Three essays on Experimental Economics

Lab evidence on preferences for redistribution, cooperation in games and voluntary contributions in public goods

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Contents

Ringraziamenti

È molto strana e particolare la sensazione che provo mentre scrivo queste righe, al termine della stesura di una tesi che rappresenta il prodotto di un percorso che ha toccato gli ultimi quattro anni della mia vita.
È stata un'avventura che mi ha segnato profondamente, probabilmente più di quanto potessi immaginare all'inizio, non solo in ambito accademico.
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Venezia però non è stata solo importante per l'Università. È una città che mi è entrata dentro e mi ha fatto conoscere delle persone che sono entrate a far parte della mia vita e della mia storia. In ordine strettamente cronologico, ci sono diverse persone che mi hanno lasciato davvero tanto.
Filippo Consoni è l'unica persona che già conoscevo quando sono arrivato a Venezia. Il nostro rapporto di amicizia è però aumentato fortemente, siamo stati coinquilini, prima a Mestre e poi a Venezia. È stato un privilegio non da poco avere in casa un amico con cui condivido una passione sfrenata per le lingue e le letterature straniere, oltre che un fedele compagno di pizza, birra e partita dell'Atalanta, la squadra della nostra città che ci ha accompagnato durante il secondo lockdown.
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Sensazioni simili a quelle che provo oggi per Venezia le ho provate per Torino, una città che mi ha radicalmente cambiato. Sono arrivato con una laurea triennale in Economia Aziendale dall'Università di Bergamo e me ne sono andato quattro anni dopo con un master in Economics del Collegio Carlo Alberto che mi ha permesso di arrivare a Venezia. A Torino ci sono diverse persone che hanno continuato a fare parte della mia vita e che mi hanno accompagnato anche da quando non vivo più lì.

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I came in on tiptoe, with a very different background from all my colleagues. I remember very well a strong initial feeling of inadequacy and I thank all the people who over the years have given me confidence, helping me to grow and improve year after year.

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Alongside the research activity at the PhD there was the teaching activity. With teaching, it was love at first sight, to the point that I said several times that "I would pay to teach". So I thank all the students I had for three years in classes and at endless office hours. I think I really gave what I could with respect to my skills, but I certainly believe that I have also received a lot in terms of respect and affection, there are days and moments that I will always hold within me.

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Filippo Consoni is the only person I already knew when I arrived in Venice. However, our friendship has grown strongly. We were flatmates, first in Mestre and then in Venice. It was no small privilege to have a friend in the house with whom I share an intense passion for foreign languages and literature, as well as a faithful companion of pizza, beer and the Atalanta match, the team of our city that accompanied us during the second lockdown.

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I spent my third year in another city, Vienna, for what was supposed to be an academic visiting and then turned into one of the most intense experiences of my life, for better or worse. Despite the bad start, with three weeks spent in a hospital, I have great memories of Vienna since I got out. Although because of the isolation, I spent much of the time alone, it was a time of great introspection with endless walks in the Prater and in the city center. Vienna won me over as soon as I had the opportunity to get to know it, and whenever possible, I met fantastic people inside and outside of the University. I would like to thank all those who helped me and with whom I also spent beautiful moments, especially Elena Bruni, Rupert Sausgruber, Giulia Fonelli, Radka Zalomova, Lucia Mack, Sophie Foessleitner, Michele Pezzotta, Laura Toma, Andrea Vaccaro, and Stefan Halbauer.

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Similar feelings to those I have today for Venice I had for Turin, a city that radically changed me. I arrived with a bachelor's degree in Business Administration from the University of Bergamo and left four years later with a master's degree in Economics from the Collegio Carlo Alberto, which allowed me to arrive in Venice. In Turin, several people have continued to be part of my life and have accompanied me even if I no longer live there.

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However, the city where I grew up is Bergamo. In Bergamo, I had one of the most beautiful and important experiences of my life when I coached the Under 14 kids of Lussana Basket with whom we won the championship in the 2012/2013 season. Coaching has been fundamental training for me and has given me a huge advantage in managing many situations when I started teaching. All the guys I have coached have a special place in my mind and heart. I would like to thank, in particular, Silvio Devicenzi, who, with incredible trust, gave me the opportunity to make that experience, and Nicola Barcella, with whom a beautiful relationship of friendship and trust has developed over the years.

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Chapter 1

Introduction

The present dissertation explores three different issues using the methodology of experimental economics. Specifically, results are presented from three experiments conducted in a laboratory environment. One experiment was conducted in a laboratory with the physical presence of participants, and the other two experiments were conducted online, remotely, due to restrictions caused by the COVID-19 pandemic.

The data obtained in the laboratory potentially have the advantage of being able to isolate and evaluate the causal effect of the variation of a single variable on an outcome, keeping everything else constant and avoiding, therefore, the risk of confounding effects present in administrative or survey data. The main limitation of the laboratory data is, however, represented by their external validity, or the degree to which the results obtained can be extended to the real world. The theoretical ideal solution would be to carry out field experiments, experiments in the real world. However, this procedure often has limitations of a financial nature (due to high costs) as well as ethical concerns. Experiments in the laboratory are now valuable support for research in the social sciences. This paper has used this type of analysis in order to test the hypotheses related to three issues related to economic research.

In Chapter 2 we study how preferences for the redistribution of individuals vary according to the relative importance of luck in the income accumulation process (which depends partly on a random shock and partly on an ability test), the different interests at stake, and the experiencing of roles of players with different interests and information available.[1] We find that self-interest is the main driver of the choices of the individuals when they have direct monetary interests in the redistribution process. When leaving subjects under the veil of ignorance about their relative wealth position in the society, uncertainty damps selfish behavior. We also observed that, when asked to express their preferences as impartial spectators who have no personal stake in the outcome, subjects are inequality averse and sensitive toward fairness reasons in the treatment with the veil of ignorance, while less inequality averse in the treatment where there is perfect information about wealth ranking in the society. Finally, having more experience as players who perform better on the ability test increases the demand for redistribution when luck makes the most skilled player the least wealthy.

In Chapter 3 we experimentally investigate the role of temptation and efficiency in shaping the ability of subjects to cooperate in two indefinitely repeated one-shot games with anonymous random matching, the Stag Hunt (SH) and the Prisoner's Dilemma (PD).[2] Additionally, we test the existence and direction of behavioral spillovers between these two strategic games. Taking the results of Duffy and Fehr (2018) (D&F) as a baseline, we find that increasing efficiency lead to an increase in cooperation both in SH and PD. Similar to D&F, temptation positively affects cooperation in the SH while there are no significant effects in the PD.

Behavioral spillovers between SH and PD are rather limited. We find positive behavioral spillovers from SH in the first stage of subsequent PD when temptation is low. However, the transfer is not persistent as subjects decrease cooperation in the subsequent rounds of the PD, but, in our experiment, where efficiency is high, this decreasing in cooperation is lighter than in D&F where efficiency is low; high efficiency with low temptation delays the return to defection.

Chapter 4 aimed at investigating the effects of endowment and preference heterogeneity on coordination, cooperation, and welfare in a setting with multiple threshold

[1] Chapter 2 is based on joint work with Michele Bernasconi and Valeria Maggian. The author is particularly grateful to Michele Bernasconi and Valeria Maggian for valuable discussions and comments and to Andrea Albarea for the assistance during the experiment.

[2] Chapter 3 benefited from key comments and suggestions of Marco LiCalzi. Special thanks go to John Duffy and Dietmar Fehr for sharing their experimental programs and to Max Grossmann for all the advice and technical support in implementing the online experiment.

public goods.[3] In treatments with homogeneous endowments, group members contribute the same amount and, regardless of their preferences over the public goods, no alternative stands out to be more salient. Instead, in treatments with heterogeneous endowments, the wealthiest agent makes substantially higher contributions, and her preferred public good becomes a viable coordination device for the rest of the group. In terms of welfare, financing the public good preferred by the wealthiest agent is not only beneficial for all group members but also reduces within-group inequality, thus making the most of the benefits from successful coordination concentrate on lower endowment classes.

Chapters 2-4 can be read independently, each providing a separate introduction and conclusion.

[3] Chapter 4 is based on joint work with Luca Corazzini, Tommaso Reggiani and Christopher Cotton.

Chapter 2

Distributive Justice and Perception of Fairness: an experimental study

2.1 Introduction

Individuals' perceptions of fairness and preferences for redistribution are critical nowadays, where most of the countries have experienced a significant increase in income inequality in the last 30 years (see Piketty, 2014; Sarfati, 2015; Akbaş et al., 2019). For this reason, income inequality and preferences for redistribution have been at the center of policy and academic debates and represent widely investigated research topics in philosophy as well as in the other social sciences (see Aristotle, 2000, Hobbes, 1980, Smith, 1759, Paine, 2004 and Rawls, 1971).

In this paper, we investigate subjects' preferences for redistribution depending on i) their personal stake in the outcome (either absent or not), ii) the relative weight of luck and merit in affecting income inequality, and iii) whether individuals are informed about their relative wealth position in the society or not.

Over time, various distribution theories have attempted to describe the fundamental principles according to which one distribution might be preferred to another (see the Literature Review for more details). Evaluating the preferences and sensitivities of individuals for these theories becomes a complex undertaking since cultural aspects, past experiences, perceptions, and personal interests can alter them significantly. Preferences for redistribution have thus been studied by introducing the concept of the *impartial spectator*, an individual who has no personal monetary interest or incentives to prefer any distribution of wealth, and of the *veil of ignorance*, where individuals' decisions are taken without being aware of own relative wealth in the society.

The impartial spectator is a well-known and extensively used tool to define and evaluate theories of distributive justice and can be traced to Hume (1751) but also to Smith (1759). In *The Theory of Moral Sentiments*, Adam Smith claims that human beings are characterized by an innate interest in the fortune and misfortune of other people and by a desire for sympathy with others[1]. Individuals, however, are not sit in a vacuum but rather possibly share the same experiences in different life moments, so that our paper aims at investigating whether having a direct experience of the economy affects the distributive preferences of otherwise external observers. The first novelty of this experiment lies in the fact that it provides the subjects who are called upon to express themselves as external observers with direct experience in the setting they have to evaluate. We are therefore interested in assessing the effect on external observers of being able to directly experience, in previous rounds, the setting they are asked to evaluate.

Moreover, in most previous literature (see Gee et al., 2017), the relative importance of different sources of income is separately investigated, such that income distributions are either entirely due to luck or to effort. Differently, in our experiment, we aim to resemble a more realistic environment in which the income-generating process is partly due to the outcome of an ability test and partly to a random component[2].

Additionally, we also investigate how having (or not) a perfect knowledge of one's own relative wealth position influences individuals' choice to redistribute income. This is crucial because one's own definition of distributive justice is indeed influenced by own material consequences in a given context, a condition that is not met when referring to the theoretical concept of the *original position*, in which individuals evaluate social institutions behind a *veil of ignorance* i.e., they do not know their own relative wealth position in the society (see Gaertner and Schokkaert, 2012). Harsanyi (1953) argues

[1]Recent research in Neuroscience on mirror neurons supports this perspective, providing evidence that humans have an innate capability to understand the mental states of others at a neural level (see Kiesling, 2012).

[2]To the best of our knowledge, only Cappelen et al. (2017) consider together these sources of income but in a different setting and with different objectives than us.

that, in such a case, the opinions expressed would be free from constraints and distortions and that a rational decision-maker would opt for an *utilitarian* decision rule. Differently, according to Rawls (1971), individuals' principles of justice are driven by the *difference principle*, so that inequalities are justified only if their presence improves the conditions of the worst-off[3].

In our laboratory experiment, we render a society formed by three individuals randomly re-matched in each period. In each group, at the beginning of each period, a member is randomly assigned to the role of the external observer, whose earnings are fixed. The gross income of the other two group members depends instead i) on their outcome in an ability task and ii) on a random component, which might be of high or low intensity and that affects their income in a way such that if it is positive for one subject, it is negative for the other. Both the individuals' performance in the ability task and the realization of the random component, which is of common knowledge, define the level of inequality among group members, resulting in four possible states of the world.

In the *Baseline treatment* each individual is asked to vote for a re-distributive scheme, being perfectly informed about own relative wealth position in the society, a condition that is not met in the Veil Of Ignorance (VOI) treatment, where group-members not drawn as external observers are not informed of their performance (i.e. income) in the ability task.

We find that self-interest is the main driver of behavior when individuals have a direct interest in the re-distributive scheme and are perfectly informed about their relative position in the society. The same result also applies when individuals are uncertain about their relative position in the society, once controlling for beliefs. Surprisingly, when subjects are drawn as external observers, they have slightly different behavior in the VOI treatment with respect to the baseline treatment. In particular, in the VOI treatment, we observe evidence of both inequality aversion and fairness: individuals, when playing the role of external observers, ask for a higher redistribution as inequality

[3]The Rawlsian veil is much thicker than the one proposed by Harsanyi (1953): individuals do not know anything even about the characteristics of the society.

increases and when the random component leads to a re-ranking of wealth positions among the best and worst performer in the ability task. In the baseline treatment, we observe only slight evidence of inequality aversion but no evidence of fairness.

Finally, we also find evidence that the type of role experienced when subjects are not extracted as external observers has an effect on redistribution preferences: subjects who perform better on the ability test tend to redistribute more when the luck shock reverses the test result when they are external observers.

2.2 Literature Review

According to the approach of standard theoretical models (Meltzer and Richard, 1981, Hotelling, 1929 and Downs, 1957), individuals are rational subjects with perfect information, and their preferences for redistribution are driven by self-interest alone. The median voter theorem applies to these models, for which individuals who benefit from redistribution should support it while individuals who would lose money from the redistribution should be against it. Thus, in a world of high inequality, with a large majority of poor people, the standard models predict that the demand for redistribution should be high. However, several empirical papers (see Alesina and Giuliano, 2011) have shown how these models, while capturing a crucial element in comprehensively explaining the preferences of individuals for redistribution, are unable to explain the data observed in the real world. For example, although inequality in income distribution in the US is higher than in Europe, redistributive policies are more extensive in the latter country than in the former one (Alesina and Angeletos, 2005). Following the approach of Durante et al. (2014), we can identify two other main motives for the demand for redistribution, in addition to the already described self-interest: risk aversion and social preferences.

In the presence of uncertainty about future income and sufficiently persistent tax regimes, risk aversion can be a key determinant of the demand for redistribution, as even the richest individuals can insure themselves against negative income shocks; some experiments found support for risk aversion in determining the demand for redistribution (see Beck, 1994, and Schildberg-Hörisch, 2010).

Social preferences, on the other hand, concern the possible willingness of individuals to correct distributions they do not consider fair or reasons of efficiency. Several works have shown that the demand for redistribution decreases in the loss of efficiency (see Krawczyk, 2010, Durante et al., 2014 and Beckman et al., 2004).

Fairness has been studied with multiple theoretical and experimental contributions in the field of distributive justice (see Konow, 2003 for a detailed survey). Cappelen et al. (2017) outlines three main ideals of fairness: egalitarianism, libertarianism, and liberal egalitarianism. Egalitarianism implies that no inequality can be justified within a society; regardless of the marginal contribution of individuals, everyone should get an equal share of total production and wealth. According to libertarianism, instead, a fair distribution should reflect precisely the contribution of each subject, but then any form of inequality is acceptable. Liberal egalitarianism, instead, represents an intermediate position where inequalities are acceptable only when they depend on factors within the control of the subjects. In contrast, inequalities that depend on factors beyond the control of the subjects are unacceptable.

Rawls (1971) significantly criticizes an idea of distributive justice based solely on merit because even the distribution of talents would result from an arbitrary lottery of nature. Therefore, a distribution that reflected only this characteristic would still be morally arbitrary (see also Sacconi, 2011 and Becchetti et al., 2011). Therefore, Rawls (1971) suggests that inequality in favor of the more able would be justified when they, through their abilities, help the whole society to improve, particularly the condition of the poorest.

A very different, libertarian approach is that of Robert Nozick (see Nozick and Williams, 2014). In his entitlement theory, he asserts that as long as assets are acquired by subjects respecting the principles of justice in the acquisition or not violating the rules of transfer, no intervention of redistribution of resources would be justifiable, not even on the basis of merit.

As pointed out by Becchetti et al. (2011) the experimental method may thus play an important role in this debate about distributive justice to verify which of these visions of justice find consensus among people, not just in their survey answers but also in their actual behavior in randomized experiments where their choices affect monetary payoffs. Previous evidence suggests that the *source* of wealth plays a crucial role in affecting re-distributive preferences (see Leventhal and Michaels, 1971, Hoffman and Spitzer, 1985).

There is an extensive literature of laboratory experiments that investigate the role for the source of income, and of whether whether the income is obtained by effort/ability or luck, has in driving the preferences for redistribution (see Balafoutas et al., 2013, Fong, 2001 Durante et al., 2014, Krawczyk, 2010, Fong and Luttmer, 2011, Lefgren et al., 2016). Even if there have been some conflicting results (see Ku and Salmon, 2013), the vast majority of the experiments show that inequality obtained after performing an ability or effort task leads to a lower support for redistribution (Lefgren et al., 2016; Hoffman et al., 1994; Ruffle, 1998; Cherry et al., 2002).

