately named "trust" games. Theory and experimentation have both indicated that under most circumstances, individuals are incentivized to cheat in public goods and prisoner dilemma games. The paradox, then, is that individualized incentives create lower collective payoffs.

The public goods game exemplifies the economic phenomena concerning cooperation and trust. Policy issues and social sciences concerning voluntary contribution, coordination, and cooperation are expressed as a fundamental public goods problem. Such problems are concerned with determining which conditions people cooperate and which institutions and environmental factors foster cooperation.

One example of public goods implications is the failure of motorists to voluntarily reduce their own carbon emissions. If every motorist did reduce the amount of gasoline they used, either by carpooling or by purchasing more fuel efficient cars, carbon emissions will decrease. But the individual motorist's costs would be higher than if that motorist were to do nothing and let everyone else change their behaviors to improve the environment. This is the "free rider problem," in which benefits are received collectively but the costs dispersed to each individual.

Most public goods games, however, have experimental settings that are disanalagous with this and other examples of real life public goods problems. One of the most significant differences is that every player knows the point at which the game ends in most experiments. Game theory, the study of strategic rational behavior, offers some theoretical insight into why the public goods problem occurs under these conditions. In a one-shot public goods game, each player ought to not cooperate, according to theoretical predictions. The reason for this is that a decision maker would maximize his earnings in this way, given the theoretical expectations of the other players. This reasoning, whereby a strategic decision maker takes into account the decisions of other players leads to the group of individuals to reach what is known as a "Nash Equilibrium."

Even in a multiple-round public goods game, theory does not change. To demonstrate this conclusion, Game theorists engage in "backward induction," where the Nash Equilibrium of the last round is determined first. In the last round, equilibrium is the same as the one-shot game, since no future losses due to defection are incurred. The equilibrium of the second-to-last round is then determined, and again, because no future losses are forgone, the dominant strategy is to not cooperate. Every round up to the first, then, carries the same strategy.

Such public good games with a known endpoint are common, but represent a departure from real world instances of public goods problems. Hence, the public goods games with this kind of boundary condition provide are limited in its insight to real-world application.

Most public goods experiments do not study the effect of the boundary condition but instead the effect variation of group size, punishments, and information has on individual contribution. Ledyard (1995) provides an overview of public goods experimental literature with known endpoints up to the mid 1990's, starting with the influential early work undertaken in the area by Bohm (1972), where he set up a well thought out test "involving five different approaches to estimating demand for a public good." His conclusion after the data were analyzed was that people may be willing to contribute to the public good even if it is not in their self-interest. This experiment suggested the emergence of a social cohesion, which we attempted to capture in our intertemporal treatment.

With several other works conducted by Robyn Dawes, John Orbell, and by their colleagues (Dawes 1980, Dawes et al., Orbell et al.), by Gerald Marwell and Ruth Ames (1979), and by Mark Isaac and James Walker (1988), the findings in this time period suggested that A)There is more contribution than predicted in the Nash Equilibrium for one shot-versions of the public goods game, but also B) If the players interact repeatedly over a number of rounds, then contributions often start out higher than the Nash equilibrium and decline over time as more players choose to "free ride."

Public goods experiments with a known endpoint but otherwise similar parameters to our own experiment demonstrate this decline as well. In Isaac et al (1998), Andreoni (1995b), and Croson (1996), experimental groups consisted of three or four people, as well as treatments consisting of ten or fifteen rounds. All three had treatments without punishments or other special parameters that would make comparison difficult. In all three, we find a free rider problem that leads to a decline in individual contribution.

Since Ledyard, public goods experiments explored the role of “conditional cooperation” Chaudhuri (2010) notes that much of the literature since 1995 has been concerned with establishing stable conditions to encourage cooperation. This was done with either punishments by monetary means (Fehr and Gachter 2000) or non-monetary means (Maschlet et al 2003). Methods to promote cooperation by allowing cooperation (Chaudhuri 2006) were also developed, and many of these methods were developed in the study of trust and prisoner’s dilemma games.

Literature involving repeated public goods games with an unknown endpoint exist, but is sparse Cooperation or contribution to the public good can often be sustained under these conditions. Fudenberg and Maskin (1986), Friedman (1986), Raub (1988), and Taylor (1987) all demonstrate that if actors have an unknown time horizon and use a trigger strategy, then voluntary cooperation is possible.

Palfrey and Rosenthal (1994) was an early attempt to test whether a repeated public goods game resulted in higher individual contribution than a one-shot public goods game. Each game in the experimental treatment was repeated with a “random stopping rule,” and found that this treatment consistently resulted in higher returns than the one-shot public goods game (which was the control). None of the players knew any details regarding this rule, and so the game is a noteworthy example of an early public goods game with an unknown endpoint.

Sell and Wilson (1999) also ran a public goods experiment with an unknown endpoint. While the paper was concerned with the effect of group punishment, their treatments were divided into *Required Grim* and *No Required Length*. The *Required Grim* treatment forced the subjects into a grim trigger strategy (that is, once a player defects, all players must defect), and hence other players were punished for defecting. The *No Required Length* allowed the subjects to behave in any way they prefer, and subjects were merely informed of their choices. In this treatment, conditions of the experiment were similar to ours, with contribution levels similar to our own. The results from this experiment will be compared to our results in Table 1.2.

In this treatment, subjects contributed on average 43 percent of their endowment to the public good. This is compared to 63 percent in the *Required Grim* treatment with the same discount rate. While a treatment with an unknown endpoint and punishment results in significantly larger contribution rates, a treatment with just an unknown endpoint results in approximately half of the average subjects' endowment being contributed.

Gonzalez et al (2004) conducted a public goods experiment with groups of three players. In any one period, each participant is endowed with 20 ECU (Experimental Currency Unit), and must privately decide how much to contribute to a public good, keeping the remaining ECU for herself. The experiment's control non-intertemporal treatments the *standard protocol* (SP), in which all three group members are publicly informed that the interaction will last for exactly 10 periods. In the *interval protocol* (PIP), each group member is aware that the interaction will last at least 8 rounds. In this game, Gonzalez et al found that when using the standard practice of publicly announcing a definite endpoint, participants tend to reduce the variation of cooperation levels across periods.

While public goods games have used unknown endpoints, there are not public goods experiments with probabilistic endpoints. For this reason, we examine trust and prisoner dilemma games with probabilistic endpoints. J. Engle-Warnick and Robert L. Slonim (2006), for example, use a probabilistic endpoint in their trust game experiment.

Their experiment consists of several "supergames" of forty sequentially repeated trust games divided in two parts. The first twenty games have indefinite length (i.e. an uncertain number of rounds) and the last twenty have definite lengths (i.e. a number of rounds that is known with certainty). The first player then chooses between two actions: *Send* and *Don't Send*. . If they play *Don't Send*, the stage game ends and both players receive their endowment. If they play *Send*, their endowment doubles and is given to the second player. At this point in the stage game, the second player chooses between two actions: *Return* and *Keep*. If they play *Return*, the doubled endowment is split evenly between the players, and the stage game ends. If they play *Keep*, the second player receives the entire endowment. In the indefinitely repeated supergames, after each round there is a fixed probability that the game will end. When the supergame ends, subjects are randomly and anonymously re-paired with new opponents to play the next supergame.