Two other aspects highlighted and analyzed in the literature of inequality and fairness are the concepts of inequality aversion and perception of inequality. Inequality aversion, defined as the disutility arising from differences between one's own payoff and other's payoffs (Engelmann and Strobel, 2004), independently from the source of income, has been investigated both experimentally and theoretically (see Atkinson et al., 1970). Finally, several experimental contributions (see Norton and Ariely, 2011, Cruces et al., 2013, Kuziemko et al., 2015) highlight how perceived inequalities are often substantially different than their actual size. In a relevant theoretical paper, Alesina and Angeletos (2005) show how two otherwise identical societies can end up in two very different levels of inequality and redistributive schemes, depending on the perception that individuals have on merit and justice compared to chance and luck in determining the distribution of gross income.

Our experiment contributes to the literature on inequality and preferences for redistribution in several directions. We keep constant efficiency when determining income distribution and focus on the role of fairness and inequality aversion. In this setting, fairness is to be interpreted as the relative importance that factors as ability and effort have with respect to luck and randomness in shaping the income distribution and the consequent level of inequality. With respect to inequality aversion, we rely on the definition of Engelmann and Strobel (2004) as the disutility arising from differences between one's own payoff and other's payoffs. Specifically, in our repeated setting, we constantly alternate the role and interests of the players. In addition, the income of each period is composed of two parts, one coming from ability, one from luck, and the relative weight of the two components changes in each period.

Our experimental framework allows us to study the role of the experience in the game and the role of different degrees of uncertainty may have on the choices of external observers.

A work that, like ours, tries to jointly investigate inequality aversion and source of inequality is that of Gee et al. (2017). In their experiment, they find that an increase in inequality has less impact on demand for redistribution when income is earned through performance than when income results from luck. They interpret this difference as people taking earned income as a signal of deservingness and, thus, not increase their support for redistribution in response to an increase in inequality when income is earned through effort. In their experiment, the game is one-shot and does not allow to see how players behave over periods or when they alternate their roles. Also, in the treatment where incomes are assigned based on performance, they use absolute cutoffs to assign players to top, middle and low incomes. The redistribution voted then can take on very different characteristics depending on the performance achieved by the group, but, at the time of voting, subjects have feedback and know their position, not the composition of the group. Like Dengler-Roscher et al. (2018) our setting lets us understand whether people try to maintain consistency between their expressed fairness ideals as impartial spectators with respect to when they have a material interest. In Dengler et al. (2018), they first assign resources after a real-effort task, then manipulate the order in which subjects decide how to allocate the resources, whether with or without personal stakes in the outcome, however they do not consider a repeated setting like us. In the partial allocation task, like a standard dictator game, participants determine the earnings for themselves and another participant. In the impartial allocation task, the participant determines the earnings for two other participants. In line with previous findings and also to our findings, they observe that participants allocate more to themselves than what they have earned when choices have direct payoff consequences for themselves. They also find that the order of decision matters only among participants who have not participated before in allocation experiments.

2.3 The Experiment

The experiment consists of two main between-subjects treatments: the Baseline and the VOI (Veil of ignorance). The experiment consists of twelve rounds. In the Baseline treatment, we randomly group participants into groups of three individuals, with a random re-matching at the beginning of each of the 12 rounds.

Each group of three individuals is composed of three types of players: player A, player B, and player C. The role of player B is randomly assigned, in each group, at the beginning of each round.

In contrast, the roles of Player A and Player C depend on the performance of the remaining two group members in an ability task, with the best (worst) performer getting the role of Player A (Player C).[4]

More specifically, the ability task consists of five closed-ended questions.[5] Each question has one correct answer, three wrong answers, and one *"I do not know"* option. Players have two minutes to answer the five questions and, for each question, they get a +1 score if they choose the right answer, a −1 score if they select the wrong answer, and finally, a 0 score if they choose the *"I do not know"* option.[6]

[4]The role of Player B is assigned before the ability task, and this is common knowledge in order to avoid individuals not to put the effort in their performance if expecting that with a one-third probability their outcome will be irrelevant. However, during the ability task, player B has the opportunity to read the questions that the other two players are answering.

[5]The questions were chosen from very different fields like mathematics, psychology, history, general culture, grammar and required a different degree of skills, knowledge, and effort. The questions were taken from pre-selection tests used for entry into Economics, Business and Psychology universities.

[6]If both group-members get the same score in the ability task, the subject who spent less (more) time in answering the questions is assigned the role of Player A (Player C). If the two group members

While Player B gets a fixed income equals to 75 points, Player A and Player C gross income depends i) on their performance in the ability task, with player A getting 100 points and player C getting 50 points[7] and on ii) a random component, which value is only communicated to participants once types A, B, and C are assigned.

The random component consists of a sum of points that are added to Player A and subtracted to Player C or vice-versa, meaning that the random component of income is of *opposite* sign for the two players. The magnitude of the random component might be of two different levels, with equal probability, in each round. In case a high-intensity random component applies, ±40 points are added to Player C and subtracted to Player A or subtracted to Player A and added to Player C. In case a low-intensity random component applies, ±20 points are added to Player C and subtracted to Player A or subtracted to Player A and added to Player C.

To sum up, in each round, four possible distributions of gross income or *states of the world* can emerge with equal probability (25%). In the following, we will refer to them as *Luck_20_A*, *Luck_20_C*, *Luck_40_A* and *Luck_40_C* as shown in Table 2.1:

When comparing the effects of the high- intensity and low-intensity random components, we observe that in the latter, the relative wealth position of players is kept constant with respect to the outcome of the ability task. Differently, when considering the effects of the high-intensity random component, its realization could cause a re-ranking of players, making player C (player A) becoming the richest (the poorest). After each member of each group is informed of the distribution of gross income in their group, each member of each group will have to make a choice. In particular, each participant will have to select the level of tax rate they would like to be applied to their group's gross income distribution. To facilitate the choice, for each tax rate, the distributions of net income that would occur for each member of the group if each possible rate were implemented are presented to each player. More specifically, each

also spend the same amount of time answering the questions, the computer randomly chooses the assignment of types A and C. However, given the precision of the software in determining the subjects' response time (i.e., time is counted in milliseconds), the use of this random draw was never necessary during the experiment.

[7]The conversion rate used in the experiment is such that 10 points = 0.1 Euros

Table 2.1: Four states of the world are implemented with equal probability in each round: *Luck_20_A*, *Luck_20_C*, *Luck_40_A* and *Luck_40_C*. Player B always has a fixed income of 75. The random component of the gross income is always opposite in sign for player A and C. *Inequality* is equal to the difference, in absolute value, between the gross income of player A and player C.

State of the World	Player	Ability	Random Shock	Gross Income	Inequality
	A	100	+20	120	
Luck_20_A	B	/	/	75	90
	C	50	-20	30	
	A	100	-20	80	
Luck_20_C	B	/	/	75	10
	C	50	+20	70	
	A	100	+40	140	
Luck_40_A	B	/	/	75	130
	C	50	-40	10	
	A	100	-40	60	
Luck_40_C	B	/	/	75	30
	C	50	+40	90	

player has to vote a tax rate from 0%, which preserves the status quo, meaning that the distributions of gross and net income are identical, up to 100%, which involves a completely egalitarian distribution of net income, equals to 75 points for each player. All intermediate rates, from 10% to 90%, measured in intervals of 10% points each, allow a reduction in the inequality between group members.

Depending on the state of the world, for each possible tax rate level, participants are informed about the consequent distribution of net income, as shown in Table 2.2:

Table 2.2: Each of the four boxes identifies a state of the world. For each state of the world, the table shows the distribution of net incomes that would be obtained for each possible tax rate level.

Luck_20_A	Net Incomes		
Tax Rate	A	B	C
0%	120	75	30
10%	115,5	75	34,5
20%	111	75	39
30%	106,5	75	43,5
40%	102	75	48
50%	97,5	75	52,5
60%	93	75	57
70%	88,5	75	61,5
80%	84	75	66
90%	79,5	75	70,5
100%	75	75	75

Luck_20_C	Net Incomes		
Tax Rate	A	B	C
0%	80	75	70
10%	79,5	75	70,5
20%	79	75	71
30%	78,5	75	71,5
40%	78	75	72
50%	77,5	75	72,5
60%	77	75	73
70%	76,5	75	73,5
80%	76	75	74
90%	75,5	75	74,5
100%	75	75	75

Luck_40_A	Net Incomes		
Tax Rate	A	B	C
0%	140	75	10
10%	133,5	75	16,5
20%	127	75	23
30%	120,5	75	29,5
40%	114	75	36
50%	107,5	75	42,5
60%	101	75	49
70%	94,5	75	55,5
80%	88	75	62
90%	81,5	75	68,5
100%	75	75	75

Luck_40_C	Net Incomes		
Tax Rate	A	B	C
0%	60	75	90
10%	61,5	75	88,5
20%	63	75	87
30%	64,5	75	85,5
40%	66	75	84
50%	67,5	75	82,5
60%	69	75	81
70%	70,5	75	79,5
80%	72	75	78
90%	73,5	75	76,5
100%	75	75	75

After each member of the group has confirmed his choice, the tax rate implemented to define the effective distribution of the group's net income for the round will therefore be the one preferred by the majority, that is the higher tax rate that at least two out of three individuals are willing to accept. In particular, if all three members of the same

group voted for the same tax rate, that tax rate is implemented. If two out of three members of the same group vote for the same tax rate, that tax rate is implemented. Finally, if the three members of the same group vote for three different tax rates (for example 10%, 50% and 70%), the implemented rate will therefore be equal to 50%, or the highest rate that at least two out of three individuals would be willing to accept. Each member of the group is therefore informed of both the tax rate implemented and the distribution of net income resulting from its implementation.
In a more formal way, for each agent:

$$z_i = (1 - \tau)y_i + M\tau$$

Where z_i is the net income of agent i, y_i is the gross income of agent i, τ is the implemented tax rate in the round, and M is the mean income of the group which is fixed and always equal to 75 points (the income of player B). In this re-distributive scheme, each agent pays a tax proportional to her gross income and then receives a transfer that is equal for all agents. So, for players A and C, the gross income y_i is given by:

$$y_i = Ability\ Test + Luck\ Shock$$

Where *Ability* is always equal to 100 points for player A and to 50 points for player C; *Luck Shock* can be, with equal probability, of high intensity ($\pm 40 points$) or low intensity ($\pm 20 points$) and it is of opposite sign for the two players. Another intuitive way of looking at this framework is proposed in Figure 2.1.
The overall wealth in the economy is always equal to 225 points that can be viewed as a *pie* that has to be divided among the three types of players. Player B has a fixed amount of 75 points which corresponds to one-third of the pie, and the other two players must divide the other two-thirds.

In the veil of ignorance (VOI) treatment, the only difference with respect to the Baseline treatment is that those two group members who are not assigned the role of

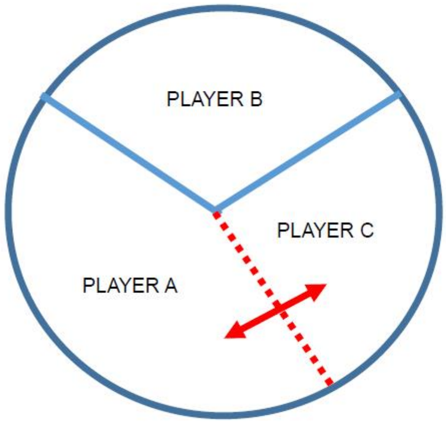


Figure 2.1: The overall pie, player B has always a fixed one-third.

Player B are not informed about their outcome in the ability task, meaning that they are not aware on whether they are Type A or Type C players. Instead, their beliefs about their type are elicitated, so that they are asked to select one option out of five, going from *"I believe I am player A"* to *"I believe I am player C"*[8]. At the end of each round, besides being informed about the implemented distribution of net income, they are also provided feedback on their type (either type A or type C).

2.3.1 Experimental procedures

The experiment was carried out at the CERME (Center for Experimental Research in Management and Economics) Laboratory at the Ca' Foscari University of Venice[9] in October 2019 and programmed with the *zTree* software Fischbacher (2007). We run 10 sessions, 5 sessions of the Baseline and 5 session of the VOI treatment. Each session involved 18 participants and lasted 12 rounds. At the end of the experiment, a random round was drawn and paid privately in cash to each participant. The average payment was 11.5 Euros, including a participation fee of 4 Euros. Sessions lasted about one hour and a half. The overall sample was composed of 180 individuals, 76 males (42%) and 104 females (58%). The mean age was 21.5, and more than half of the sample (67.22%) studied Economics or Management at Ca' Foscari University of Venice.

[8]The beliefs are not incentivized but self-reported. This is to avoid possible disincentive behaviors to perform at the best in the ability test.

[9]The experiment was conducted in Italian. An English version of the instructions is available in the Appendix.

2.4 Hypotheses

The aim of the experimental design is to test different hypotheses concerning subjects' behavior and their preferences for redistribution when they are type A, B, or C in the Baseline treatment and in the VOI treatment.

Hypotheses on Player B

Player B's gross and net income is always equal to 75 points, no matter the tax rate implemented. So, given that she has no direct monetary interest in the redistribution setting, standard economic theory would predict the absence of any specific regularity in her voting behavior. Therefore, according to the standard theory approach, we expect no differences in the tax rate voted by Player B in the various states of the world, both in the VOI and in the BSL treatment. Formally, we can then formulate the following hypotheses:

Hypothesis 1
When extracted as external observers, subjects are indifferent to the level of redistribution in the different states of the world. Moreover, they do not change their behavior significantly in the VOI treatment compared to baseline.

Evidence in favor of Hypothesis 1 would support the predictions of standard economic models that individuals without a direct economic incentive would have no incentive to prefer one level of redistribution over another. Other theories of distributive justice, however, have other predictions for subjects when extracted as external observers in this setting. Specifically, egalitarianism requires that, regardless of the state of the world, subjects vote for a 100% tax rate that would allow, in every state, each subject to receive the same level of income.
Applied to our setting, also according to the theoretical approach of Fehr and Schmidt (1999) the external observer should not be indifferent with respect to the inequality

level between players A and C. In particular, in this case, as there are no efficiency considerations, self-centred inequality-averse spectators should always choose 100% taxation.

Diametrically opposed, the libertarian prediction dictates that in every state, a redistribution equal to 0% is voted and that subjects accept the inequality of every state in the world. Somewhat in the middle is the liberal egalitarian view, for which, in every state in the world, the voted tax rate should be equal to that which most closely approximates the level of net income derived from the ability test. This level of tax rate corresponds to 40%, in the Luck_20_A state, 0% in the Luck_20_C state, 60% in the Luck_40_A state, and 100% in the Luck_40_C state. Fairness reasons, on the other hand, are related to the source of income, thus predict that in states of the world with high-intensity shocks (Luck_40_A and Luck_40_C), redistribution is greater than in states of the world with low-intensity shocks. In particular, fairness can be studied by analyzing the behavior of subjects in the state Luck_40_A where the luck shock, despite relatively low inequality, causes re-ranking by making player C the richest player. Finally, inequality aversion predicts that, regardless of the source of income, the demand for redistribution is greater and increases in gross income inequality.

The second part of Hypothesis 1, on the other hand, aims to check that subjects' behavior, when extracted as external observers, does not change in the VOI treatment compared to baseline. In fact, although the theory does not provide reasons to justify a different type of behavior, it is also true that the experience that players have when they are not extracted as external observers is different in the two treatments, and this difference could have a behavioral effect.

Finally, we are also interested in exploring whether and how much previous experience in the A and C role might matter in the decisions of the same players when they are drawn as B players.

Hypothesis 2

Subjects, when extracted as external observers, do not change their behavior significantly if they have more experience as an A or C player in the other rounds.

Hypotheses on Player A and C

According to standard economic theory, in the Baseline treatment, we expect players to vote according to their self-interest; the richest player (Player A in *Luck_20_A*, *Luck_20_C* and *Luck_40_A* and Player C in *Luck_40_C*) should choose a tax rate equals to 0% while the poorest player (i.e., Player A in *Luck_40_C* and Player C in *Luck_20_A*, *Luck_20_C* and *Luck_40_A*) should choose a tax rate equals to 100%.

Hypothesis 3
In the baseline, when players are of type A or C, they vote according to their *material interest*.

In the Veil of Ignorance (VOI) treatment, the theoretical prediction of the standard economic theory would be that each player, when not drawn as player B, votes according to her beliefs. The strongest one's belief of being Player A (Player C), the lowest (highest) the tax rate voted in the states of the world *Luck_20_A*, *Luck_20_C*, and *Luck_40_A*. Individual risk aversion plays a role, making players more cautious in their choices when being uncertain about their type.

Hypothesis 4
In the VOI treatment, when players believe they are of type A or C, they vote according to their beliefs, but the tax rate voted is less extreme than in the baseline because of the uncertainty of the real position.

2.5 Results

We begin our analysis by considering the behavior of players when drawn as type B. Then, we will focus on players A and C, both in the Baseline and VOI treatments. In the Appendix we provide additional Tables and Figures on the dynamics of the mean tax rate voted by subjects A, B and C in the different treatments and states of the world.