The experimenters noted the tit-for-tat strategy employed by most subjects in the experiment. The vast majority of first players in round 2 play *Send* when the second player in round 1 plays *Return*. Within each supergame, trust and reciprocity declines between the first and second player, and the number of times Send and Return is played declines. But in each new supergame, reciprocity resets to a higher level. Under a probabilistic endpoint, the length of a trust game also is shown to have an effect on reciprocity, and does not diminish with experience.

Pedro Dal Bo conducted a similar experiment in "Cooperation under the shadow of the future: experimental evidence from infinitely repeated games," incorporating a prisoner's dilemma game, where subjects interacted anonymously. In half the experiments, the session has a known endpoint, and the other half, the endpoint is probabilistically determined. The experimenter writes that, the greater the uncertainty (that is, the lower the probability that the session will be terminated), the greater the level of cooperation. Cooperation was determined by percentage of subjects choosing to Push and not Pull. Bo finds that the greater the uncertainty, the greater the level of cooperation. Just as with Warnick and Slonin (2006), the tit for tat strategy leads to subjects to cooperate due to increasing levels of uncertainty.

Tit-for-tat strategy is important in encouraging cooperation in trust games of indefinite length. This phenomenon allows, for instance, cooperation of randomized strangers to occur. “Cooperation among strangers: an experiment with indefinite interaction” by Gabriele Camera and Marco Casari examined how random subjects could cooperate in a prisoner’s dilemma game, and the emergence of norms of cooperation in experimental economies populated by strangers interacting indefinitely and lacking formal enforcement institutions. 160 subjects were recruited from the University of Purdue and sessions were run at the Vernon Smith Experimental Economics Laboratory.

The experiment has four treatments differing in two dimensions. The first was the level of information available to each subject. Under *private monitoring*, subjects observed only their own history and under *public monitoring*, they observed the history of the whole economy. The second dimension was the method of punishment. In some treatments subjects could only punish by defecting, while in the *personal punishment* treatment, they could pay a cost to inflict a loss on their opponent. The experimenters describe that the availability of information on actions in the economy was set at one of three different degrees. First, subjects could be aware only of their own history (private monitoring, private monitoring with punishment) or of the history of the entire economy. Second, the history of the economy could be made available at an aggregate (anonymous public monitoring) or individual level (non-anonymous public monitoring).

Camera and Casari (2009) studied the qualitative impact of indefinite lengths, with a continuation probability, on cooperation among strangers in prisoner dilemma games. What is examined by Camera and Casari is that, under prisoner dilemma games of indefinite length, cohesion emerges and the Nash equilibrium is higher. Even in anonymous settings, where players are unaware of what contribution other players make, cooperation can be sustained.

Normann and Wallace observed to what extent is uncertainty about the game endpoint crucial to the Prisoner's Dilemma game in terms of experimental evidence in "The impact of the termination rule on cooperation in a prisoner's dilemma experiment." The game consisted of four treatments. In treatment Unknown, the length of the experiment (28 periods) was not mentioned to the participants and the instructions merely said that the experiment would last at least 22 periods. In RandomLow, the instructions said that the experiment would last least 22 periods, and then the experiment would continue with a probability of 1/6. In treatment RandomHigh, there were at least 22 periods and then the experiment would continue with a probability of 5/6. In all four treatments, the matching of participants was fixed over the entire experiment. There were 15 pairs per treatment.

The experiment demonstrated no significant difference in average cooperation rates among the four treatments. However, there is a significant and negative trend between number of rounds and cooperation in all rounds except Unknown. This indicated that the treatment Unknown was able to avoid what the experimenter referred to as the "end-game effect."

<u>Experimental Design</u>

The experiment is a public goods game with a probabilistic endpoint and consists of two treatments, the non-intertemporal treatment and the intertemporal treatment. In the non-intertemporal treatment, players make their decisions for all rounds during one meeting, while in the non-intertemporal treatment, which consisted of two waves of experimentation, subjects meet multiple times and each meeting consisted of one decision round, and otherwise consisted of the same experimental parameters. In the non-intertemporal treatment, the sessions lasted approximately twenty minutes and subjects attended a session once every Tuesday, Wednesday, Thursday, and Friday. In the non-intertemporal treatment, 48 subjects were recruited and organized in groups of 4 from undergraduate classes at the College of William and Mary in Williamsburg, Virginia. Each player was given five dollars which he can either keep, and earn one dollar, or invest in the group's total investment, earning $.50, knowing that every other player will also earn $.50 for his investment and knowing that he will also earn $0.50 for each token invested by other people in the group. After the end of every round, a die is rolled for each group. If the die lands on 1,2,3,4, or 5, then the designated group continues for the next round. If the die lands on a 6, then the experiment is over for that group. Given the limited amount of money available for this experiment, it was sometimes necessary to increase the probability of the experiment ending for a given group from one-in-six to five-in-six, in order to end the experiment quickly. This was done due to the exceeding level of contribution exhibited by subjects in the non-intertemporal treatment. This was used at the end of Round 5 in both waves of the non-intertemporal treatment, and in two instances in the non-intertemporal treatment.[1]

Because the public goods model includes risk (in the form of conditional cooperation) and uncertainty (in the form of others' investment), subjects' risk preferences may play a role in decision making. In our model, we measure risk preferences in two distinct ways. The first is through a lottery choice economics experiment designed by Holt and Laury (2002) with real monetary payoffs. This lottery-based decision making exercise measures agent-specific risk tolerance, and is conducted at the beginning of the experiment. The second is through a series of survey questions framed around an agent's reaction to various hypothetical scenarios and

[1] A total of 41 subjects, or 7.9% of all observations, were affected by this increase in the probability of termination

frequency of participating in risk-oriented behavior such as wearing a seatbelt or gambling. A copy of the survey questions may be found in Appendix A.

Lottery Choice Risk Tolerance Game

In the Holt and Laury (2002) design, subjects make ten decisions between Options A and B. Each option comprises of two possible payoffs, with a probability assigned to each payoff. Option A is the "safer" choice because there is less variance between the two payoffs than in Option B. The payoffs assigned to each option are fixed but the probability of receiving a higher payoff in an option increases moving from decision number 1 to 10. In decision 1, the high payoff is selected if the number generated by the ten-digit random number generator is 1 and the low payoff is paid if the number generated is 2-10. In decision 2, the higher payoff is chosen if the number generated is 1 or 2 and the low payoff is chosen if the number generated is 3-10. By decision 10, the choice is between amounts of money that are fixed, either the high payoff or the low payoff. Because of this, we disregard subjects who choose B over A in decision 10, since they are choosing a low payoff over a high payoff, which suggested that they did not understand the game. A full copy of the experimental instructions may be found in Appendix B.

The decision where subjects switch from the safe option to the risky option can be used to categorize their risk tolerance. Subject's decisions in the experiment are used to define a range of values for the coefficient of relative risk aversion (CRRA). In the regression analysis, we use the mid-point of the range for the CRRA. Holt and Laury (2002) report ranges for a utility function of constant relative risk aversion in their paper. A value for the mid-CRRA that is less than zero indicate risk-seeking preferences, values that are greater than zero indicate risk aversion, and a mid-CRRA that is equal to zero indicates risk neutrality. Empirically, these designations conform closely with our intuitions regarding risk behavior. For instance, subjects that choose Option A in the first four decisions are generally considered to be risk neutral. Subjects that choose Option A more than four times are generally risk averse, and a risk-seeking (or risk "loving") subject will choose the safe option fewer than four times.

All subjects completed the risk preferences experiment at the beginning of the session. Once subjects completed all 10 decisions, one decision was randomly chosen for payment using a number generator. In the first treatment, subjects and were paid after completion of the public goods experiment and the final survey questions.

Survey

Subjects were asked to complete a survey with demographic and behavioral information, such as volunteering contribution. Some of this information was used in order to control for heterogeneity in our data. This included gender (1 represents male, 2 represents female), number of friends involved in the experiment, whether they volunteer for social or environmental causes, religious and political affiliation, and major (different numbers were associated with the various qualitative answers each individual gave). A selected number of relevant survey questions used in this study can be found in Appendix A.

Theoretical Expectations

Theoretical expectations for an infinitely repeating public goods game were designed on the basis of an infinitely repeated prisoner's dilemma model. Not all public goods games have a boundary solution, but since our public goods game is modeled as a prisoner's dilemma game, we will treat the other three as a monolithic unit that is either absolutely cooperating or defecting, then we can find the Nash equilibrium for the infinitely repeated public goods game by finding the Nash equilibrium of the prisoner's dilemma game with the same parameter. Hence, the prisoner's dilemma game would be as follows:

		Players 2, 3, and 4	
		Cooperate	Defect
Players 1	Cooperate	10,10	2.5, 12.5
	Defect	12.5, 2.5	5,5

Each player can only cooperate or invest in this model, and can only base his strategies on two alternatives: either all other players cooperate, or none of them do. In this model, there is a higher collective payoff brought about by all players cooperating, and therefore cooperating would be a more efficient strategy. However, defecting while the other players cooperate provides higher individual payoffs, and hence under conditions with a known endpoint, we can expect widespread defection. However, under the circumstances used in our treatments, each player could conform to one of two potential strategies. The first, the *grim strategy*, a player cooperates for as long as the other player (in this circumstance, players) cooperate, but as soon as the other player defects, the first player begins to defect for the remainder of the game. In the context of the public goods game, this means that if any of the three players defects, the first player takes notice of the free rider, and ceases cooperating.

The second strategy is referred to as the tit-for-tat strategy. In this circumstance, a player simply follows the strategy employed by the other player in the previous round. For simplicity, we can assume that all players follow the same strategy. For this reason, whether we assume that each player follows the tit-for-tat or grim strategy has little practical implication, since a tit-for-tat strategy is effectively also a grim strategy, and there are no differences in payoff outcomes. If each player is operating under the tit-for-tat strategy, then one player defecting will lead to the others defecting, which results in the first player defecting in response, thus creating a feedback loop that appears to be identical to the grim strategy.

The game continues to the next stage with probability 1-p and stops with probability p. Using the probability p, we can determine the payoff to which a given strategy converges to. The probability of the session continuing discounts the expected value of all future earnings, and hence we can theoretically represent the expected value convergence as:

$$V^A = \pi_1 + \delta\pi_2 + \ldots + \delta^{N-1}\pi_N + \cdots + \delta^{\infty}\pi_{\infty} = \pi(1 + \delta + \delta^2 + \delta^3 + \delta^4 + \ldots . + \delta^{\infty})$$

Where δ is the discount factor exhibited by each additional round, and π representing the payoff before expected value estimations and is constant across all periods.

With five tokens, the strategy (Cooperate, Cooperate), which we shall refer to as the *Reward* (R), gives each player a payoff of 10. Since the probability of the experiment terminating in the given round is (5/6) The expected payoff for cooperating for all rounds converges to:

R(1+ δ + δ^2 + δ^3 + δ^4 + ….+ δ^{∞})=R/(1- δ)= 10/(1-(5/6))=59.998≈60

The expected payoff for cooperating is now compared to the expected value of defecting on the value gained from one player cooperating and the rest defecting (represented as the *Temptation*, or T) compared to the result if everyone defects (represented as the *Punishment,* or P). The T value would be 12.5, while P would be 5. The expected payoff for defecting for all rounds converges to:

T+ P(1+ δ + δ^2 + δ^3 + δ^4 + ….+ δ^{∞})=T+Pδ/1-δ=12.5+(5(5/6))/(1-(5/6))=37.49

Since the expected value of cooperating exceeds the expected value of defecting, the Nash equilibrium has the players cooperating indefinitely. It must be noted that in this theory, we assume all players use the grim trigger strategy. We assume that players either completely cooperate or completely defect, even though in the actual experiment, they would likely pursue a mixed strategy. However, the theory does hold even when players can use mixed strategies if we assume that the strategy to cooperate consists of any positive amount (1-5 tokens).

After round five in the non-intertemporal treatment for both waves, as well as in two sessions in the non-intertemporal treatment, we raised the probability of the game ending from one-in-six to five-in-six. The probability of the session continuing fell from .83 (83%) to .167 (16.7%), and therefore the Nash equilibrium changes. The expected payoff for cooperating for all rounds converges to:

$R(1+ \delta + \delta^2 + \delta^3 + \delta^4 + \ldots + \delta^\infty)=R/(1- \delta)= 10/(1-(1/6))=12$

The expected payoff for defecting for all rounds converges to:

$T+ P(1+ \delta + \delta^2 + \delta^3 + \delta^4 + \ldots + \delta^\infty)=T+P\delta/1-\delta=12.5+(5(5/6))/(1-(5/6))=13.5$

Hence, under these circumstances, the Nash equilibrium has the players defecting indefinitely.

Results

During the experiment for the non-intertemporal treatment, we had experienced subjects not attending the experiment in later rounds. In order to maintain the groups that they had left, we had replaced their contributions with robot players, in order to continue the same incentives for the subjects still involved in the experiment. These robot players were calculated on the basis of the average contribution by the other players, and were removed from the data once analysis had begun, and therefore only the actual player contributions were examined in the econometric analysis.

At the same time, we made a few errors in the beginning of the first wave of the non-intertemporal treatment, where we did not provide the subjects with information on group investment. This was due to my relative inexperience in the process of running the experiment, but as examined in the econometric analysis, the effect size of these errors (as a function of Treatment) were not significant enough to alter the contribution rates in a significant way.

We've included a table of summary statistics that compare our experiment with experiments that contain at least one treatment that is analogous to our experiment in some way, such as including four subjects per group and where a token invested is worth roughly .5 of a token kept. We cannot reasonably conclude on the basis of these comparisons that our experimental design is superior or inferior with regard to reducing free riding, but this provides a heuristic comparison nonetheless. Below we see that, compared to analogous experiments with known endpoints, we have relatively higher contribution rates.

As shown in Appendix D, total group investment was consistently positive across all rounds for all sessions while the probability for experiment continuing was 5/6 (.833). There were no indications of a downward trend in the mean group investment, which we would expect to see in a public goods experiment with a predetermined endpoint. When part II of the experiment was conducted and the probability of the experiment continuing fell to 1/6 (.1667), there is a downward trend in mean group investment in Wave 2 as well as the session run by Professor Anderson's Research Methods class (the probability change for part II occurred in Round 4 for waves 1 and 2, while for the Research Methods class, the change occurred in Round 11 for Group 10, and in Round 12 for groups 1 and 3).

Graphically, it would appear that the data supports our theoretical expectations that each subject would cooperate indefinitely as long as the probability for continuation was 5/6. When probability fell to 1/6, investment began to decline. However, econometric analysis will be necessary to determine the statistical significance of the relationship between contribution and the probability of continuation. The next section will also determine a statistically significant relationship between investment and any relevant variable, as well as a panel data and covariance analysis. In this section, it is important to explore how each variable affects the individual economic agent, and hence we will use individual investment as our dependent variable.