2.5.1 Inequality aversion and fairness of the external observers

State	Mean τ Voted BSL Treatment	Mean τ Voted VOI treatment	Inequality
Luck_20_A	64.37%	59.88%	90
Luck_20_C	55.12%	47.83%	10
Luck_40_A	68.70%	75%	130
Luck_40_C	60.79%	72.33%	30

Table 2.3: Average tax rate voted by subjects drawn as player B by state of the world and treatment.

Table 2.3 reports the average tax rate voted by subjects when drawn as player B in the four states of the world, both in the Baseline and VOI treatments.

In the VOI treatment, if we consider the state of the world *Luck_C_*40, where Player A (Player C) becomes the poorest (richest) because of the random component, the average tax rate voted by Player B is greater than the one voted in the state of the world *Luck_A_*20, even if the former is characterized by an inequality level three times lower than the latter.

Between the two treatments, it is interesting to note that Player B chooses a significantly higher tax rate in the VOI treatment than in the Baseline treatment only in the state of the world *Luck_C_*40, where the random component causes a re-ranking of wealth positions of Players A and C (Mann-Whitney rank-sum test, $p = 0.077$).

Within the two treatments, in the states of the world where the random component does not imply a re-ranking of the relative wealth position of Players A and C, we observe that the average tax rate voted is strictly *increasing* in the inequality level.

However, in the VOI treatment, the tax rate voted in each state of the world is significantly different from one another (except between *Luck_40_A* and *Luck_40_C*, see Table 2.4). In contrast, in the baseline, only the difference between the lowest (in *Luck_20_C*) and highest (*Luck_40_A*) tax rate voted becomes significant.

These differences in behavior in the two treatments suggest a possible impact of the VOI treatment in player B's choices as well. To explore this further, we decided to conduct the analysis of player B's redistribution choices separately in VOI and baseline. In Table 2.7, in order to control for other possibly relevant factors in determining the tax rate chosen by player B and to more formally test our hypotheses 1, 2, and 3, we report the results of a series of multi-level regression models, with standard errors clustered both at the session and at the individual level. While in the first three columns of Table 2.7 we focus on the Baseline treatment, in the last three columns, we only consider the VOI treatment.

BSL Treatment	Luck_20_A	Luck_20_C	Luck_40_A	Luck_40_C
Luck_20_A	/	1.042	-1.117	0.273
Luck_20_C		/	-2.503**	-1.180
Luck_40_A			/	1.515
Luck_40_C				/

VOI Treatment	Luck_20_A	Luck_20_C	Luck_40_A	Luck_40_C
Luck_20_A	/	1.913*	-3.212**	-2.439*
Luck_20_C		/	-4.602***	-3.445***
Luck_40_A			/	-.451
Luck_40_C				/

Table 2.4: Non-parametric two-side Mann-Whitney rank-sum test among the average tax rate voted in the four states of the world. Significance levels are denoted as follows: * $p < 0.1$, **$p < 0.05$, and *** $p < 0.01$.

Our dependent variable is the voted tax rate by Player B, which can take values in between 0 and 100, in ten percentage points. We use as independent variables *inequality*, a categorical variable that represents the difference in absolute value between the richest and the poorest subject in a given state of the world (with 10 being the omitted category). In order to check whether previous experience in the game, either as the richest or the poorest group member, affects an individual's willingness to ask for redistribution when acting as an external observer in the society, we include in the regression *ProportionA*, which identifies the number of times the individual was assigned the role of Player A in the ability task with respect to the total number of rounds played, not considering when she was assigned the role of Player B. By means of *Inquality*ProportionA* we interact with the above described variables, investigating whether a greater experience in the game as the richest group member differently affects the voted tax rate depending on the inequality level of the current state of the world. In columns 3 and 6 of Table 2.7 we also add as a series of control variables extracted from the post-experimental questionnaire. *Female* stands for the subject's gender while *period* takes into account the effect of experience in the experiment. *Economics* and *YearStudy* are respectively a dummy variable and an ordinal variable representing the subject's field and year of study, while *job* identifies whether the subject is regularly working or not. The variables *Incomefamily* measures whether the subject perceives her family's income as very low or very high on a scale from 1 to 10 while *familyTax* represents the tax rate imposed on the income of the subject's family from less than 10% to more than 60%, with 10 intervals in between. *Trust* indicates subjects' opinion on whether one's can trust others and is included in between 1 (not at all), to 10 (surely). Additionally, we measure an individual's beliefs on whether helping other people represents a moral obligation, by means of *helpothers*, which can take values from 1 (helping other does not represent a moral duty) to 10 (helping other does represent a moral duty). We also asked individuals whether they think rich people deserve their prosperity and whether poor people do not force themselves enough to improve their situation, measuring their answer on a scale from 1 to 10 throughout the variables *Deserve* and *Effort*. Each subject's increasing level of risk aversion is measured by *Risk*, which can take values from 1 to 10. Finally, *inequality reduction* indicates if the subject totally disagrees (equals to 0) or totally agrees (equals to 10) with the proposition that income inequality should be reduced in her country.

Table 2.5: Multi-level regression, the dependent variable is the tax rate voted by type B subjects. The first three columns consider the type B in the baseline, the last three columns the type B in the VOI treatment. Standard errors are clustered both at the individual and session level.

Tax_Rate_Voted	(1)	(2)	(3)	(4)	(5)	(6)
Luck_40_C	0.066	-0.105	-0.078	0.225***	0.202**	0.220**
	(0.047)	(0.080)	(0.081)	(0.052)	(0.092)	(0.092)
Luck_20_A	0.099*	0.082	0.078	0.129***	0.117	0.110
	(0.060)	(0.090)	(0.091)	(0.048)	(0.075)	(0.074)
Luck_40_A	0.135***	0.116	0.119	0.254***	0.274***	0.267***
	(0.049)	(0.073)	(0.073)	(0.043)	(0.067)	(0.067)
ProportionA	-0.073	-0.137	-0.154	0.023	0.031	0.028
	(0.065)	(0.109)	(0.108)	(0.056)	(0.129)	(0.129)
*Luck_40_C*ProportionA*		0.385***	0.336**		0.038	-0.009
		(0.146)	(0.147)		(0.168)	(0.169)
*Luck_20_A*ProportionA*		0.028	0.019		0.025	0.032
		(0.154)	(0.155)		(0.149)	(0.149)
*Luck_40_A*ProportionA*		0.004	0.015		-0.048	-0.059
		(0.133)	(0.133)		(0.139)	(0.139)
Period	-0.003	-0.006	-0.005	0.007	0.007	0.004
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.005)
female			0.076			0.019
			(0.055)			(0.054)
economics			0.016			-0.058
			(0.060)			(0.055)
Italian			0.192*			-0.019
			(0.112)			(0.079)
yearstudy			-0.027**			-0.004
			(0.012)			(0.013)
job			-0.018			0.039
			(0.040)			(0.033)
incomefamily			0.022			0.021
			(0.017)			(0.015)
familyTax			-0.002			-0.018*
			(0.011)			(0.010)
trust			0.025*			-0.020
			(0.015)			(0.014)
helpothers			0.012			0.019
			(0.012)			(0.013)
risk			-0.000			-0.032**
			(0.012)			(0.014)
deservingness			-0.010			-0.004
			(0.011)			(0.011)

effort			0.006			0.004
			(0.010)			(0.011)
life_satisfaction			-0.007			-0.010
			(0.015)			(0.019)
Constant	0.604***	0.657***	0.279	0.439***	0.435***	0.729***
	(0.065)	(0.077)	(0.217)	(0.055)	(0.067)	(0.232)
Log likelihood	-120.521	-115.707	-108.345	-80.380	-80.045	-70.064
Wald chi2	12.67	22.91	40.67	42.96	43.67	67.60
Prob >chi2	0.027	0.004	0.006	0.000	0.000	0.000
Observations	360	360	360	360	360	360
Number of groups	5	5	5	5	5	5

From the first column of Table 2.7 we can see that, in the Baseline treatment, subjects, when not involved in the redistribution scheme, are marginally sensitive to inequality. More specifically, they are more likely to ask for redistribution as inequality increases from *Luck_C_20* (the omitted state of the world) to *Luck_A_20* and *Luck_A_40*, where the absolute difference of gross income between the richest and the poorest player is equal to 130, as shown by the significant and positive coefficients of *Luck_C_40* and *Luck_A_40*. Similarly, our data do not provide strong evidence in favor of fairness motives in the Baseline treatment: when the random component causes a re-ranking of wealth positions of group members in the state of the world *Luck_C_40*, making inequality to be equal to 30, no significant difference in the tax rate voted is observed when comparing *Luck_C_40* to *Luck_A_20*, a state of the world characterized by an inequality equal to 90. Moreover, according to a Wald test performed on the state estimates after the regression, the mean tax rate voted in *Luck_C_40* is not different from the one voted in *Luck_C_20* ($\chi^2(1) = 0.00, p = 0.991$). Interestingly, in column 2, we observe that a greater experience as Player A (i.e. the group member with the highest performance in the ability task) makes the individual, when acting as player B, to ask for a lower higher level of redistribution in the state of the world *Luck_C_40*, as shown by the significant and negative coefficient of *ProportionA*Luck_C_40*, with this result being robust to the controls included in column 3.

In the same vein, no experience as Player A makes Player B to reduce the tax rate voted when the random component causes a re-ranking of wealth position between the best and the worst performer in the ability task in the state of the world *Luck_C_40* than in *Luck_C_20*, as shown by the negative but non significant coefficient of *Luck_A_20*. These results indicate that, when acting as external observers, in the baseline treatment individuals partially project their past "status" in their decisions: those who were more likely to be identified as the richest are indeed more likely to vote for redistribution in the state *Luck_C_40*.

Looking at the VOI treatment, in the fourth column of Table 2.7 we observe that the tax rate voted by Player B when the level of inequality between the poorest and the richest players is equal to 10 (in the state of the world *Luck_C_20*) is significantly lower than in all other states, so that inequality is an important driver of the decisions taken by Player B, as suggested by Hypothesis 1. In the VOI treatment, individuals seem also to be more concerned also about fairness. According to a Wald test performed on the state estimates after the regression, the mean tax rate voted in *Luck_C_40* is significantly different from the one voted in *Luck_C_20* ($\chi^2(1) = 3.03, p = 0.082$) despite the inequality level in the latter being three times lower.

Differently than in the Baseline treatment, previous "status" of Player B, identified by the variable *ProportionA* does not affect her choices as an external observer.

In column 5 and 6 we observe that only those player who have no previous experience as Player A choose a higher tax rate in *Luck_A_40* and in *Luck_C_40* than in *Luck_C_20*, as shown by the positive and significant coefficient of *Luck_A_40* and of *Luck_C_40*.

Among the control variables, in the column 6 of Table 2.7, it is interesting to note that *FamilyTax* negatively affects the voted tax rate: the higher the tax rate imposed on the income of the family of the participant, the lower the chosen tax rate.

These results suggest that individuals, even when their monetary interests are not at stake, are concerned by the level of inequality of a society. However, such an effect is affected by two main conditions. First, as previous studies have shown (see Becchetti et al., 2011), the veil of ignorance makes inequality concerns more relevant. However, we provide evidence that such an effect also applies to external observers, who are not directly affected by the redistribution of income. This might be due to the greater level of uncertainty of the situation with respect to the baseline, which makes individuals more likely to identify with others' positions. Additionally, this result is exacerbated when analysing fairness motives: the voted tax rate increases as the role of luck in the re-ranking the distribution of incomes as determined by ability becomes crucial, but only when subjects have not full knowledge of their status during the repetition of the game. Second, previous status of external observers plays a role in their current decision: those who were more likely to be the wealthiest in the society because of their higher ability, are also more likely to vote in favor in the richest when their income is not affected by the redistributive scheme. A possible explanation for the difference in the behavior of external observers in the baseline and in the VOI treatment may be related to the different degrees of the uncertainty of the two treatments and, consequently, to the different experiences that the external observers are exposed to. In the baseline, where there is perfect information about their position, subjects polarize their choices according to their personal interest when not extracted as external observers. In the VOI treatment, instead, the players, when not drawn as external observers, are uncertain of their position, and this could stimulate them to put themselves in the shoes of others and think more about other motivations, having to consider the possibility of being the other player.

Both of these behaviours can have an impact on the choices and preferences of subjects when extracted as external observers. In particular, in the VOI, being forced to think of being the player of the other type may have stimulated a greater attention to the dynamics of inequality and fairness that characterise the game. In the baseline, vice versa, thinking and voting systematically a tax rate strongly linked to one's own self-interest may have brought less attention to the same dynamics. Obviously, these are possible explanations and speculations on the results obtained.

Another possible explanation could be whether the external observer is characterized by let-down aversion (see Battigalli and Dufwenberg, 2007)[10]. According to this, it is "easier" to redistribute when the recipient is blind about her gross income so that in the baseline a lower redistribution should be observed because an aggressive redistribution would let down the damaged party.

Although the presence of let-down aversion may also help explain some of the results and the difference in external observer behavior of the two treatments, we think the two explanations can help both. Indeed, the presence of let-down aversion alone helps explain why the same inequality aversion is more pronounced in the VOI treatment. However, it would not explain why in the state of the world where re-ranking occurs relative to the ability test ordering we have high redistribution despite very low inequality.

Overall, then, we observe that players, when drawn as external observers are not indifferent to the state of the world that is realized, as predicted by standard economic models, but neither do they vote to eliminate inequality as predicted by egalitarianism and (in this case) by Fehr and Schmidt (1999). External observers in our experiment are sensitive to the level of inequality that is realized at the time of voting and to questions of fairness related to the different origins of incomes. However, these considerations are influenced by the experience they have in the rounds in which they were not external observers: in particular, the redistribution related to fairness increases in the external observers of the VOI treatment and for those who performed better in the ability test in previous rounds.

2.5.2 Players A and C vote according to their self interest in the Baseline

Table 2.6 shows the average tax rate voted by agents of type A and C in the Baseline treatment.

It is immediate to notice that their choices are different with respect to the preferences expressed by the same individuals when drawn as type B. In particular, we can

[10]We would like to thank Matteo Ploner who suggested this possible interpretation.

Mean τ Voted (Baseline)			
State	Player A	Player C	Inequality
Luck_20_A	12.71%	84.79%	90
Luck_20_C	13.33%	80.60%	10
Luck_40_A	16.93%	85.18%	130
Luck_40_C	90.70%	12.19%	30

Table 2.6: Average tax rate voted by type A and C players by the states of the world in the baseline.

see the major role played by *self-interest*: in the states where a player is the richest, she voted for a tax rate in between 12% and 16%, while when she is the poorest, she voted for a tax rate in between 80% and 90%, with these percentages not being significantly different depending on the level of inequality nor for Player A and neither for Player C.

Table 2.7: Non-parametric two-side Mann-Whitney rank-sum test among the average tax rate voted in the four states of the world. Significance levels are denoted as follows: * $p < 0.1$, **$p < 0.05$, and *** $p < 0.01$..

Player A	Luck_20_A	Luck_20_C	Luck_40_A	Luck_40_C
Luck_20_A	/	0.926	-0.342	-10.475***
Luck_20_C		/	-1.247	-11.631***
Luck_40_A			/	-12.546***
Luck_40_C				/
Player C	Luck_20_A	Luck_20_C	Luck_40_A	Luck_40_C
Luck_20_A	/	0.401	-0.545	9.796***
Luck_20_C		/	-1.144	10.488***
Luck_40_A			/	12.073***
Luck_40_C				/

In Table 7 we report the results of a series of multilevel regression models with standard errors clustered both at the individual and at the session level. In the first three columns of table 7 we only include the tax rate voted by subjects acting in the role of Player A, while in the last three columns we only consider those subjects who worst performed in the ability task (i.e. Player C).

Looking at the first and fourth columns, we observe that, with respect to the state of the world *Luck_C_20*, characterized by a difference of 10 points between the richest and the poorest player, only in the state of the world *Luck_C_40*, in which the random component causes a re-ranking of the income of the best and worst performers in the ability task, the voted tax rate is different. More specifically, the coefficient of *Luck_A_20* is significant but opposite in sign for the two types of player, positive for player A and negative for player C. This behavior can also be explained by selfish motives: in *Luck_C_40*, Player A, being the poorest group-member, asks for a high level of redistribution, while the opposite is true for Player C. Our analysis provides support for hypothesis 3: players A and C, in the Baseline treatment, are mainly driven by self-interest motives.

Table 2.8: Multi-level regression, the dependent variable is the tax rate voted in the baseline treatment. The first three columns consider the type A subjects, the last three columns the type C subjects. Standard errors are both clustered at individual and session level.

Tax_Rate_Voted	(1)	(2)	(3)	(4)	(5)	(6)
Luck_40_C	0.772***	0.837***	0.871***	-0.710***	-0.728***	-0.726***
	(0.032)	(0.092)	(0.094)	(0.037)	(0.066)	(0.066)
Luck_20_A	-0.032	0.060	0.083	0.052	-0.002	-0.009
	(0.040)	(0.119)	(0.120)	(0.047)	(0.071)	(0.070)
Luck_40_A	0.023	0.102	0.133	0.048	-0.025	-0.041
	(0.033)	(0.096)	(0.097)	(0.039)	(0.060)	(0.060)
ProportionA	0.014	0.103	0.132	0.050	-0.070	-0.094
	(0.060)	(0.108)	(0.109)	(0.071)	(0.122)	(0.121)
*Luck_40_C*ProportionA*		-0.099	-0.146		0.058	0.054
		(0.133)	(0.135)		(0.156)	(0.157)
*Luck_20_A*ProportionA*		-0.132	-0.157		0.173	0.205
		(0.157)	(0.157)		(0.190)	(0.190)
*Luck_40_A*ProportionA*		-0.115	-0.156		0.232	0.284*
		(0.131)	(0.132)		(0.146)	(0.147)
Period	-0.008*	-0.007*	-0.007	0.005	0.005	0.003
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
female			0.014			-0.000
			(0.034)			(0.041)
economics			0.001			0.025
			(0.037)			(0.046)
Italian			0.012			0.109
			(0.063)			(0.076)
yearstudy			0.011			-0.005
			(0.007)			(0.009)
job			-0.006			0.032

			(0.025)			(0.031)
incomefamily			-0.003			0.004
			(0.011)			(0.012)
fairtaxrate			-0.003			0.016*
			(0.006)			(0.008)
trust			-0.004			0.022**
			(0.009)			(0.011)
helpothers			-0.008			0.004
			(0.008)			(0.010)
risk			0.004			-0.013
			(0.007)			(0.009)
deservingness			-0.014*			-0.006
			(0.007)			(0.009)
effort			0.007			0.008
			(0.006)			(0.007)
life_satisfaction			0.006			-0.006
			(0.009)			(0.010)
Constant	0.184***	0.124	0.135	0.765***	0.807***	0.562***
	(0.064)	(0.089)	(0.151)	(0.050)	(0.059)	(0.161)
Log likelihood	30.421	30.927	34.987	-26.333	-24.851	-16.105
Wald chi2	897.27	901.11	928.02	613.41	622.26	656.09
Prob >chi2	0.000	0.000	0.000	0.000	0.000	0.000
Observations	360	360	360	360	360	360
Number of groups	5	5	5	5	5	5

2.5.3 The impact of uncertainty

We now turn our attention to hypothesis 4, which focuses on the behavior of subjects not drawn as type B in the VOI treatment. As explained in the "Experimental Design" section, players not drawn as type B do not receive any feedback after performing the ability task in the VOI treatment. Consequently, they do not know whether they are player A or player C when asked to vote for a redistribution rate, but we elicit their beliefs.

In Table 2.9 we present the average tax rate voted depending on subjects' beliefs about being player A or C.[11]

[11] In the experiment, group members who were not randomly drawn as Player B, were asked to

Table 2.9: Average tax rate voted by type A and C players by the states of the world in the VOI treatment.

	Mean τ Voted (VOI)		
State	I believe I am Player A	I believe I am Player C	Inequality
Luck_20_A	46.54%	78.26%	90
Luck_20_C	33.75%	72.22%	10
Luck_40_A	54.32%	82.25%	130
Luck_40_C	76.14%	43.67%	30

It is clear from the table that subjects' redistribution choices in the VOI treatment are strongly related to expressed beliefs. Therefore, the analysis is conducted by separating subjects who believe they are type A, those who believe they are type C, and the undecided. Table 2.2 reports, for each state of the world, the average tax rate voted according to the beliefs of subjects.

According to a set of two-side Mann-Whitney rank-sum non-parametric tests we observe that in each state, *Luck_A_20* ($z = -5.852$, $p = 0.000$), *Luck_C_20* ($z = -3.562$, $p = 0.000$), *Luck_A_40* ($z = -9.039$, $p = 0.000$) and *Luck_C_40* ($z = 4.600$, $p = 0.000$) the tax rate voted by the agents who believe being of type A are significantly less "extreme" than the ones in the baseline treatment by players that know they are type A. The situation is slightly different if we compare the tax rate voted by the players who believe they are type C and the players who know they are of type C in the baseline. In this case we have a significantly less extreme tax rate in the states *Luck_C_40* ($z = -4.575$, $p = 0.000$) and *Luck_C_20* ($z = 1.744$, $p = 0.081$) but not in the states *Luck_A_20* and *Luck_A_40*.

In Table 10 we investigate whether the tax rate voted in the VOI treatment is affected by the level of inequality and by individuals' fairness motives, taking into account both their beliefs, experience as player A and risk preferences. More specifically, in the first three columns of Table 10 we only include observations from those individuals who believe being of type A while in the last three columns we only in-

report their beliefs about being Player A or Player C on a 5 point rating scale scale from 1 (I'm sure to be Player A) to 5 (I'm sure to be Player C), immediately after the ability task. Table 2.9 reports the average tax rate chosen by those individuals who think being Player A (Player C) with a positive probability, meaning that those who choose 3 on the 5 point rating scale are not included in this analysis but in the following.

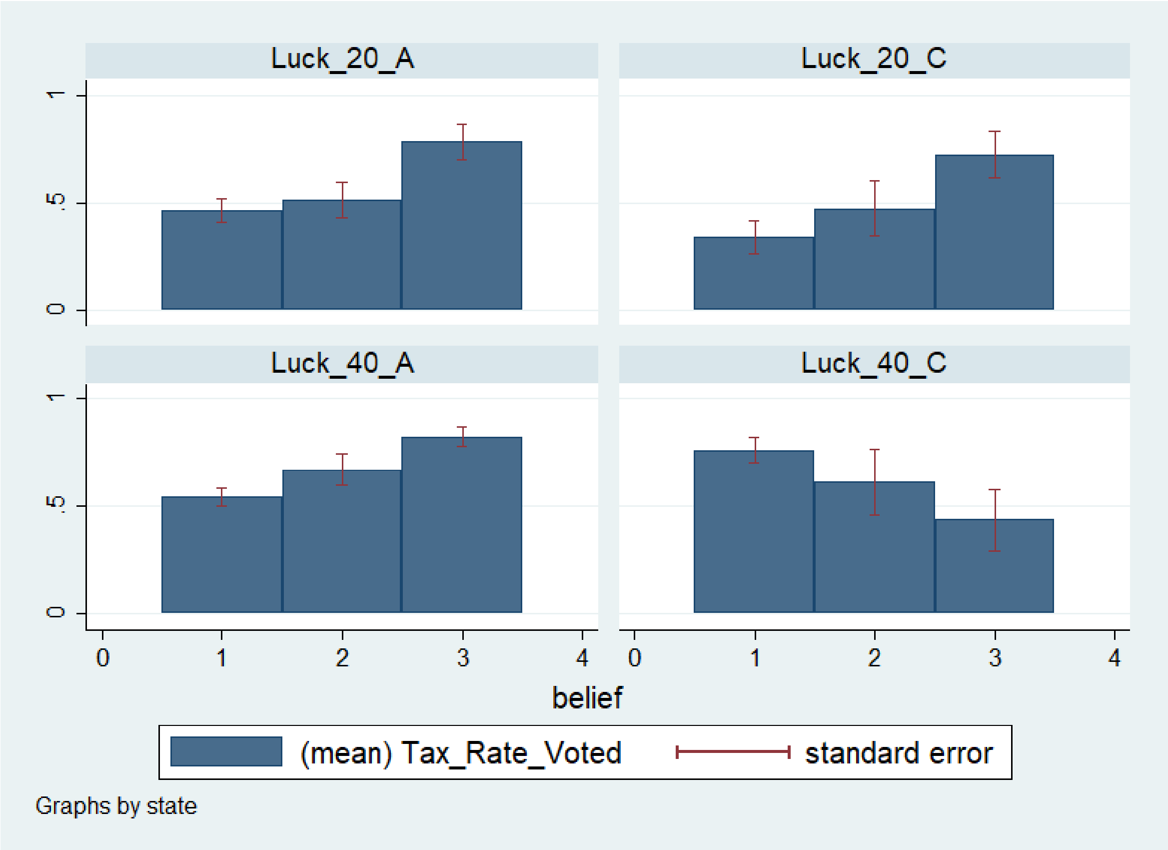


Figure 2.2: Mean tax rate voted according to the state of the world. In each graph the bar on the left represents the mean tax rate voted by subjects who believe they are of type A, the bar on the right represents the mean tax rate voted by subjects who believe they are of type C, while the bar in the middle represents the tax rate voted by subjects who do not have a clear belief about their position.

clude observations of those group-members who believe being of type C. In particular, *strong_belief* is a dummy variable which takes into consideration the strength of the belief of the individuals, which is equal to 1 if the subject strongly (weakly) believe of being a certain type of Player, either A or B.

Table 2.10: Multi-level regression, in the first three columns the dependent variable is the tax rate voted by subjects who believe they are type A in the VOI treatment, in the last three columns the dependent variable is the tax rate voted by subjects who believe they are type C in the VOI treatment. Standard errors are both clustered at individual and session level.

Tax_Rate_Voted	(1)	(2)	(3)	(4)	(5)	(6)
Luck_40_C	0.424***	0.312***	0.314***	-0.274***	-0.552***	-0.561***
	(0.043)	(0.085)	(0.084)	(0.063)	(0.098)	(0.098)
Luck_20_A	0.130***	0.119	0.116	0.035	0.031	0.034
	(0.042)	(0.075)	(0.075)	(0.056)	(0.096)	(0.096)

Luck_40_A	0.197***	0.228***	0.222***	0.091**	0.072	0.072
	(0.038)	(0.070)	(0.069)	(0.046)	(0.080)	(0.080)
ProportionA	-0.068	-0.086	-0.080	0.054	-0.046	-0.076
	(0.049)	(0.115)	(0.113)	(0.061)	(0.122)	(0.122)
*Luck_40_C*ProportionA*		0.204	0.186		0.765***	0.775***
		(0.145)	(0.145)		(0.194)	(0.194)
*Luck_20_A *ProportionA*		0.024	0.026		0.004	0.019
		(0.135)	(0.134)		(0.173)	(0.172)
*Luck_40_A *ProportionA*		-0.059	-0.062		0.033	0.051
		(0.126)	(0.125)		(0.138)	(0.137)
strong_belief_integer	0.171***	0.168***	0.171***	0.041	0.042	0.048
	(0.028)	(0.028)	(0.027)	(0.039)	(0.038)	(0.037)
Period	-0.002	-0.002	-0.004	0.005	0.005	0.007
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
female			0.044			-0.043
			(0.040)			(0.057)
economics			-0.107**			-0.073
			(0.042)			(0.053)
Italian			0.044			0.082
			(0.060)			(0.075)
yearstudy			-0.009			-0.006
			(0.010)			(0.011)
job			0.044			-0.016
			(0.028)			(0.041)
incomefamily			0.000			0.010
			(0.011)			(0.014)
fairtaxrate			-0.015*			-0.011
			(0.007)			(0.009)
trust			-0.001			-0.015
			(0.010)			(0.013)
helpothers			0.006			0.008
			(0.010)			(0.012)
risk			-0.013			0.009
			(0.010)			(0.013)
deservingness			-0.001			0.000
			(0.008)			(0.010)
effort			0.002			0.007
			(0.008)			(0.010)
life_satisfaction			-0.027**			-0.015
			(0.014)			(0.018)
Constant	0.280***	0.287***	0.667***	0.662***	0.711***	0.802***
	(0.062)	(0.078)	(0.177)	(0.062)	(0.077)	(0.227)
Log likelihood	-214.837	-212.068	-196.441	-29.164	-19.145	-14.210

Wald chi2	141.82	148.56	188.74	51.52	77.39	90.44
Prob >chi2	0.000	0.000	0.000	0.000	0.000	0.000
Observations	669	669	669	232	232	232
Number of groups	5	5	5	5	5	5

The opposite (positive for those who believe they are player A, negative for those who believe they are player C) and significant sign of *Luck_C_40* confirms us that subjects vote according to their beliefs. In the first column of Table 10, we can see that in presence of a veil of ignorance about the actual ranking of wealth among group members, *inequality* plays an important role in affecting the tax rate voted by those group members who believe being of type A. Indeed, we observe that the coefficients of *Luck_C_40* and *Luck_A_40* are positive and significant, which mean that individuals vote for a higher tax rate in, respectively, *Luck_A_20* and *Luck_A_40*, than in *Luck_C_20*, the omitted state of the world, a behavior that cannot be explained by self-interest. The strength of individuals' beliefs are also important, as shown in column 2 and 3 by the significant coefficient of *Strong_belief_integer*.

Differently, when looking at the behavior of those group-members who believe being of type C, we do not find strong evidence of inequality concerns. In the fourth column of Table 10, indeed the coefficient of *Luck_A_40* is only significant at 10% while there is no significant difference between the tax rate voted in *Luck_A_20*, characterized by an inequality level of 90, and *Luck_C_20*, the omitted category where inequality is equal to 10. Additionally, these results are not robust to the inclusion of control variables, as it can been seen in column 6. On the same vein, the strength of the beliefs play no role in affecting the voted tax rate.

Our results partially confirm our Hypothesis 4. While individuals who believe being of Type C are indeed voting to maximize their income, those individuals who believe being of type A are also driven by inequality aversion. Moreover, our results are robust even controlling for risk aversion.

As a final step of our analysis, we consider the determinants of the behavior of agents that believe to be player A or C with equal probability, that we refer to as "uncertain subjects". We think that this group of subjects could be particularly interesting to analyze. In fact, as they are quite uncertain about their type, they should not have selfish interests, and their behavior could be similar to the behavior of type B players, the external observers.

Table 2.11: Multi-level regression, the dependent variable is the tax rate voted by subjects who do not have clear beliefs of being A or C. Standard errors are both clustered at individual and session level.

Tax_Rate_Voted	(1)	(2)	(3)
Luck_40_C	0.166**	-0.027	0.015
	(0.075)	(0.131)	(0.131)
Luck_20_A	0.106*	0.241**	0.250**
	(0.064)	(0.114)	(0.113)
Luck_40_A	0.216***	0.283***	0.326***
	(0.058)	(0.105)	(0.105)
ProportionA	-0.029	0.052	0.090
	(0.087)	(0.152)	(0.152)
*Luck_40_C*ProportionA*		0.417*	0.389*
		(0.222)	(0.221)
*Luck_20_A*ProportionA*		-0.301	-0.300
		(0.195)	(0.194)
*Luck_40_A*ProportionA*		-0.155	-0.214
		(0.182)	(0.180)
Period	0.009	0.008	0.013*
	(0.006)	(0.006)	(0.007)
female			0.143**
			(0.065)
economics			-0.015
			(0.064)
Italian			0.228**
			(0.099)
yearstudy			-0.009
			(0.015)
job			-0.078
			(0.048)
incomefamily			-0.002
			(0.019)
fairtaxrate			-0.004
			(0.012)
trust			-0.016

			(0.018)
helpothers			0.006
			(0.017)
risk			0.003
			(0.017)
deservingness			-0.017
			(0.013)
effort			0.003
			(0.012)
life_satisfaction			-0.009
			(0.022)
Constant	0.381***	0.349***	0.251
	(0.078)	(0.099)	(0.279)
Log likelihood	-41.057	-35.332	-26.451
Wald chi2	17.37	30.18	52.02
Prob >chi2	0.003	0.000	0.000
Observations	179	179	179
Number of groups	5	5	5

In table 11 we report the coefficients of a series of multi-level regression models, with standard errors clustered both at the session and at the individual level. From column 1 it can be seen that these subjects exhibit inequality aversion, as demonstrated by the positive and significant coefficients of *Luck_20_A*, *Luck_40_C* and *Luck_40_A*. It is also very interesting to note that, in columns 2 and 3, subjects who have had more experience as type A redistribute more in the state *Luck_40_C*.

Moreover, for this group of uncertain subjects, *gender* plays a role: females redistribute significantly more.

Gender and beliefs: overconfidence of male and underconfidence of female

While not a central focus of our paper, the setting we constructed allows us to observe and analyze some differences in the behavior of men and women with respect to expressed beliefs and actual results on the ability test. Table 2.12 contains the distribution of the frequencies of the beliefs by gender. The rows indicate the "Belief elicitation" where 1 stands for *"I think I am player A"* and progressively goes towards

5 that means *"I believe I am player C"*. The two columns "Player A" and "Player C" divide the belief elicitation frequencies among the actual results. From this distribution, it is possible to see that men are more overconfident than women with 48% of players that believe they are player A when instead they are player C, against 28% of women[12]. Female, instead, are relatively underconfident with 32% of type A female players that believe they are type C, against only 20% of men[13].

These results are in line with previous findings in the literature (see Kamas and Preston, 2012). In a seminal paper, Niederle and Vesterlund (2007) examine whether men and women of the same ability differ in their selection into a competitive environment and find that men select the tournament twice as much as women, this result is not explained by performance, instead it is driven by men being more overconfident and by gender differences in preferences for performing in a competition.

		Who I actually am			
		Player A		Player C	
	Beliefs	Female	Male	Female	Male
Who I believe I am	I believe I am player A	42.63%	60.59%	28.69%	47.41%
	I do not know	25.79%	18.24%	30.33%	21.55%
	I believe I am player C	31.58%	21.18%	40.98%	31.03%

Table 2.12: Distribution of Frequencies of the belief elicitation, by gender.