For both treatments, which together comprise seven sessions, we show a mean that is roughly half of each individual's total endowment, which as demonstrated by the data in Appendix D, remains consistent across several rounds until the probability of continuation drops. The standard deviation of the experiment is also relatively high, considering the total number of tokens each player has.

Table 1: Summary Statistics

	# of Observations	Mean	95% Conf. Interval	Standard Deviation
Carlen (2014)	590	2.285	[2.145, 2.420]	1.723

Below we compared the frequency of each token as a selected amount invested by each player under a high probability of continuation (P=5/6) and a low probability of continuation (P=1/6). Under high probability of continuation, the most common number of tokens invested per round by each player was two, followed by zero. The next highest number was five. The distribution table below demonstrates skewness with regard to player's decisions. While this gives some justification for our theoretical assumption that individuals will choose to either cooperate or defect, the table below shows players engaging in mixed strategies.

Under a low probability of continuation, the frequency distribution shows an even stronger positive skew. Although the sample size is much smaller, we see that zero was selected as the number of tokens invested the most frequently. The next highest was two. While we see evidence of mixed strategies under these conditions, the distribution was more concentrated due to lower contribution rates.

Table 2: Frequency of Token Selected

Probability	0	1	2	3	4	5
P=5/6	107	76	127	80	57	97
P=1/6	17	9	11	4	3	2

Below I compare the mean invested, as a percent of total endowment, to experiments with a known, unknown, and probabilistic endpoint. Since there are no other public goods experiments with a probabilistic endpoint, the experiments used for comparison are of the trust and prisoner's dilemma game variety. The experiments selected were either similar to our experiment or had a treatment that was similar to our experiment. Experiments with punishments, variation in information disclosure, and public monitoring were not used. The number of observations depends on the number of decisions made by each subject. For instance, if one subject made three decisions in three rounds, that would be three observations. It is difficult to make strong claims from these tables, as the experiments still vary from ours, especially the trust and prisoner's dilemma games. However, the tables below provide a rough indication of our relative success in increasing cooperation in public goods games.

Table 3.1: Data comparison against experiments with a known endpoint

	# of Observations	Invested (%)	Mean	95% Conf. Interval
Carlen (2014)	590	45.6	2.285	[2.145, 2.420]
Isaac et al (1998)*	220	29.1	***	***
Andreoni (1995b)*	400	44.1	***	***
Croson (1996)*	240	11.77	***	***

* Summary found in review by Zelmer 2003.

This supports our initial expectations that a probabilistic endpoint would result in higher contribution rates, with the exception of Andreoni (1995b). As explained in the literature review, cooperation levels declined in all three experiments as the session progressed across rounds. In our experiment, we experienced no such drawback until the probability for the continuation of the experiment dropped. Not only were subjects contributing significant percentages of their endowment, they were able to maintain that for the entirety of the experiment where the probability of continuation was 5/6.

Table 3.2: Data comparison against experiments with an unknown endpoint

	# of Observations	Invested (%)	Mean	95% Conf. Interval
Carlen (2014)	590	45.6	2.285	[2.145, 2.420]
González et al (2004)	1080	44.24	***	***
Sell and Wilson (1999)	320	43.0	***	***
Palfrey et al (1994)	630	46.3	***	***

In experiments with an unknown endpoint, we find that our experiment has roughly comparable results. Again, this provides no significant verification of our results, but it is important to note that the mean contribution rates in our experiment are significantly above zero.

Table 3.3: Data comparison against experiments with a probabilistic endpoint

	# of Observations	Invested (%)	Mean	95% Conf. Interval
Carlen (2014)	590	45.6	2.285	[2.145, 2.420]
Warnick et al (2004)	290	63.2	***	***
Pedro Dal Bo (2005)	975	30.0	***	***
Normann et al (2011)	455	59.1	***	***

For the probabilistic endpoint, cooperation seems comparable, and two of the three experiments demonstrated higher proportion invested than ours. However, the experiments in Normann et al (2011) and Pedro Dal Bo (2005) are prisoner's dilemma games, while Warnick et al (2004) is a trust game. These games are significantly different, in the sense that each game did not permit the opportunity of mixed strategies. Either players cooperated, or they defected. If the strategies involved some cooperation and some defection, the experimental design would be more similar.

Summary statistics indicate that the mean individual investment is 2.285, with a standard error of .07 and a standard deviation of 1.723. Using a covariance analysis, we construct a correlation matrix and find a positive correlation between group contribution and individual investment. This would appear logical, since this means that as other subjects in the group shirk and reduce their contribution, individual investment also declines as others react to the declining investment. However, we find a negative correlation between individual earnings. This will be explored along with the panel data analysis. A smaller, but positive correlation exists between continued probability and individual investment.

Econometric Analysis

In the following analysis, it was necessary to include a “lag” in the data for group investment and individual earnings. Each subject must first make their decision before they have this information for the given round, the decisions are made on the basis of the previous round. Hence, our data file lists the total group contribution and individual earnings for round 1 along with round 2. In our panel data analysis, this allows us to indicate if there is an effect these variables have on individual investment, and how large that effect would be. For this analysis, we measured the significance of the relationship and the effect size between individual investment and mid-CRRA, group contribution, individual earnings, and probability, as well as several demographic characteristics.

Table 2 on the next page presents the standard errors estimated from the fixed effects and random effects models. Model 1 is a fixed effects model that does not control for demographic characteristics or risk aversion. Model 2 is a random effects model, and also does not include controls for demographic characteristics. Model 3 is a random effects model and does include controls for demographic characteristics. Before making their decision in a given round, subjects are given information on how much they earned and how much other players invested in the previous round. Hence, earnings and group investment are lagged so as to accurately measure the effect size.

Table 4: Panel Data Analysis

	Model 1	Model 2	Model 3
Risk Aversion		.0555 (.1314)	.0195 (.1351)
Earnings	-.0342 (.0856)	-.6162** (.0583)	-.6239** (.0591)
Group Investment	.0693 (.0463)	.3398** (.0295)	.3414** (.0300)
Probability	.7094* (.3961)	.8211** (.3494)	.7956** (.3493)
Gender			-.1089 (.1249)
Number of Friends			.0216 (.0443)
Major			.0157 (.0442)
Religious affiliation			-.0459 (.0390)
Environmental affiliation			-.1137 (.0839)
Social affiliation			.0222 (.1129)
Political affiliation			-.0032 (.0710)
Session Controls	No	Yes	Yes
F-Statistic	2.27	5.77	7.01
R-Squared	.1913	.4667	.4701
N	590	590	590

* Indicates a p-value of less than .1 but greater than or equal to .05
**Indicates a p-value of less than .05

In this model, Models 2 and 3 are controlled for session variation. There were seven dummy variables for the seven sessions, with the last one being dropped. The first five sessions were run by Professor Anderson's Research Methods class, while the sixth and seventh sessions were waves 1 and 2 of the intertemporal treatment.

We must also decide whether a fixed effects or random effects model is preferable for measuring the effect size of a given variable. The fixed effects model is appropriate when unobserved heterogeneity is correlated with independent variables. That is, if covariates are uncorrelated with the errors, then the random effects model is unbiased and approximately equal to the fixed effects estimator up to sampling error. The random effects model is appropriate when unobserved heterogeneity isn't correlated with independent variables.