2.6 Conclusion

In this paper we conduct an experiment to see how preferences for the redistribution of people change as we vary the interests at stake, the relative weight of luck and merit in income accumulation, and different experience as a type of player.

We show that when subjects have a direct stake in the game and are perfectly informed about their position, self-interest is the main driver of their choices: subjects ask for high redistribution when they can benefit from it and for low redistribution

[12] According to a non-parametric two-side Mann-Whitney rank-sum test, this difference is significant ($z = 3.483, p = 0.001$)

[13] According to a non-parametric two-side Mann-Whitney rank-sum test, this difference is significant ($z = -2.225, p = 0.026$)

when they can loose from it. We then introduce an element of uncertainty by asking subjects in the VOI treatment to express a preference without providing them with feedback regarding their actual position in the game. In this case, it was possible for subjects to form expectations about their performance on the ability test that would determine their actual position. Therefore, we control for subjects' beliefs and observe that indeed their voting behavior is consistent with the beliefs they expressed.

Again, self-interest is predominant, but there is also evidence of inequality aversion on the part of subjects who think they are the player who performed better in the test.

In any case, even though they vote consistently with respect to their beliefs and relative self-interest, the veil of ignorance has an impact: the tax rates voted are generally less extreme than those where there is perfect information.

Additionally, subjects, when asked to express a distributive preference as external observers, behave with some differences in the baseline and in the VOI treatment. In particular, in the VOI treatment the subjects, when extracted as external observers clearly show inequality aversion and fairness. Instead, in the baseline treatment, subjects, when extracted as external observers, show only inequality aversion but to a lesser extent. One possible explanation for this difference lies in the different degrees of uncertainty between the two treatments. From a theoretical point of view, players, when they are extracted as external observers, do not have different incentives in the two treatments. However, it is also true that, from a behavioral point of view, the experiences they have when they are not extracted as B subjects are different; they can lead to different perceptions that then can somehow influence the behavior when they are extracted as external observers.

Subjects, in the presence of perfect information, can be stimulated to think exclusively about their own interest, reflecting less on the level of inequality or the sources of it. This attitude could therefore be reflected in some way when they are called upon to express a judgment as external observers, approaching what are, in fact, the predictions of standard economic models with little sensitivity to inequality and fairness.

Conversely, in the treatment with uncertainty, people are systematically forced to put themselves in the shoes of both the richest and the poorest subjects. Since they have to form expectations about their relative wealth position in the society, they are possibly more likely to think about how the sources of income and the consequent inequality level affect the distribution of wealth in the society. As a result, they are more likely to reflect more on the dynamics of the game, the sources of income, and the inequalities that result. We speculate that these results might be consistent with inequality aversion and fairness being significant elements for subjects extracted as external observers in the treatment with uncertainty. An additional aspect that emerges from our work relates to the impact that experimenting more often (or not) with the role of the player who performs better in the ability task has on preferences for redistribution. Interestingly, we note that in several cases, when drawn as external observers, having experience as the player who performs better in the test significantly increases the demand for redistribution when the luck shock causes a re-ranking and makes the worst performing player the richest. Further identifying the relative weight of equity and inequality aversion motivations in the choice of external observer is surely a major goal for future research from these findings. In particular, future treatments could compare the results obtained with treatments in which the income distribution depends only on luck or only on talent to try to make disentangle between the two effect. Another possible research direction could be to provide players with accurate feedback on the difference in points made in the skill test, not just a ranking. In this way, one could assess whether and how differently subjects weigh more or less similar test results.

2.7 Appendix

2.7.1 Additional Tables and Figures

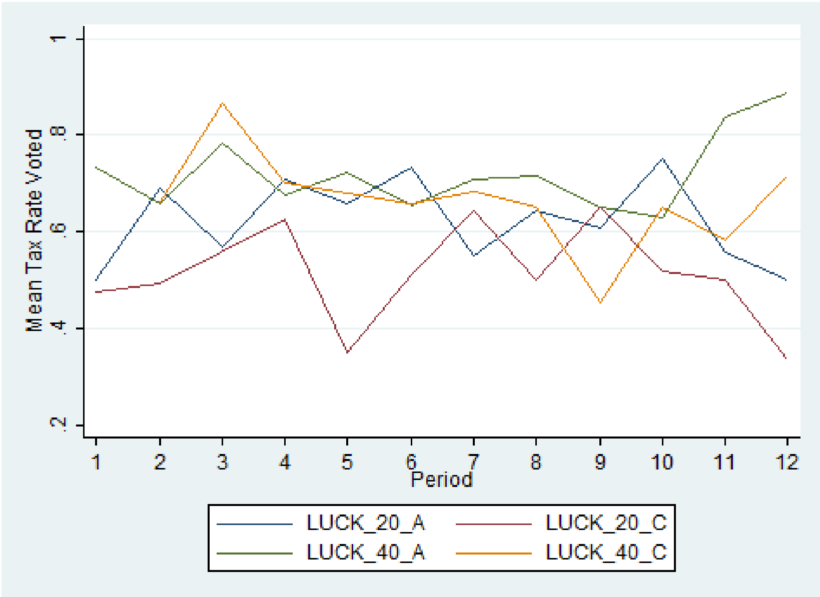


Figure 2.3: Mean tax rate voted by the subjects when drawn as type B, by period and state of the world.

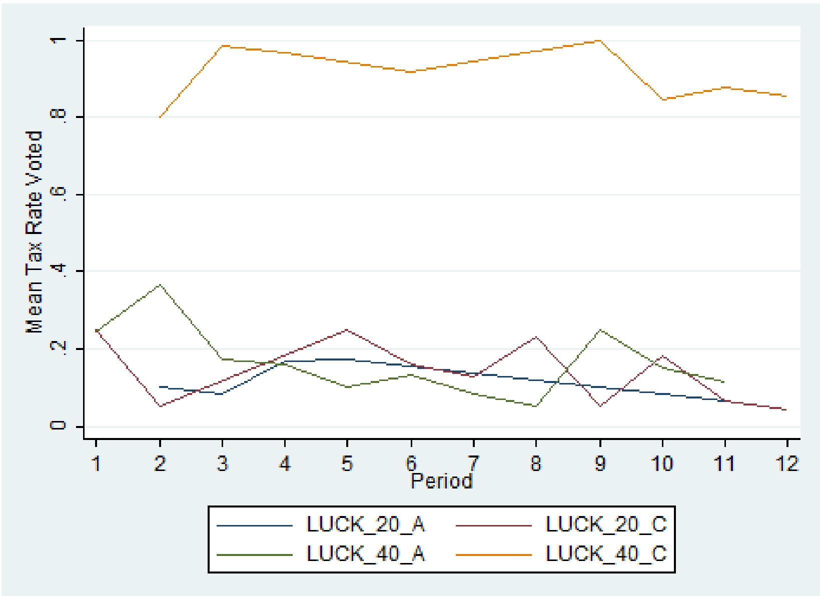


Figure 2.4: Mean tax rate voted by the subjects when drawn as type B, by period and state of the world.

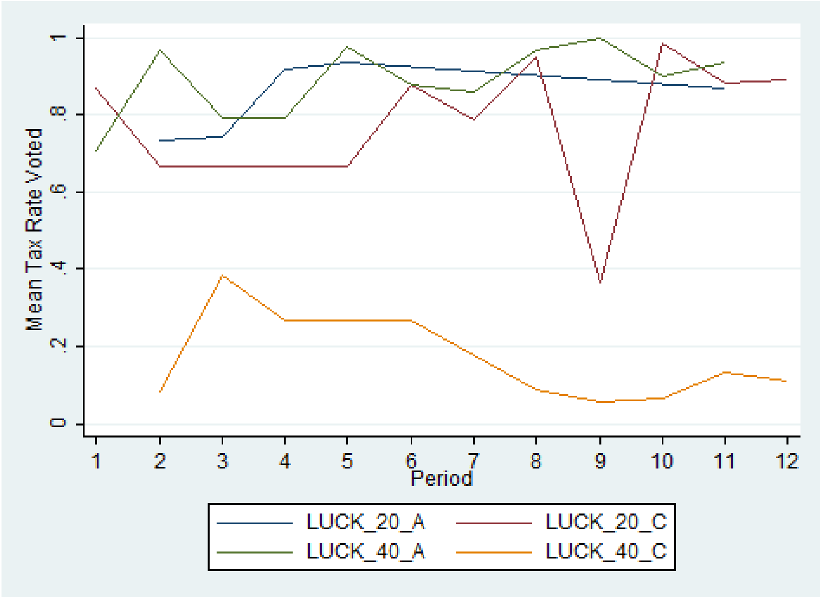


Figure 2.5: Mean tax rate voted by the subjects when drawn as type B, by period and state of the world.

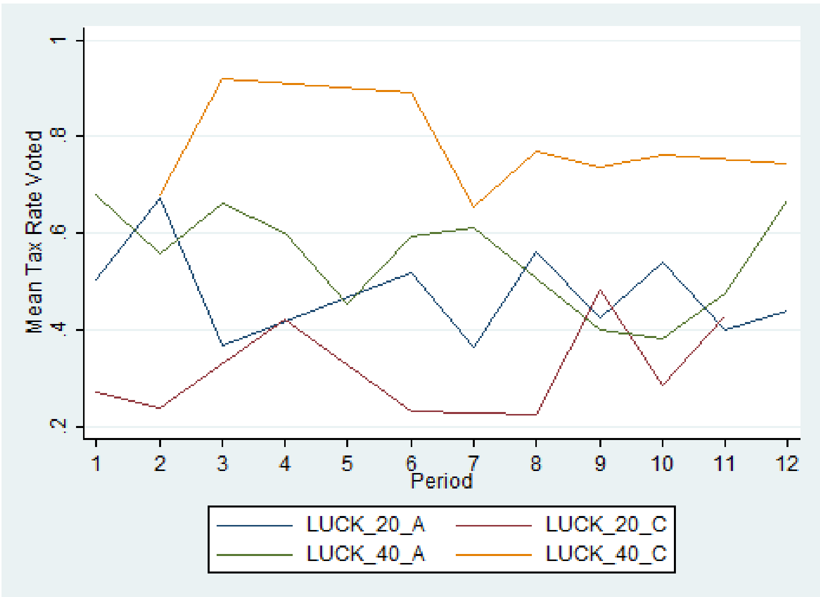


Figure 2.6: Mean tax rate voted by the subjects when drawn as type B, by period and state of the world.

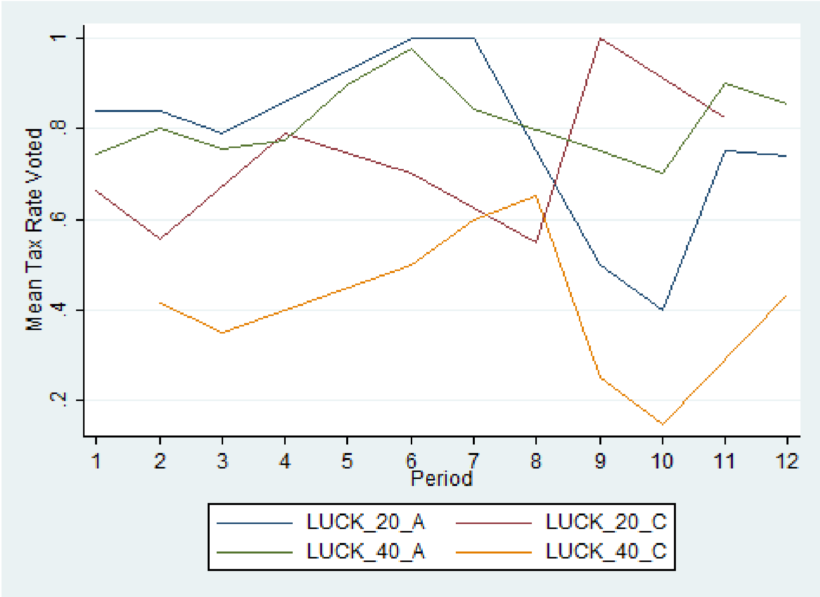


Figure 2.7: Mean tax rate voted by the subjects when drawn as type B, by period and state of the world.

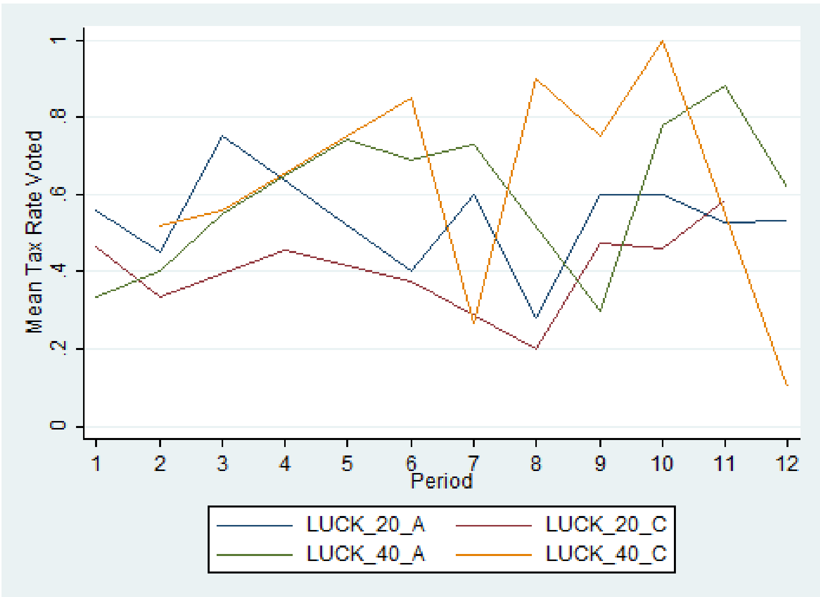


Figure 2.8: Mean tax rate voted by the subjects when drawn as type B, by period and state of the world.

AN EXPERIMENT OF ECONOMIC DECISIONS

Welcome and thank you for participating in this experiment

In this experiment 18 participants participate. The experiment will last about 1 hour and a half. For your participation you will receive 4 Euro and you will have the opportunity to earn more money based on the decisions you make during the experiment. Your earnings will be paid to you immediately at the end of the experiment. Your decisions and income will be kept confidential.

We will now start with an experimenter who will read the instructions aloud. At the conclusion of these instructions, you and the other participants can ask questions. Then the experiment will begin with the decisions made on the computer. At the end of the experiment a short questionnaire will be conducted.

During the experiment it is not allowed to speak or communicate in any way with the other participants. We also encourage you and other participants to turn off their mobile phone. If, during the experiment, you have a question at any time, raise your hand and one of the assistants will come to answer you.

Introduction

The experiment consists of twelve rounds. In each round, you and the other participants will be asked to answer questions and make choices.

At the end of the experiment, one of the twelve rounds will be drawn randomly and the gain you will have obtained in that round will be paid to you at the end of the experiment in cash, in private, by presenting the ticket you extracted when entering the laboratory. The gain in the experiment is expressed in tokens. The exchange rate with which the tokens are converted into Euro is 1 token = 0.1 Euro.

Instructions

At the beginning of each round, you and the other participants will be grouped into groups of three individuals. The formation of the group is completely random and the groups are reshuffled and reformed at the beginning of each round.

Each group of three individuals is always composed of three types of components: A, B and C.

At the beginning of each round, one component per group is randomly drawn as a type B component and the outcome of the draw is communicated to the entire group. Type B component is assigned a fixed income of 75 tokens.

After the random assignment of the type B component, the remaining two members of each group will be respectively assigned to type A and C depending on the outcome of an ability test. The ability test consists, in each round, of five closed-ended questions to answer which the two participants will have two minutes of time.

The type B player, during these two minutes, will have the opportunity to see the nature and type of questions presented to the other two members of his group in the skill test.

The ability test is characterized as follows:

- The questions are taken from psychometric tests, ranging from logic, to history, mathematics and general culture.
- The questions are presented one at a time, you can answer the question by selecting the answer you want and then clicking on the "OK" button. After answering a certain question, a new one will appear on the screen.
- Attention, once you have answered a question it is no longer possible to go back.

- If a participant finishes answering the questions before the two minutes, a waiting screen will appear on the monitor, otherwise, if he does not menage to finish the questions within two minutes, only those that have been answered will be considered.

- The remaining time will be provided in the upper right corner of the monitor. For each question there are four possible answers, plus an "I don't know" option.

- Only one of the four answers is correct, the other three are incorrect. Based on the answer chosen, you can get +1 point if you choose the correct answer, -1 point if one of the wrong answers is selected, 0 points if the "I don't know" option is chosen.

In each group, at the end of the ability test, the assignment of type A and C is determined as follows:

- The component of the two who performs the better in the ability test will be the type A component while the component of the two who scores the worst on the ability test will be the type C component.

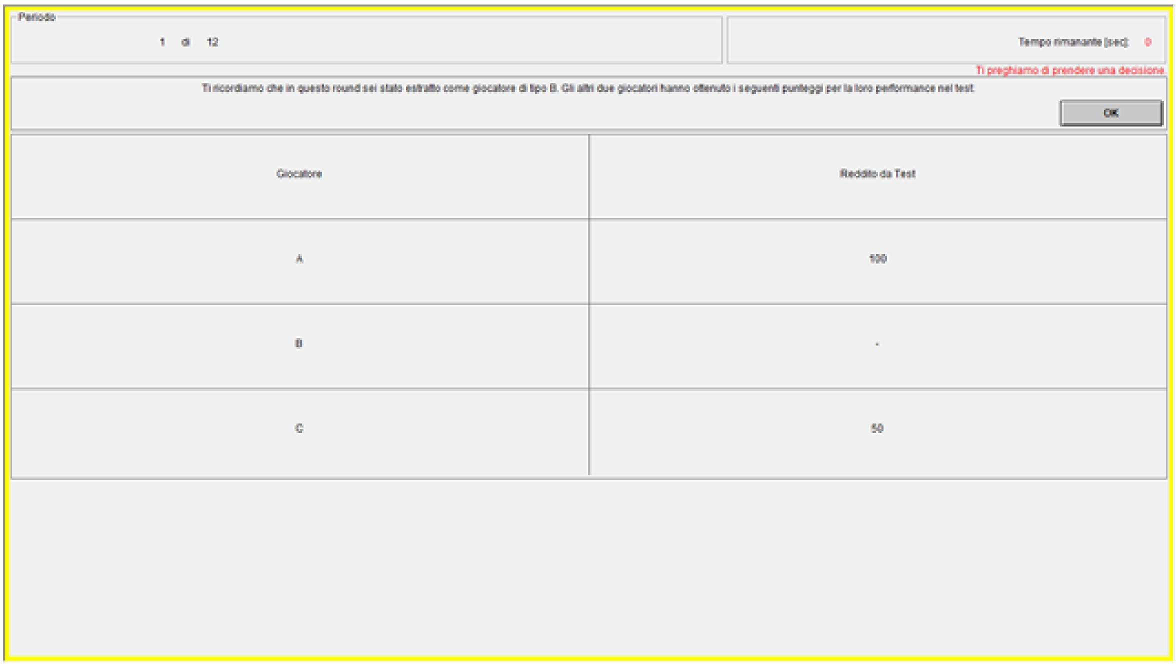
- In case of a tie, the component types A and C will be determined by considering, respectively, who took the shortest and longest time in answering the questions.

- Finally, in the event of equal scores and the time used to answer the questions, the type A and C component will be determined according to a random criterion.

Component Type A is then assigned an income of 100 tokens while Component Type C is assigned an income of 50 tokens.

At the end of the ability test, to each participant will be communicate their type in the round and their income, as shown in the following screenshot.

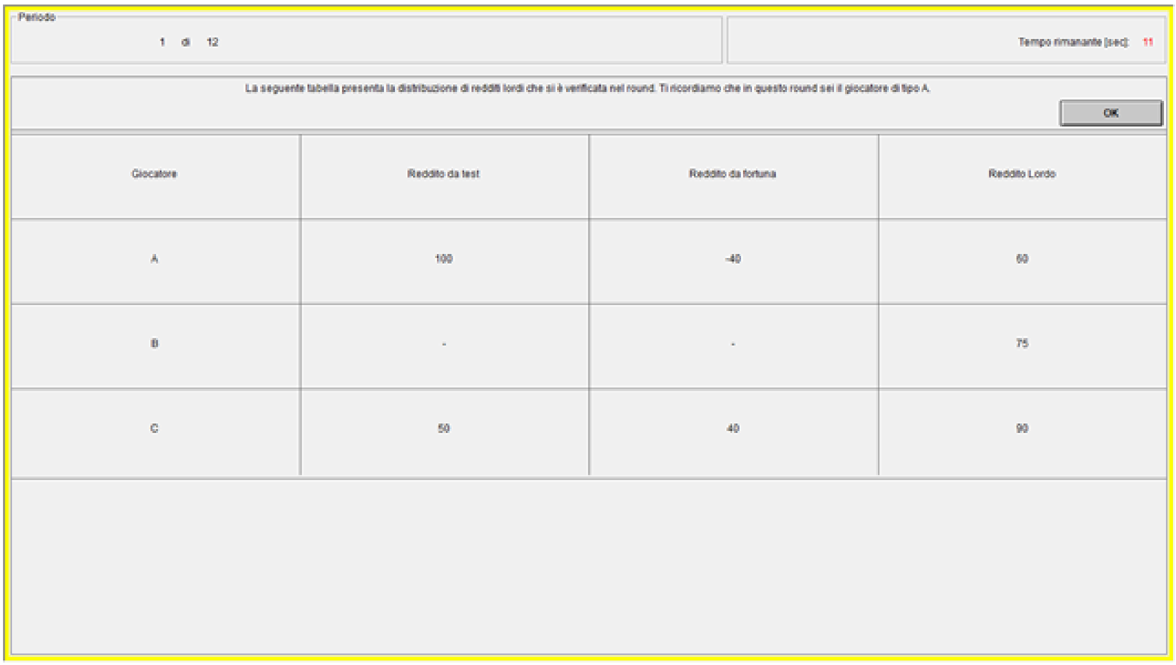
In each round, following the ability test and the assignment of the types of players, there will be a luck shock that will modify the income of the components of types A



and C. The shock can have, with equal probability, low or high intensity and it can be positive or negative.

- In case the shock is of low intensity, the income of one of the two players A or C will be selected randomly and will be increased by 20 tokens; the income of the other component will therefore be decreased by 20 tokens. Component B's income will remain unchanged.
- In case the shock is of high intensity, the income of one of the two players A or C will be selected randomly and will be increased by 40 tokens; the income of the other component will therefore be decreased by 40 tokens. Component B's income will remain unchanged.

After the luck shock has changed the income of type A and C members, a screen will summarize the distribution of income for each member of the group, which we now define as "gross income", respectively of types A, B and C , which occurred in the round.

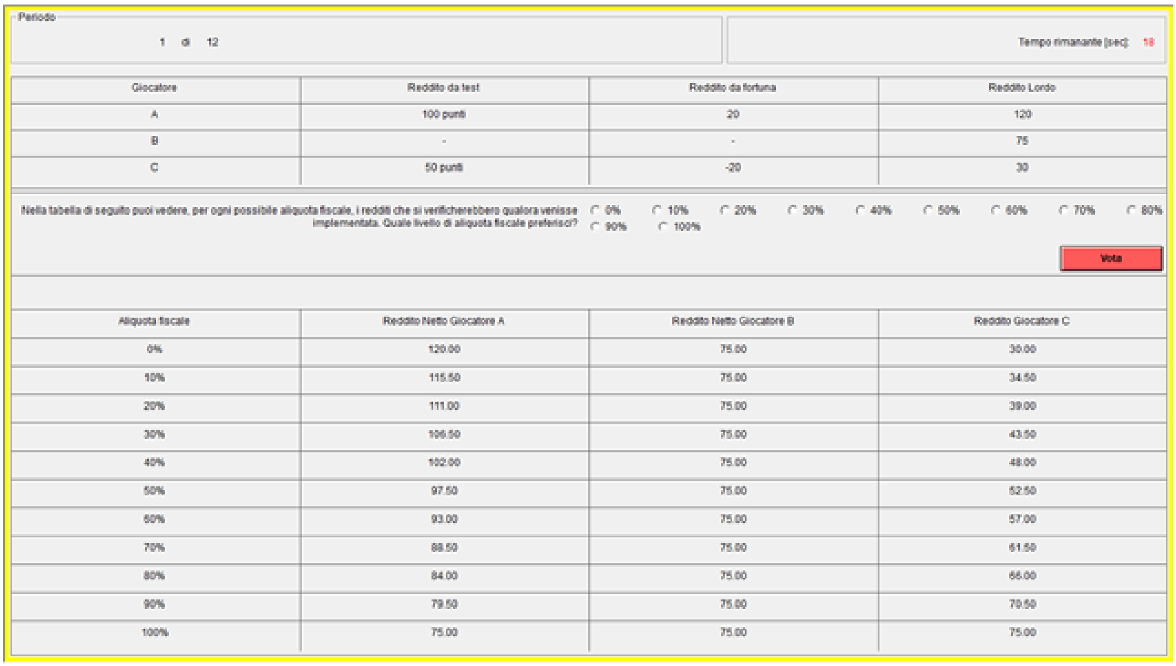


In the screenshot above we see an example of how, following the ability test, there was a high intensity random luck shock that decreased Player A's income by 40 coins while increasing Player C's income by 40 coins. Gross income is therefore 60 tokens for player A, 75 tokens for player B and 90 tokens for player C.

After each member of each group is informed of the distribution of gross income in their group, each member of each group will have to make a choice. Specifically, each participant will have to select the level of tax rate they would like to be applied to their group's gross income distribution. To facilitate the choice, for each tax rate , the distributions of net income that would occur for each member of the group if each possible rate were implemented are presented.

The proposed rates range from 0%, which preserves the distribution of gross income, to 100%, which results in a perfectly equal net income, equal to 75 tokens for each member of the group. All the intermediate rates, from 10% to 90%, progressively allow a reduction in the inequality of net incomes between the richest and the poorest component in the round.

Below there is an example of the screen you will see when you have to make the choice with respect to the rate you would like to be implemented in the gross income of your group.


Periodo

1 di 12

Tempo rimanente [sec]: 18

Giocatore	Reddito da test	Reddito da fortuna	Reddito Lordo
A	100 punti	20	120
B	-	-	75
C	50 punti	-20	30

Nella tabella di seguito puoi vedere, per ogni possibile aliquota fiscale, i redditi che si verificherebbero qualora venisse implementata. Quale livello di aliquota fiscale preferisci? 0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

Vota

Aliquota fiscale	Reddito Netto Giocatore A	Reddito Netto Giocatore B	Reddito Giocatore C
0%	120.00	75.00	30.00
10%	115.50	75.00	34.50
20%	111.00	75.00	39.00
30%	106.50	75.00	43.50
40%	102.00	75.00	48.00
50%	97.50	75.00	52.50
60%	93.00	75.00	57.00
70%	88.50	75.00	61.50
80%	84.00	75.00	66.00
90%	79.50	75.00	70.50
100%	75.00	75.00	75.00

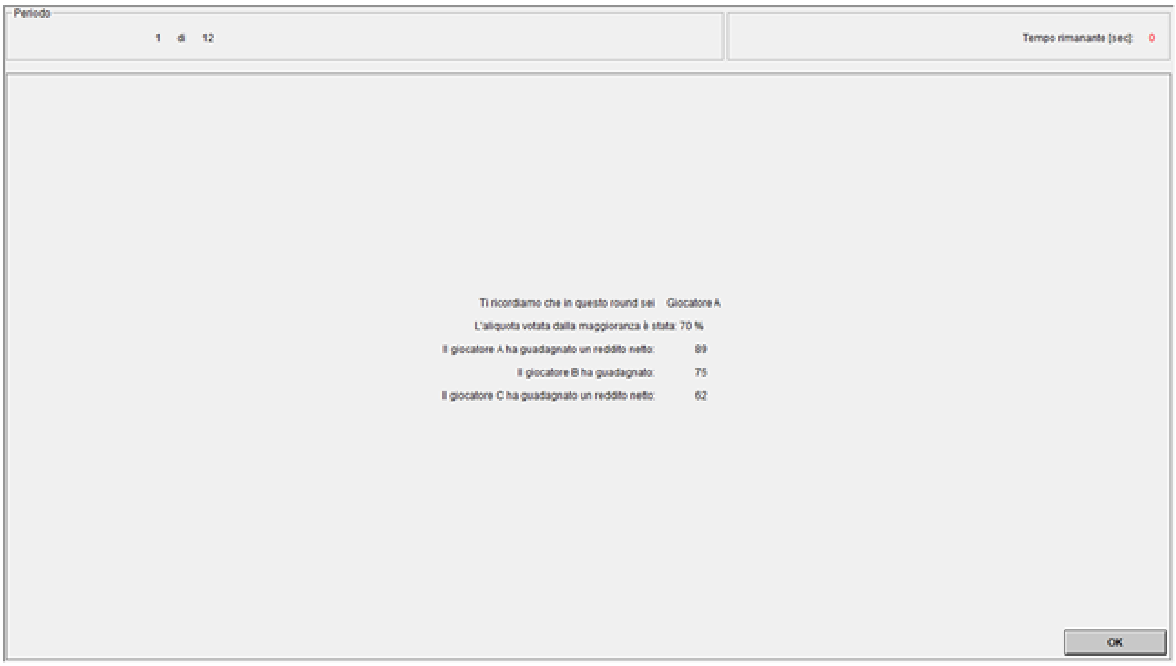
After each member of the group has confirmed his choice, the tax rate implemented to define the effective distribution of the group's net income for this round will therefore be the one preferred by the majority, that is the higher tax rate that at least two out of three individuals are willing to accept.

In particular:

- Suppose that all three members of the same group voted for the same tax rate, equal to 20%. The rate implemented will therefore be equal to 20%.
- Suppose that two members of the same group vote for the same tax rate, equal to 40%, and the third member of the group votes for a tax rate of 70%. The rate implemented will therefore be equal to 40%, that is the rate voted by two out of three individuals.

- Finally, suppose that the three members of the same group vote for three different rates equal to, respectively, 30%, 50% and 80%. The implemented rate will therefore be equal to 50%, or the highest rate that at least two out of three individuals would be willing to accept.

Each member of the group is therefore informed of both the tax rate implemented and the distribution of net income resulting from its implementation, as indicated in this example screen.



What happens now?

Now you will have to answer a few questions to check that everything is clear to you about the functioning of the experiment. As soon as all the participants have answered the questionnaire, the first round of the experiment will begin.

You can raise your hand at any time for any doubt. An assistant will come to you to answer your questions privately.

Chapter 3

Cooperation and Transfer in similar one-shot repeated games: experimental evidence on the role of temptation and efficiency

3.1 Introduction

The ability of agents to elaborate the feedback they receive after their actions can be seen as a form of *learning*. The learning process is crucial to increase the knowledge of the environment in which agents live and to improve their future choices for the survival, the development, and ultimately the well-being of individuals and communities as a whole. This ability has been extensively studied and analyzed in several fields like cognitive science, psychology, and economics.

The Stag Hunt (SH) game and the Prisoner's Dilemma (PD) game are two standard settings used in the literature on learning, transfer, and coordination problems.

Consider the 2×2 generic game G in Figure 3.1; C (cooperation) and D (defection) are the available actions, while *a, b, c, d* are the payoffs; for each pair the first letter is the payoff of the row player while the second letter is the payoff of the column player. For $c > a > d > b \geq 0$ the game G is a Prisoners' Dilemma, while for $a > c \geq d > b \geq 0$ it is a Stag Hunt.

The Stag Hunt game has two pure-strategy equilibria: the *efficient* but *risky* equilibrium $EFF = (C, C)$ and the *secure* but *inefficient* equilibrium $SEC = (D, D)$[1].

[1]The experimental evidence on the equilibrium selection in the SH game is mixed: the payoff

	C	D
C	a,a	b,c
D	c,b	d,d

Table 3.1: The generic 2×2 game G.

This trade-off between security and efficiency is the reason why the SH game has been extensively studied in the literature of the equilibrium selection problem.[2]

A stylized fact in the experimental literature for the repeated Stag Hunt game, both with random matching and partner matching, is that the cooperation rate approaches 0 or 1 over time.

The Prisoners' Dilemma game instead presents the individual with a tension between self and common interest. The payoff from mutual cooperation a is higher than the payoff from mutual defection d but the fact that $c > a$ makes defection a tempted choice because it increases the payoff of an agent if the opponent cooperates, but also makes cooperation a risky choice because the agent could be exploited by a defector opponent. This dilemma resembles situations like tax compliance, effort provision in teams, and public good provision and has been studied to understand and make predictions in these settings[3].

The one-shot PD game has one Nash equilibrium in which both players choose to defect. However, for some payoff parametrizations, the indefinitely repeated PD, even with random matching, can theoretically sustain the pure strategy efficient equilibrium where both players cooperate (Kandori, 1992)[4]. A stylized fact of the experimental literature of the Prisoner's Dilemma is that the cooperation rate in repeated setting

structure (Battalio et al., 2001; Clark et al., 2001; Dubois et al., 2012; Schmidt et al., 2003; Stahl and Van Huyck, 2002), the matching protocol (Cooper et al., 1990, 1992; Clark et al., 2001) and pre-play communication (Duffy and Feltovich, 2002, 2006), seem all to play a significant role.

[2]The strongest evidence of subjects coordinating on the efficient equilibrium comes from Rankin et al. (2000). In their experiment, a population of 8 human subjects anonymously and randomly matched face a sequence of 75 similar but different SH games; when a convention emerges, it is consistent with efficiency rather than security or risk dominance.

[3]In the literature there is a substantial variation in the level of cooperation rates from one experiment to another (see Andreoni and Miller, 1993; Andreoni and Varian, 1999; Bereby-Meyer and Roth, 2006; Grimm and Mengel, 2009; Friedman and Oprea, 2012).

[4]Both in our setting and in the experiment of Duffy and Fehr (2018), in the PD stage games that we consider, besides mutual defection, mutual cooperation also is an equilibrium.

declines over time.