The way of choosing between a fixed and random effects model is by running a Hausman test. The Hausman test checks a more efficient model against a less efficient but consistent model to make sure that the more efficient model also gives consistent results. The Hausman test tests the null hypothesis that the coefficients estimated by the random effects estimator and the coefficients estimated by the fixed effects estimator are the same. If they are, then it is acceptable to use random effects. If we get a significant P-value, however, we will use fixed effects.

Because risk aversion is time invariant for all subjects, it must be dropped from the fixed effects model. Demographic characterisitics are also time invariant, and hence are also dropped. This is also expected for all independent variables in our experiment since the effect these have on individual investment varies from person to person. Based on the results from the Hausman test, unobserved heterogeneity cannot be expected to be correlated with independent variables. Therefore, we will use a random effects model for all variables. Details regarding the panel data analysis, both the variable identification and indication of the meaning of the variable names, are included in Appendix E.

For group investment, earnings, and continued probability, we rejected the null hypothesis, and hence can conclude that a statistically significant relationship exists between these variables and individual investment in models 2 and 3. Even when taking into account demographic characteristics, the null was still rejected. Demographics such as gender and affiliation with environmental or social organizations do not have significant effect on individual investment. However, the demographic variables affected the coefficients and standard errors of continued probability, individual earnings, and group contribution, as evidenced from the move between models 2 and 3.

It is surprising that the session was not found to have a significant effect on individual contribution. Our experiment involved an intertemporal and nonintertemporal public goods game, which did not vary individual investment as we had expected. What is not surprising is that continued probability did have a significant effect on individual investment, which was predicted by our theory. We can conclude that the sudden decline in our graphs that occurred when the probability of continuation fell sharply was largely driven by this probability change.

It is important to note, however, that earnings and total group investment also drove individual investment. This indicates that the players are significantly aware of the free rider phenomenon, and are engaged in a trigger strategy of a kind. Along with probability of continuation, individuals are largely driven by conditional cooperation.

Discussion and Conclusion

We see that our experiment demonstrates higher mean investment proportional to one's endowment than the surveyed experiments with a known endpoint, while results our comparable to those experiments surveyed with an unknown and probabilistic endpoints. Our data graphs demonstrated that, unlike comparative experiments with known endpoints in Table 3.1, our experiments did not result in a decline in investment until after the probability of continuation was dropped.

While evidence of mixed strategies exists, players often selected amounts closer to zero and five when investing. This gives some support to our theoretical approach, which assumed that individuals would either cooperate or defect. However, a more constructive theory will have included the possibility of mixed strategies.

In our Panel Data Analysis, we used random effects models, considering the fact that in experimental settings, the effect that any of these factors have on individual investment varies from individual to individual. Nevertheless, each of these variables has a statistically significant effect on individual contribution. We failed to reject the null hypothesis for all variables except for total group contribution, earnings, and the probability of continuation. This indicates that there was not a significant effect size resulting from session or demographic variation, and that risk aversion similarly represents an insignificant effect size. The data suggests that the variation between one round per session and multiple rounds in one session does not result in a statistically significant change in contribution rates.

We rejected the null hypothesis for group mean investment, probability of continuation, and individual earnings, indicating that these factors drive a large part of the individual investment. On the one hand, the probability of continuation declining drives the sudden decline in individual investment seen at the end of several sessions. On the other, players are engaged in conditional cooperation, which involves a trigger strategy that depends on earnings and group investment.

Our experiment verified that probabilistic endpoints have a positive effect to cooperation in public goods games as they do in prisoner dilemma experiments. We do find that our experiment has consistently positive contribution rates. We find that a high probability of continuation has a positive impact on individual investment, along with lagged group investment and individual earnings.

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Appendix A: Selected Survey Questions

What is your gender?

- Male
- Female

Which political party best represents your interests?

- Democratic
- Republican
- Libertarian
- Green
- Other

What best describes your religious affiliation?

- None
- Catholic
- Protestant
- Jewish
- Muslim
- Other religion

How many people participating in this experiment do you consider to be your friend?

During the past two years have you been a member, contributed time, or contributed money to a social organization (for example soup kitchens or Big Brother, Big Sisters)

a. Yes
b. No

During the past two years have you been a member, contributed time, or contributed money to an environmental organization (for example, a campus environmental group or the Nature Conservancy)

a. Yes
b. No

What is your primary academic interest area/major area?

a. Area I (Sciences)
b. Area II (Social Sciences)
c. Area III (Arts and Humanities

Appendix B: Lottery Choice Game

You will be making choices between two lotteries, such as those represented as "Option A" and "Option B" below. The money prizes are determined by the computer equivalent of throwing a ten-sided die. Each outcome, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, is equally likely. If you choose Option A in the row shown below, you will have a 1 in 10 chance of earning $6.00 and a 9 in 10 chance of earning $4.80. Similarly, Option B offers a 1 in 10 chance of earning $11.55 and a 9 in 10 chance of earning $0.30.

Decision	Option A	Option B	Your Choice
1st . .	$6.00 if the die is 1 $4.80 if the die is 2 - 10	$11.55 if the die is 1 $0.30 if the die is 2 - 10	A or B

Each row of the decision table contains a pair of choices between **Option A** and **Option B**. You make your choice by clicking on the "A" or "B" buttons on the right. Only one option in each row can be selected, and you may change your decision as you wish. Note: Note, try clicking on one of the radio buttons, then change by clicking on the other one.

Decision	Option A	Option B	Your Choice
1st . .	$6.00 if the die is 1 $4.80 if the die is 2 - 10	$11.55 if the die is 1 $0.30 if the die is 2 - 10	A or B

Even though you will make ten decisions, **only one** of these will end up being used. The selection of the one to be used depends on the "throw of the die" that is the determined by the computer's random number generator. No decision is any more likely to be used than any other, and you will not know in advance which one will be selected, so please think about each one carefully. This random selection of a decision fixes the row (i.e. the Decision) that will be used.

For example, suppose that you make all ten decisions and the throw of the die is 9, then your choice, A or B, for decision 9 below would be used and the other decisions would not be used.

After the random die throw fixes the Decision row that will be used, we need to obtain a second random number that determines the earnings for the Option you chose for that row. In

Decision 9 below, for example, a throw of 1, 2, 3, 4, 5, 6, 7, 8, or 9 will result in the higher payoff for the option you chose, and a throw of 10 will result in the lower payoff.

Decision	**Option A**	**Option B**	**Your Choice**
9th	$6.00 if the die is 1 - 9 $4.80 if the die is 10	$11.55 if the die is 1 - 9 $0.30 if the die is 10	A or B
10th	$6.00 if the die is 1 -10	$11.55 if the die is 1 - 10	A or B

For decision 10, the random die throw will not be needed, since the choice is between amounts of money that are fixed: $6.00 for Option A and $11.55 for Option B.

Making Ten Decisions: After you finish these instructions, you will see a table with 10 decisions in 10 separate rows, and you choose by clicking on the buttons on the right, option A or option B, for each of the 10 rows. You may make these choices in any order and change them as much as you wish until you press the Submit button at the bottom.

The Relevant Decision: One of the rows is then selected at random, and the Option (A or B) that you chose in that row will be used to determine your earnings. Note: Please think about each decision carefully, since each row is equally likely to end up being the one that is used to determine payoffs.

Determining the Payoff: After one of the decisions has been randomly selected, the computer will generate another random number that corresponds to the throw of a ten sided die. The number is equally likely to be 1, 2, 3, ... 10. This random number determines your earnings for the Option (A or B) that you previously selected for the decision being used.