Recently, Mengel (2018) identifies three factors related to the payoff structure of the PD game that may affect cooperation: temptation (T), risk (R), and efficiency (E). LiCalzi and Mühlenbernd (2021) extend the temptation factor in order to apply this approach also to the SH game.[5]

In a setting characterized by random matching of subjects in each round of indefinitely repeated PD and SH games, the novel question that we investigate in this paper is to experimentally explore the relative role that temptation and efficiency have in determining the cooperation level that players reach in PD and SH, and in promoting transfer of experience of cooperation between these two games.

We believe these aspects being of real-world relevance as it is likely that players face different strategic settings that resemble the incentive structure of the PD and the SH, they probably evolve or alternate from one to another. It is, therefore, crucial to understand what are the main relevant factors that promote cooperation and transfers between these strategic settings. Our work reinterprets and partially replicates the experimental setting and results of Duffy and Fehr (2018) (henceforth, D&F) through the theoretical approach of Mengel (2012) and LiCalzi and Mühlenbernd (2021). According to this theoretical approach, in their experiment, D&F consider variations only in the temptation parameter. We replicate the structure and the design of their experiment so that we consider the same levels of temptation in our experiment but, we respect to them, we also modify the efficiency parameter.

We decided to use the experimental setting of D&F for several reasons. First, D&F use the SH and PD games, which are the analysis tools at the core of the theoretical approach we adopt. Second, we can use their results as a baseline, and, third, the indefinitely repeated setting lets us study the cooperative behavior and transfer not

[5] Temptation is thus equal to the percentage gain when an agent best replies against C: $T = \frac{|c-a|}{max(a,c)}$. The absolute value and the *max* function extend the temptation factor both to the PD game, when $c > a$ and to the SH game when $a > c$. Risk and efficiency have the same interpretation in the SH and in the PD game. Risk corresponds to the percentage loss an agent suffers when cooperates against D: $R = \frac{d-b}{d}$. Efficiency is the percentage gain that an agent receives from coordinating in C versus D: $E = \frac{a-d}{a}$.

only in first rounds but also their persistence in the subsequent rounds[6].

In each experimental session, we let subjects play three stage games. Each stage game is composed by an indefinite number of repeated rounds in which subjects are randomly matched at each round. In each stage game, the subjects play the same version of a SH or PD game[7]. The duration of each of the three stage games is indefinite, but the experiment is designed in such a way that it is approximately 30 rounds so that each session lasts approximately 90 rounds[8]. Our research objectives are threefold. First, we want to investigate the behavior of subjects in each stage game; how does the level of cooperation achieved in a stage game vary if the values of temptation and efficiency vary?

As a second research objective, we want to assess whether there are significant differences in subjects' behavior between the first and the third stage games in which subjects play the same game. Do the same groups of subjects who reached, after about 30 rounds, a certain level of cooperation in stage 1 change their attitude to cooperate in stage 3 when they play the same game after having experienced, in stage 2, the other type of game? To answer this question, we consider the possibility of transfer effects, i.e., the ability to take the information learned in one game and apply it to another related game (see Cooper and Van Huyck, 2018) from SH and PD and the other way around. Also in this case, we are interested to see whether temptation or efficiency play a significant role in the transfer process between these two games. We consider the possibility of two types of transfer effects: fleeting transfer effects and permanent transfer effects. In the first case, we want to see whether subjects significantly change their behavior in the first round of stage 3, compared to the cooperation they had reached in stage 1. We, therefore, test whether there is a difference between the average cooperation exhibited in the overall stage 1 compared to that shown in the first round of stage 3. If there is a significant difference in how players cooperate in

[6]As already pointed out by d'Adda et al. (2017), great attention is given to the direct impact of policy measures. However, less attention has been given to investigate their ability to persist over time and spill across contexts.

[7]We call a *version* of SH or PD a parametrization of Table 2 and Table 3 for a given $\mathbf{x}$.

[8]See the Experimental Design section for more details on the construction of the indefinite duration of the stage games.

the first round of stage game 3 with respect to the cooperation reached in stage 1, we call this effect *fleeting transfer*.

If there is a fleeting transfer we study whether this transfer and the new level of cooperation are kept in the following rounds of stage 3 or whether subjects return to the previous level of cooperation reached in stage 1. If subjects keep the new level of cooperation in the overall third stage game, we call this effect *permanent transfer*.

We also change the order in which subjects play the stage games; some groups start with the PD, then switch to the SH, and then return to the initial PD. Other groups start with an SH, switch to the PD, and then switch back to the first SH. As a third research objective, we aim at investigating possible order effects between our games, i.e. transfer effects without previous experience with the same game. In order to test for possible order effects (i.e. transfer without experience with both games), we compare the average cooperation rate reached by groups that start playing a game (PD or SH) in stage 1 (therefore without an experience in the other type of game), with the average cooperation rate reached by other subjects playing the same game (PD or SH) in stage 2 that come from a previous experience in the other game in stage game 1. In this case, we are interested in understanding if the level of cooperation reached in stage 2, which is preceded by a stage game of the other game, is significantly higher or lower than the cooperation reached in the same game in stage 1 by other subjects without the previous experience in the other game. We find that increasing efficiency induces a significant increase in the cooperation level in the PD and SH games in our experiment with respect to the level of cooperation of D&F. Moreover, in both experiments, when temptation is high, subjects cooperate more in SH, but there are no significant effects in the PD.

In the treatments where temptation is low, both in our experiment and in D&F, we find a fleeting positive transfer from a high cooperation rate reached in the second stage SH game to the first rounds of the subsequent third stage PD game.

In both cases, we do not observe a persistent transfer as cooperation decreases in subsequent rounds. However, in our experiment, where efficiency is higher than in D&F, this positive transfer delays the return to defection in the PD; the cooperation rate reached is still higher than in D&F also when we consider the entire average cooperation rate over all the rounds of the third stage PD and not only the first rounds.

3.2 Related Literature

In a recent meta-study of 96 laboratory experiments on Prisoners' Dilemma, Mengel (2018) identifies three key descriptors for predicting cooperation: *temptation*, the percentage gain that an agent receives from defecting against a cooperator, *risk*, the percentage loss an agent suffers when she cooperates against a defector and *efficiency*, the percentage gain that an agent receives from coordinating on mutual cooperation. LiCalzi and Mühlenbernd (2021) extend these features to the Stag Hunt game. Our paper contributes both to the literature of learning in games and to the literature of transfers between games by disentangling and providing evidence on the role of temptation and efficiency. Mengel (2018) finds that, in the PD, risk and efficiency are significant for the cooperation rates in the random matching and one-shot setting, while temptation can explain variation in average cooperation rates in the repeated setting with partner matching.

Following the same theoretical approach, Gächter et al. (2020) experimentally examine the role of risk, temptation, and efficiency on cooperation in one-shot PD games. Subjects play eight different versions of one-shot PD games in which the payoff parameters across games are changed. They find that temptation has the most significant impact on cooperation in PD, while we find that efficiency plays a more relevant role. One possible reason for this difference is that the temptation levels we used are closer to each other, while they consider more different levels of temptation. The study of Engel and Zhurakhovska (2016) experimentally examines the impact of changing only the payoff value of mutual defection in PD games. They find that cooperation increases as the payoff of mutual defection decreases but, conversely than in our setting, they are not able to distinguish between efficiency and risk.

Both these studies do not provide feedback about the opponent's choice to the subjects, while in our experiment, providing feedback is a key aspect for the learning process of the agents.

Another experimental study that instead provides feedback during the experiment is Charness et al. (2016) that examines how cooperation rates vary across four one-shot PD and find that cooperation increases when the payoff of mutual cooperation is increased. This result is difficult to interpret because changing the payoff of mutual cooperation simultaneously affects temptation and efficiency. We believe that disentangling the two effects is important in order to understand the relative role of each of these factors. Our paper also contributes to the literature on the role of precedence in coordination games. The first to study experimentally the role of precedents in coordination games are Van Huyck et al. (1991). In particular, they repeatedly ask players to choose a number between 1 and 7 (inclusive). The closer the chosen number is to the median of the others' choices, the higher the payoff. Van Huyck et al. show that the median reached in the first period serves as a precedent to coordinate in subsequent periods.

Devetag (2005) finds that a precedent of efficient play in the critical mass game (see Devetag, 2003) transfers to the Pareto-efficient equilibrium of the subsequent minimum effort game (see Van Huyck et al., 1990).

Cooper and Van Huyck (2018) replicate the experiment of Rankin et al. (2000) and find that when the subjects subsequently play a random sequence of order statistic games, they significantly choose the payoff dominant equilibrium with respect to subjects without this previous experience.

Rusch and Luetge (2016) aims to experimentally explore the interdependence hypothesis (see Tomasello et al., 2012). According to this theory, the ability of humans to cooperate understood as the ability to obtain and sustain a social benefit in the presence of strong individual incentives that are not aligned with social ones (that can be connected to the PD game), is, at least in part, the result of an evolution from situations in which the incentive to deviate is weak or aligned with the social one (that can be connected to the SH game). They find that when subjects play in fixed pairs, cooperation levels in PD games embedded in a sequence of SH games are significantly increased with respect to a sequence consisting only of PD. However, this effect is no longer present when players are randomly matched each round. The results of our experiment confirm the difficulty of constructing transfer effects between SH and PD, but also suggest that transfer success may be closely related to the strength of the incentives. With high temptation, we observe no transfer effect, but with low temptation and high efficiency, the positive transfer we find provides some support, albeit limited, for this theory. Similarly, Knez and Camerer (2000) find that the cooperation level of a one-shot PD played after a series of coordination games was higher than a baseline without this precedent, but they only consider the partner matching.

Our study confirms the results of Ahn et al. (2001) that find a significant increase in the cooperation in the PD game preceded by a series of SH, even in the random matching treatment; in this paper, the transfer is evaluated on a PD played only once, so that it is not possible to analyze the persistence of the effect. We add on it by providing a test on whether such an effect is persistent. Peysakhovich and Rand (2016) instead, let subjects play sequences of finite or indefinite repeated PD structured in a way that promotes cooperation or defection[9] in a random matching protocol. After the sequence of PD, subjects play a battery of several one-shot games like the dictator game, a public good game, an ultimatum game, and a trust game. They find that subjects who experienced greater cooperation in the precedent PD are significantly more likely to act pro-socially in the subsequent games.

3.3 Theoretical Framework

In this section, we overview the main theoretical learning approaches developed in Game Theory, and we introduce the main features of the theoretical model underlying the interpretation of our results. In game theory, there exist several approaches to model learning in games.

[9] To manipulate cooperation promotion, they randomly assigned subjects to treatments with PD with different temptation values.

Traditional game theory does not involve learning; it assumes full rationality of the agents that interact, exhibiting equilibrium behavior.

Another approach is the one of *evolutionary game theory.* These models originated in the field of evolutionary biology, where a crucial concept is the replicator dynamic by which the population playing a given strategy grows in proportion to how well that strategy is doing relative to the mean population payoff (Fudenberg and Levine, 1998). An important conclusion is that, if the dynamics converge, the limiting action frequencies converge to a steady-state where they are optimal in the stage game; thus, the limiting frequencies form a Nash equilibrium (Crawford, 1997).

Somehow in between are the *models of adaptive learning* that aim both to provide foundations for theories of equilibrium and explanations or predictions for experimental results. Most of these models involve the concept of "attraction": strategies are evaluated according to several criteria, and attraction values are computed in response to experience (see Wilkinson and Klaes, 2017). Over the years, many different models of adaptive learning have been proposed. The two main approaches are reinforcement learning and belief learning.

The approach of *reinforcement learning* (Erev and Roth, 1998) derives from psychology. In this approach, agents only consider previous choices and resulting payoffs, and they tend to use strategies that provided higher payoffs in the past. *Beliefs learning*, instead, assumes that agents form beliefs about their opponent's play and accordingly maximize the expected payoff.

In fictitious play, a well-known model of belief learning originally introduced by Brown (1951), agents observe and consider the entire history of how the opponents have played over time, update their beliefs and play accordingly (see Boylan and El-Gamal, 1993). Later, Camerer and Hua Ho (1999) introduce the Experience-weighted attraction (EWA) model. This learning model is more sophisticated and attempts to combine fictitious play and reinforcement learning (it includes them as special cases).

3.3.1 The model of LiCalzi and Mühlenbernd (2021)

Most of the learning models fixate on the special case where agents face a stream of *identical* games.[10] More recently, LiCalzi and Mühlenbernd (2021) introduce a novel approach, with the goal of contributing to the study of learning over *similar* games.
LiCalzi and Mühlenbernd (2021) extend the approach of Mengel (2018) and apply the concepts of temptation, risk and efficiency to the SH game by extending the definition of temptation.
The model combines the payoff external features of the game that agents face and that are thus independent of agent's experience together with personal, intrinsic motivations that are subjective to each agent and that evolve according to the experience and feedback she faces.

	C	D
C	a,a	b,c
D	c,b	d,d

Table 3.2: The generic 2×2 game G; for $c > a > d > b \geq 0$ the game G is a Prisoners' Dilemma, while for $a > c \geq d > b \geq 0$ it is a Stag Hunt.

The *extrinsic* features are a vector of three values (*Temptation*, *Risk* and *Efficiency*) as defined in the introduction[11].
We can interpret each game G of Table 3.2, whether it is an SH or a PD, as a vector of the extrinsic parameters $G = (T, R, E)$.
In the model of LiCalzi and Mühlenbernd (2021) an agent, when facing a SH or a PD game, categorizes the game combining temptation, risk, and efficiency with the intrinsic individual-specific features and decides whether to cooperate or not according

[10] There were only a few theoretical results for learning in similar games, like LiCalzi (1995) and Mengel and Sciubba (2014) that extends the fictitious play dynamic to similar games.

[11] Please remember that temptation is equal to the percentage gain when an agent best replies against C: $T = \frac{|c-a|}{max(a,c)}$. Risk corresponds to the percentage loss an agent suffers when cooperates against D: $R = \frac{d-b}{d}$. Efficiency is the percentage gain that an agent receives from coordinating in C versus D: $E = \frac{a-d}{a}$.

to this categorization. Thus, the attraction towards cooperation (C) or defection (D) for an agent depends on the interaction among each extrinsic and intrinsic feature.

The three *intrinsic* individual motivations are fear (f), the willingness to avoid the lowest payoff b; greed (g), the willingness to attain the highest payoff (c in PD, a in SH); harmony, (h), the propensity to coordinate on the highest common payoff a. The model lets the extrinsic and the intrinsic features interact in three ordered pairs, generating three agent's dispositions in $[0, 1]$; the disposition to avoid the lowest payoff $d_R = f \cdot R$, the disposition to achieve the highest payoff $d_T = g \cdot T$ and the disposition to coordinate on the highest common payoff $d_E = h \cdot E$.

Thus, the probability $P_i(C)$ that an agent i with extrinsic features *(T, R, E)* and with intrinsic features *(f, g, h)* chooses to cooperate is:

$$P_i(C) = \begin{cases} \frac{d_E}{d_E+d_T+d_R} & \text{if G is a PD game} \\ \\ \frac{d_E+d_T}{d_E+d_T+d_R} & \text{if G is a SH game} \end{cases}$$

The probability of the opposite choice is $P_i(D) = 1 - P_i(C)$. Consequently, for both SH and PD games, the probability of playing C increases with efficiency and harmony, while the probability of playing D increases with risk and fear. Temptation and greed are context-dependent as they increase the probability of playing C if the game is a SH and of playing D if the game is a PD.

The intrinsic features are personal for each subject. They evolve dynamically according to the experience that the agents face of the choices of their opponents and to the learning rate of the agents, i.e., the sensitivity of the reaction with respect to this experience. In particular:

$$(f', g', h') = \begin{cases} (f + \gamma_i, g, h)_i & \text{if } s_j^t = D \\ (f, g + \gamma_i, h)_i & \text{if } s_j^t = C \\ (f, g, h + \gamma_i)_i & \text{if } s_j^t = C \text{ and (C,C) gives the highest possible payoff} \end{cases}$$

and then divided by $(1+\gamma_i)$ to renormalize their sum to 1. s_j^t represent the action (C or D) chosen by the opponent j in the last period t. The magnitude of γ_i represents the learning rate of agent i and the model sets $\gamma_i = 1 - max(P(C), P(D))$. According to this update rule, after a recent history of experienced defection from the opponent D_j^t, fear will increase, increasing the probability of choosing defection. Instead, after a recent history of experienced cooperation of the opponent C_j^t, harmony will increase if (C, C) gives the highest possible payoff as it is in the SH game, increasing thus the probability of cooperation; otherwise, greed will increase, as in the PD game, increasing the probability of defection.

The model of LiCalzi and Mühlenbernd (2021) in our experiment

Before describing the details of the experimental design, we will interpret the basic games matrices used in the experiment according to the theoretical model of LiCalzi and Mühlenbernd (2021).

As mentioned in the Introduction, we will vary the parameter **x** in Table 3.3, and for each value of this parameter, we will obtain a version of a PD or SH game. Varying the parameter **x**, we will only vary the temptation level of each version, leaving all the other extrinsic features constant. In each experimental session we only consider two values of **x** for a total of four versions, either $x \in \{10, 30\}$ that defines the treatments with *high temptation*, or $x \in \{15, 25\}$, that describes the treatments with low temptation. If $x \in \{25, 30\}$ the stage game in Table 3.3 becomes a PD (Table 7 and Table 9), while as $x \in \{10, 15\}$, the stage game is a SH (Table 6 and Table 8).