Instructions Summary

To summarize, you will indicate an option, A or B, for each of the rows by clicking on the "radio buttons" on the right side of the table.

Then a random number fixes which row of the table (i.e. which decision) is relevant for your earnings.

In that row, your decision fixed the choice for that row, Option A or Option B, and a final random number will determine the money payoff for the decision you made.

Payoffs will be made in cash.

DECISION PAYOFF STRUCTURE

Decision	Option A	Option B	Your Decision Circle One
1	$6.00 if the die is 1 $4.80 if the die is 2-10	$11.55 if the die is 1 $0.30 if the die is 2-10	**A or B**
2	$6.00 if the die is 1 -2 $4.80 if the die is 3-10	$11.55 if the die is 1-2 $0.30 if the die is 3-10	**A or B**
3	$6.00 if the die is 1-3 $4.80 if the die is 4-10	$11.55 if the die is 1-3 $0.30 if the die is 4-10	**A or B**
4	$6.00 if the die is 1-4 $4.80 if the die is 5-10	$11.55 if the die is 1-4 $0.30 if the die is 5-10	**A or B**
5	$6.00 if the die is 1-5 $4.80 if the die is 6-10	$11.55 if the die is 1-5 $0.30 if the die is 6-10	**A or B**
6	$6.00 if the die is 1-6 $4.80 if the die is 7-10	$11.55 if the die is 1-6 $0.30 if the die is 7-10	**A or B**
7	$6.00 if the die is 1-7 $4.80 if the die is 8-10	$11.55 if the die is 1-7 $0.30 if the die is 8-10	**A or B**
8	$6.00 if the die is 1-8 $4.80 if the die is 9-10	$11.55 if the die is 1-8 $0.30 if the die is 9-10	**A or B**
9	$6.00 if the die is 1-9 $4.80 if the die is 10	$11.55 if the die is 1-9 $0.30 if the die is 10	**A or B**
10	$6.00 if the die is 1-10	$11.55 if the die is 1-10	**A or B**

Appendix C: Repeated Public Goods Experiment Instructions and Relevant Forms

Instructions

Matchings: The experiment consists of a series of **rounds**. You have been randomly assigned to a group with a total of 4 people – you and 3 other people. In each round, you will be matched with the **same** group of 3 other people. The decisions that you and the other 3 people make will determine the amounts earned by each of you. The identities of the other people in your group will never be revealed to you.

Investments: You begin each round with a number of "tokens," which may either be kept or invested. The 3 people you are matched with will decide how many of their tokens to keep, and how many to invest. You will not be able to see the others' decisions until after your decision is submitted.

Earnings: The payoff to you will equal:
$1.00 for each token you keep,
$0.50 for each token you invest, and
$0.50 for each token invested by the 3 other people who you are matched with.

Subsequent Matchings: You will be in the same group of 4 participants in all subsequent rounds, so the 3 other people you are matched with in one round are the same people that you are matched with in the next round.

Examples: Suppose you have only two tokens for the round, and the earnings from tokens kept, invested, and invested by the others are **$1.00**, **$0.50**, and **$0.50** respectively.

- If you keep both tokens, then your earnings will be: **$1.00 x 2 = $2.00** from the tokens kept, plus **$0.50** times the number of tokens invested by the other people in your group.
- If you invest both tokens, then your earnings will be: **$0.50 x 2 = $1.00** from the tokens invested, plus **$0.50** times the number of tokens invested by the other people in your group.
- If you keep one and invest one, then your earnings will be:
  $1.00 x 1 = $1.00 from the token kept, plus
  $0.50 x 1 = $0.50 for the token invested, plus
  $0.50 times the number of tokens invested by the other people in your group.

Note: In each of the 3 above cases, what you earn from the others' investments is: **$0.00** if the others invest 0 tokens, **$0.50** if the other people invest 1 token (in total) and keep the rest, **$1.00** if the other people invest 2 tokens (in total), etc.

You begin each round with an endowment of **5 tokens**, each of which can either be kept or invested. The 3 other people in your group will also have 5 tokens.

Everybody earns money in the same manner: **$1.00** for each token kept, **$0.50** for each token invested, and **$0.50** for each token invested by the 3 other people.

Once all investment decisions are recorded for a round, we will collect your decision sheet. Before your next meeting on Wednesday, we will calculate the total amounts invested in each group of 4 people and fill in the total amount invested and your earnings information for the round at the bottom of your decision sheet. We will return your decision sheet and your cash earnings to you next Wednesday.

At the end of the session today, we will throw a 6-sided die to determine whether or not there will be another round of decision making on Wednesday. If the throw of the 6-sided die is 1, the experiment will end. If the throw of the 6-sided is 2, 3, 4, 5 or 6, a new decision making round will begin. We will make a separate die throw for each of the 15 groups of 4 people who are participating. To protect the anonymity of the people in your group, no one will be told their group number. Thus, you will not know whether or not your will make a round 2 decision until we meet on Wednesday. If one of the die throws determines that your group will not make another round of decisions, this information will be included in your packet on Wednesday.

At the start of each new round, you will be given a new endowment of **5 tokens**. You are free to change the numbers of tokens kept and invested from round to round.

Anonymity: As noted above, the identities of the specific members of your group will never be revealed to you. In addition, you should not reveal your ID number or any information about this experiment to anyone else at the College. You will have an opportunity to ask the researchers questions about the study at the end of the semester.

Instructions Summary

- You will be matched with the **same** group of 3 other people in each round.
- All people will begin with **5 tokens** which they may keep (and earn **$1.00** each) or invest (and earn **$0.50** each), knowing that they will also earn **$0.50** for each token invested by other people in the group.
- You will begin each round with a new endowment of **5 tokens**, irrespective of how many tokens you may have kept or invested in previous rounds.

- At the end of each round we will throw a 6-sided die to determine whether or not there will be another decision making round. If the throw of the die is 1, the experiment will end. Otherwise, there will be another decision making round.

Part II Instructions

- Starting at the end of this round and at the end of every subsequent round, we will throw a 6-sided die to determine whether or not there will be another decision making round. If the throw of the die is 1,2,3,4, or 5 the experiment will end. Otherwise, if the throw of the die is 6, there will be another decision making round.
- All other features of the experiment will remain the same.
- You will be matched with the same group of 3 other people that you have been matched with since Round 1, and your group will remain the same for all additional rounds.
- All people will begin with 5 tokens which they may keep (and earn $1.00 each) or invest (and earn $0.50 each), knowing that they will also earn $0.50 for each token invested by other people in the group.
- You will begin each round with a new endowment of 5 tokens, irrespective of how many tokens you may have kept or invested in previous rounds.

Decision Sheet Your ID________

Round _______

Your token endowment this round = 5.

Please fill in the 2 blanks highlighted in yellow below:

Tokens Invested by You this Round = __________

Tokens Kept by You this Round = __________

Note: Sum of the 2 amounts must = _____5______

We will fill in information below this line at the end of the round.

Total Invested by all 4 People in Your Group this Round = ___________

Your Total Earnings this Round = $__________ (record this amount on your receipt form)

Based on the die throw for your group at the end of last round you will (will not) play another round today

ID Number

This packet contains a card with the ID number that will be used to link your decisions throughout the semester. To protect the anonymity of your decisions, this ID number will not be linked to your name. Thus, we will not be able to retrieve your ID number if you lose this card. It is very important that you keep the ID card in a safe place and bring it with you to every decision making session. You must show your ID card in order to be paid earnings from the experiment. You make take a picture of the ID card with your phone and show the picture to us to claim your earnings. If you lose your ID card and you do not have a picture of it, you forfeit any unclaimed earnings and your participation in the experiment will be terminated.

You may not give the ID card to anyone else to claim earnings or make decisions for you. You are on your honor as a student at the College of William and Mary that the ID card will only be used by you.

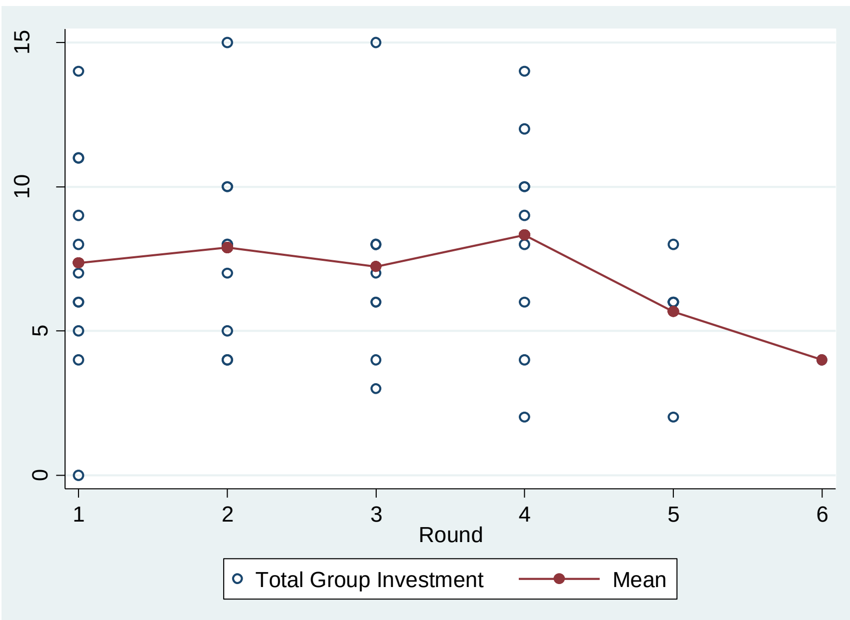
Please sign below that you have received a copy of this information regarding the ID card and that you agree to the conditions of use described above.

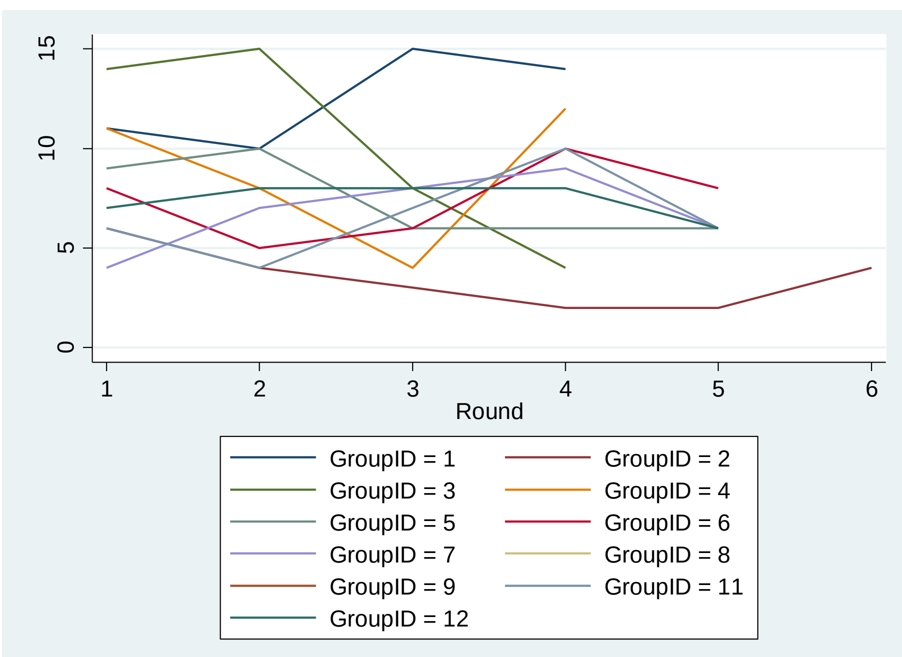
____ ________________

Sign Date

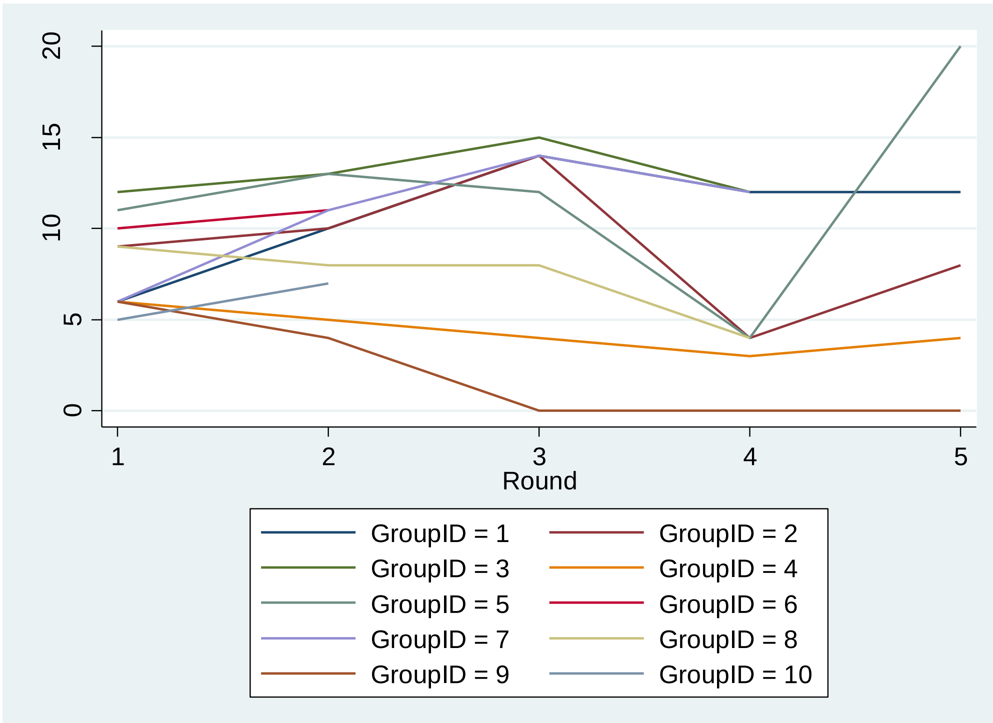
Appendix D: Total Group Investment across time, organized by session

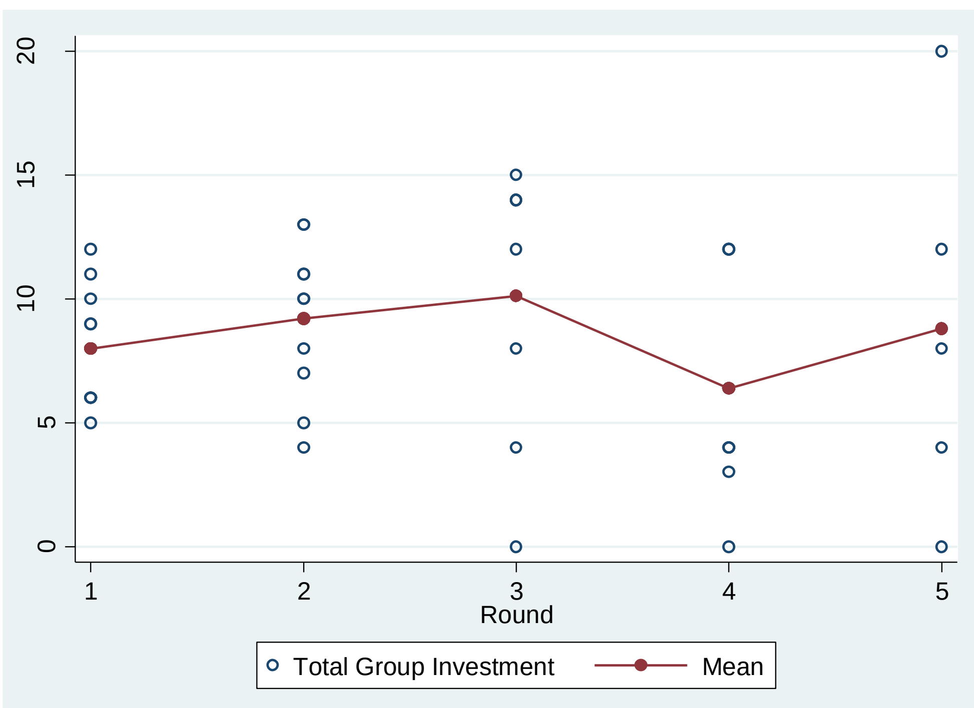
<u>Wave 1</u>



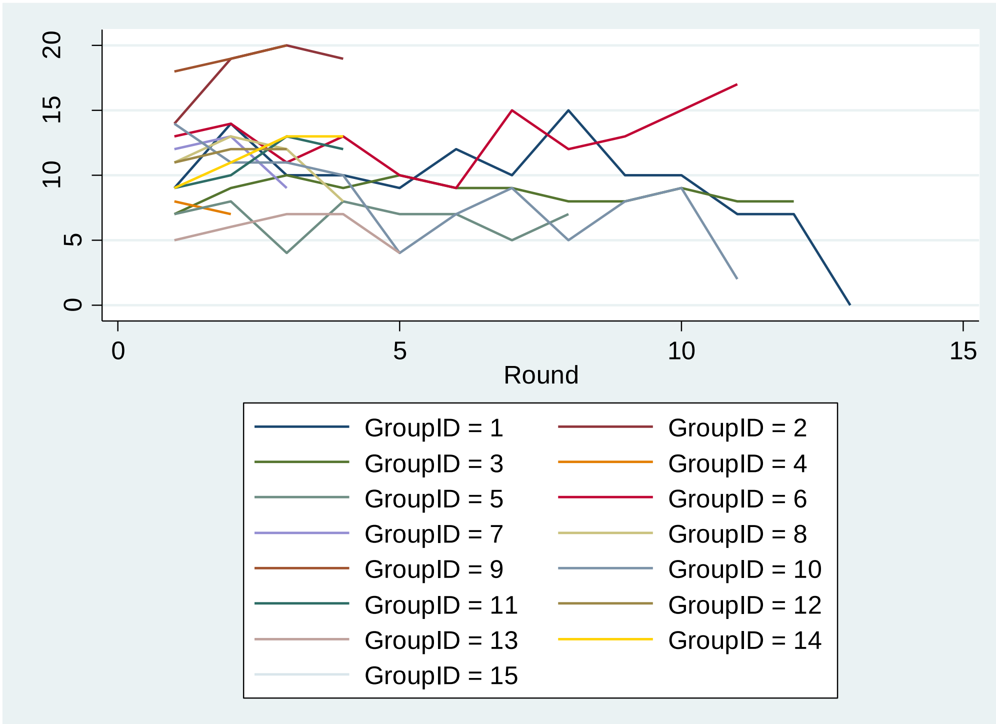


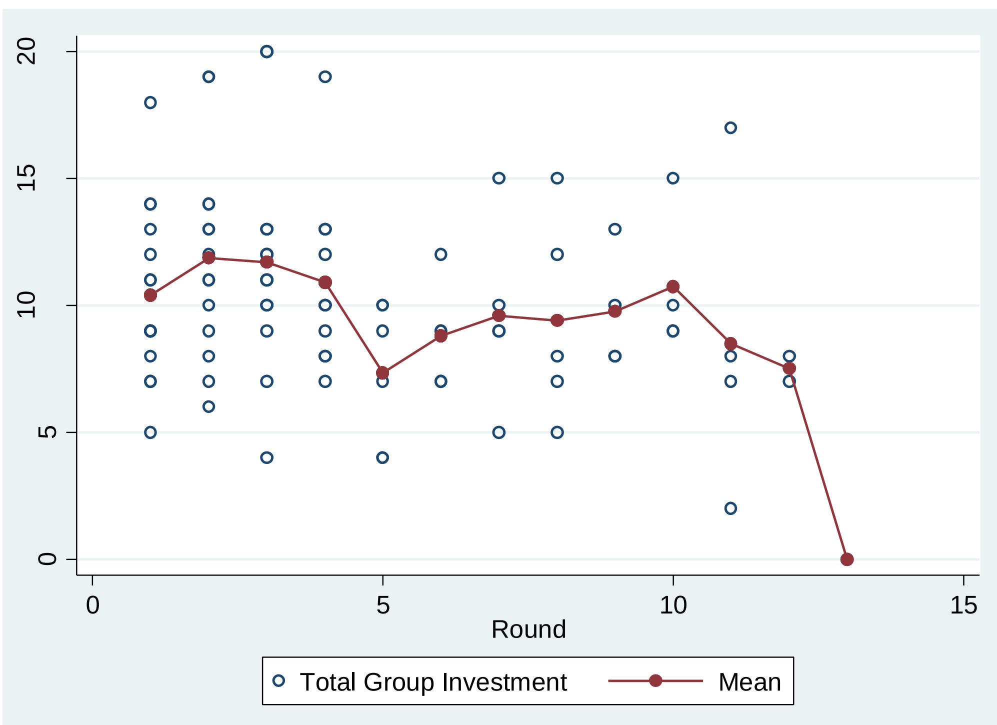
Wave 2


0
5
10
15
20
1
2
3
4
5
Round
GroupID = 1
GroupID = 2
GroupID = 3
GroupID = 4
GroupID = 5
GroupID = 6
GroupID = 7
GroupID = 8
GroupID = 9
GroupID = 10


0
5
10
15
20
1
2
3
4
5
Round
Total Group Investment
Mean

Econ 400 Research Methods Session:





Appendix E: Panel Data Analysis Variables

Variables	**Identification**	**Notes**
Round	Panel Variable	
ID	Time Variable	
Individual Investment	Independent Variable	
Total Group Investment	Dependent Variable	Group Investment
Risk Aversion	Dependent Variable	Mid-CRRA Measurment
Session	Dependent Variable	
Gender	Dependent Variable	
Earnings	Dependent Variable	Individual earnings
Probability	Dependent Variable	Probability of continuation
Major	Dependent Variable	
Political affiliation	Dependent Variable	
Religious affiliation	Dependent Variable	
Environmental affiliation	Dependent Variable	Whether volunteered for environmental group
Social affiliation	Dependent Variable	Whether volunteered for social group
Number of Friends	Dependent Variable	