	C	D
C	20,20	0,**x**
D	**x**,0	1,1

Table 3.3: The $G[\mathbf{x}]$ stage game used in our experiment.

$\mathbf{SH_A}$	C	D
C	20,20	0,**10**
D	**10**,0	1,1

Table 3.4: SH version with high temptation and $x = 10$.

$\mathbf{PD_A}$	C	D
C	20,20	0,**30**
D	**30**,0	1,1

Table 3.5: PD version with high temptation and $x = 30$.

$\mathbf{SH_B}$	C	D
C	20,20	0,**15**
D	**15**,0	1,1

Table 3.6: SH version with low temptation and $x = 15$.

$\mathbf{PD_B}$	C	D
C	20,20	0,**25**
D	**25**,0	1,1

Table 3.7: PD version with low temptation and $x = 25$.

According to the model of LiCalzi and Mühlenbernd (2021), we can interpret each of these four versions of stage games as vectors of their extrinsic parameters temptation, risk, and efficiency (T, R, E):

$$SH_{\text{A (High Temptation)}} = \left(\tfrac{20-10}{20}; \tfrac{1-0}{1}; \tfrac{20-1}{20}\right) = \left(\tfrac{1}{2}; 1; \tfrac{19}{20}\right)$$

$$PD_{\text{A (High Temptation)}} = \left(\tfrac{30-20}{30}; \tfrac{1-0}{1}; \tfrac{20-1}{20}\right) = \left(\tfrac{1}{3}; 1; \tfrac{19}{20}\right)$$

$$SH_{\text{B (Low Temptation)}} = \left(\tfrac{20-15}{20}; \tfrac{1-0}{1}; \tfrac{20-1}{20}\right) = \left(\tfrac{1}{4}; 1; \tfrac{19}{20}\right)$$

$$PD_{\text{B (Low Temptation)}} = \left(\tfrac{25-20}{25}; \tfrac{1-0}{1}; \tfrac{20-1}{20}\right) = \left(\tfrac{1}{5}; 1; \tfrac{19}{20}\right)$$

Please note that, while risk and efficiency are kept constant and are equal to 1 and $\frac{19}{20}$ respectively, the value of temptation varies across the four versions; $\frac{1}{2}$ and $\frac{1}{4}$ in the SH versions and $\frac{1}{3}$ and $\frac{1}{5}$ in the PD versions.

Figure 3.8 shows the 2×2 stage game of the experiment of Duffy and Fehr (2018). If $x \in \{25, 30\}$ the stage game in Table 3.8 becomes a PD (Table 12 and Table 14), while as $x \in \{10, 15\}$, the stage game is a SH (Table 11 and Table 13). The only difference in the payoff structure between the two experiment is the payoff of mutual defection (D, D).

	C	D
C	20,20	0,**x**
D	**x**,0	10,10

Table 3.8: The $G[\mathbf{x}]$ stage game used in the experiment of D&F.

$\mathbf{SH_A}$	C	D
C	20,20	0,**10**
D	**10**,0	10,10

Table 3.9: SH version with high temptation and $x = 10$.

$\mathbf{PD_A}$	C	D
C	20,20	0,**30**
D	**30**,0	10,10

Table 3.10: PD version with high temptation and $x = 30$.

$\mathbf{SH_B}$	C	D
C	20,20	0,**15**
D	**15**,0	10,10

Table 3.11: SH version with low temptation and $x = 15$.

$\mathbf{PD_B}$	C	D
C	20,20	0,**25**
D	**25**,0	10,10

Table 3.12: PD version with low temptation and $x = 25$.

We can interpret also these four version of stage games of the experiment of Duffy and Fehr (2018) as vectors of their extrinsic parameters $G = (T, R, E)$:

$$SH_{\text{A (High Temptation)}} = \left(\tfrac{20-10}{20}; \tfrac{1-0}{1}; \tfrac{20-10}{20}\right) = \left(\tfrac{1}{2}; 1; \tfrac{1}{2}\right)$$

$$PD_{\text{A (High Temptation)}} = \left(\tfrac{30-20}{30}; \tfrac{1-0}{1}; \tfrac{20-10}{20}\right) = \left(\tfrac{1}{3}; 1; \tfrac{1}{2}\right)$$

$$SH_{\text{B (Low Temptation)}} = \left(\tfrac{20-15}{20}; \tfrac{1-0}{1}; \tfrac{20-10}{20}\right) = \left(\tfrac{1}{2}; 1; \tfrac{1}{2}\right)$$

$$PD_{\text{B (Low Temptation)}} = \left(\frac{25-20}{25}; \frac{1-0}{1}; \frac{20-10}{20}\right) = \left(\frac{1}{5}; 1; \frac{1}{2}\right)$$

The risk parameter is constant and equal to 1, as it is in our experiment. The temptation parameter varies in the same way and assumes the same values in both our experiment and in D&F assuming value $\frac{1}{2}$ and $\frac{1}{4}$ in the SH versions and $\frac{1}{3}$ and $\frac{1}{5}$ in the PD versions. Efficiency in D&F is constant in all the four versions and equal to $\frac{1}{2}$. Instead, in our setting, efficiency is constant and equal to $\frac{19}{20}$. This difference is due to the different payoff of mutual defection equal to $(1, 1)$ in our experiment and equal to $(10, 10)$ in D&F. Consequently, we will analyze the role of temptation within each experiment while we can interpret each difference between the two experiments as the impact of the increased efficiency parameter.

3.4 Experimental design

Our experiment exploits a 2×2 design in which we vary both the level of temptation across the stage games, as described in the above paragraph[12], and the order with which the players play the two types of games, either $PD \to SH \to PD$ or $SH \to PD \to SH$ as illustrated in Table 3.13.

		ORDER EFFECTS	
		$PD \to SH \to PD$	$SH \to PD \to SH$
TEMPTATION	HIGH, $x \in \{10, 30\}$	PDSH3010	SHPD1030
	LOW, $x \in \{15, 25\}$	PDSH2515	SHPD1525

Table 3.13: The four treatments of our experiment: PDSH3010, SHPD1030, PDSH2515, SHPD1525. The treatment variables are the temptation level across the games and the order with which subjects face the games.

[12] Duffy and Fehr (2018) interpret the variation of the value of x as a measure of the *similarity* across games. At the same time, we look at this parameter as a key variable to disentangle the role of temptation in the experiment. There is no general theory of how players perceive similarity in games, and we do not consider it in our interpretation of the results.

The combination of the temptation treatment and the order effects generates four treatments: PDSH3010, SHPD1030, PDSH2515, and SHPD1525. When we refer to treatments with high temptation, we consider together the treatments PDSH3010 and SHPD1030, while when we refer to treatments with low temptation, we consider together the treatments PDSH2515 and SHPD1525[13].

In order to control for possible order effects, our second treatment variable is the order in which the games are played. In each session, we let 10 subjects interact and play indefinitely rounds of SH or PD, where they are randomly and anonymously paired at each round. Each session is composed of three stages. In each stage, only a SH or PD game is played.

Each stage consists of an indefinite number of sequences, and each sequence is composed of an indefinite number of rounds.

In each sequence, subjects play only one type of stage game in each round until the sequence ends. Players start at the first round and face the first stage game, either a PD or a SH.

At the end of the round, players receive feedback on their opponent's choice and their payoffs. Then, a six-faces virtual dice is rolled. If the resulting face is equal to 1, 2, 3, 4, or 5, the sequence continues, the subjects are randomly matched again, and another round with the same stage game begins. If the resulting face is equal to 6, then the sequence ends. In this case, players know that the stage game *may* change.[14]

Only when a sequence ends, the experimenter terminates or moves to the next stage game.

If the experimenter continues with the same stage game, then the subjects are informed that another sequence of the same stage starts and another round begins. If instead, the experimenter changes the stage game, then the subjects are informed that the stage game is changed and the other stage game starts.

[13]Please note that in the name of each treatment, the first component is the type of game with which the players start the experiment, while the numbers represent the values of $\mathbf{x}$ used in the treatment and, thus, the level of temptation.

[14]This random termination procedure is equivalent to an infinite horizon where the discount factor δ attached to future payoffs is $\frac{5}{6}$ per round.

The length of the experiment is thus indeterminate. Still, we wanted each experimental session to last about 90 rounds with around $\frac{1}{3}$ of the rounds involving the play of the first stage game; the next $\frac{1}{3}$ involving the play of the second stage game; and the final $\frac{1}{3}$ involving the play of the first stage game once again. In order to approximate the 90 rounds, the experimenter starts a new stage game either when subjects approach 30 rounds of the same stage game or when more than 30 minutes pass[15]. At the beginning of each sequence, independently from the fact that the same stage game is played or it changes, subjects are asked to choose a number from 0 to 9 that represents the number of players they believe will choose to cooperate in that round. The matrix game that subjects face during each round is black if it is a PD and red if it is a SH in order to make the change of the game structure even more salient and prevent habituation effects. Overall the experimenter switches the value of **x** *twice*, but the subjects are not aware of it.[16]

Each session utilizes two stage games, one version of SH and one version of PD. The stage game with which the experiment begins is repeated twice, at the first stage and at the third stage of the session. Players are not aware of the expected length of 90 rounds or of the number of times that the stage game changes. They know that they will play a series of sequences of games and that when a sequence ends, another sequence starts, and the stage game *may* change.

Subjects are unaware of our objective of two stage game changes or of the duration of each of the three sequences of stage games.

At the beginning of the experiment, they are instructed about the random matching procedure, and they are shown the two possible stage games they will face during the experiment according to the treatment they are assigned. They are informed that a change could occur at the start of each new sequence (after the virtual die roll of a 6)[17]. When the number of overall rounds approaches 90, and a sequence ends, the experi-

[15] This rule is necessary to ensure a reasonable duration of the experiment but its use was marginal.

[16] Thus, subjects that start playing a stage game, for example, the PD version with $x = 30$, then will play the SH stage game $x = 10$, and finally they will conclude by playing the starting stage game again, in this case, the PD with $x = 30$.

[17] Before starting with the sessions, subjects answered to several closed questions to make sure they understood the correct functioning of the experiment.

menter finishes the experiment, and the subjects answer an anonymous questionnaire.

3.4.1 Procedures

The experiment was carried out in September 2020 in *online* mode because of the *COVID-19* pandemic. The experiment was programmed with the software *z-Tree* (Fischbacher, 2007) and run online through *z-Tree Unleashed* (Duch et al., 2020a).
We run 20 sessions; each session involved 10 participants that interacted together at the same time. The average payment was $14,2$ Euro, including a show-up fee of 3 Euro; each session lasted about an hour and a half.
Subjects were primarily students at Ca' Foscari University of Venice, and they were recruited through the *ORSEE* (Greiner, 2015b) platform[18].

3.5 Hypotheses

Our experimental design lets us test several predictions of the theoretical model of LiCalzi and Mühlenbernd (2021) for this setting.
The first hypothesis aims to assess the impact of the efficiency parameter on the capability of subjects to cooperate in PD and SH. To assess this effect, therefore, we compare the cooperation levels achieved in our experiment against those of D&F that we use as a baseline.

Hypothesis 1: Increasing (almost doubling) the efficiency component brings a significantly higher frequency of cooperation in our experiment than in D&F, both in the PD and SH game.

To test for this hypothesis, we compare the overall rate of cooperation that agents reach in each stage game of our experiment with the overall cooperation rate reached in each corresponding stage game in the experiment of D&F (see Figure 3.1). The only

[18]The experiment was carried out in Italian. The English version of the instruction is available in the Appendix Section.

difference between the two experiments is the value of efficiency. For this reason, the model of LiCalzi and Mühlenbernd (2021) predicts that in our experiment, we should observe a greater or equal level of cooperation in each stage game. Efficiency is an extrinsic feature of the model that is increasing in the probability of cooperation in both SH and PD.

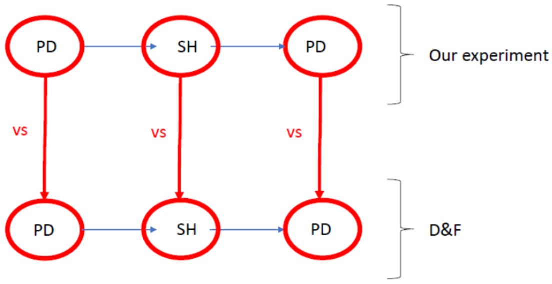


Figure 3.1: In the first hypothesis, we compare the average rate of cooperation reached in each stage game in our experiment with the corresponding stage game in the experiment of D&F. This figure presents the case of a comparison for a PD-SH-PD treatment, but the same comparison is made for SH-PD-SH treatments.

The second hypothesis investigates the impact of the temptation parameter on the capability of subjects to cooperate in PD and SH. To assess this effect, therefore, we compare the cooperation levels achieved in PD and SH games in treatments with high temptation against treatments with low temptation (see Figure 3.2).

Hypothesis 2: Increasing the temptation parameter brings a significantly lower frequency of cooperation in the PD game. Instead, increasing the temptation component brings a significantly higher frequency of cooperation in the SH game.

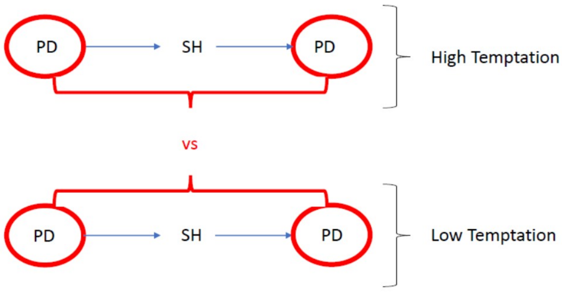


Figure 3.2: In the second hypothesis, we compare the average rate of cooperation reached in each game (PD as in this Figure, or SH) in the high temptation treatments against the cooperation rate reached in the same game but in the low temptation treatment.

For Hypothesis 2, the model of LiCalzi and Mühlenbernd (2021) has different predictions in the SH and in the PD game. In fact, the temptation parameter is increasing in cooperation in the SH and decreasing in cooperation in the PD. In treatments with high temptation, we should observe greater or equal cooperation in SH and lower or equal cooperation in PD with respect to the treatments with low temptation. In the first two hypotheses, we want to understand the role that temptation and efficiency play in determining the levels of cooperation achieved in the several versions of PD and SH used in the experiments.

In the third hypothesis, instead, the aim is to evaluate the role that temptation and efficiency play in the possible transfer effect between these two classes of games. In particular, within each treatment, we test whether the same subjects that start playing a PD (SH) stage game and reach a cooperation rate in the first stage change their behavior at the beginning of the third stage when they play the same PD (SH) again after having experienced an indefinite repeated second stage game of SH (PD). We are interested in understanding whether their behavior changes in the first round of the third stage and, only if this is the case, whether this change is kept in the subsequent rounds.

Hypothesis 3: The cooperation level that subjects reach in the third stage is different from the cooperation rate that they reached overall in the first stage. This difference may only appear in the first round of the third stage (fleeting transfer) or be also sustained in subsequent rounds (permanent transfer).

We test whether the overall cooperation rate reached in a PD (SH) in the first stage game is significantly different from the average cooperation rate reached in the first round of the third stage for the same version of PD (SH) game played after having experienced a SH (PD) stage game. We define this type of transfer as *fleeting* transfer. We consider this type of transfer important for a continuation argument. If there were no initial fleeting transfer, we would expect subjects to continue cooperating in the first round of stage 3 at a level not statistically different from what they had achieved overall in stage 1. Suppose the level of cooperation in the first round is different and increases or decreases with the experience of the second stage with the other game. In that case, it is possible to evaluate this difference as a transfer (positive or negative) from the second stage to the beginning of the third stage.

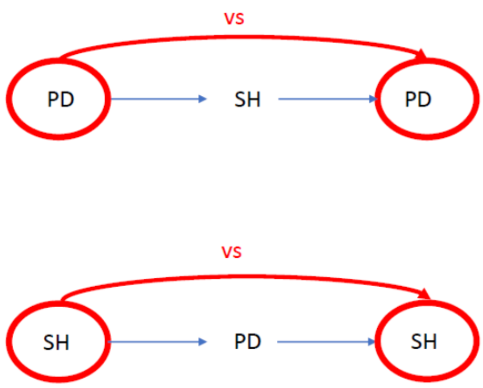


Figure 3.3: In the third hypothesis, we compare the average rate of cooperation reached in the first stage game by a group of subjects with the average cooperation rate reached in the third stage game.

If there is a fleeting transfer, our setting also allows us to control for the persistence of this transfer between games. We can compare the rate of cooperation reached in the first rounds of the third stage game with respect to the overall cooperation rate reached in the overall same stage game to see whether the transfer persists in the entire stage, i.e., there is no significant difference between how subjects cooperate at the beginning of the third round and in the third round overall. In this case, we define this type of transfer as *permanent* transfer.

The prediction of the model of LiCalzi and Mühlenbernd (2021), for hypothesis 3 is more complex, especially for the case of fleeting positive transfer from SH to the first round of PD.
When agents face a PD after a recent history of high cooperation in the SH game, according to the update rule of the model of LiCalzi and Mühlenbernd (2021), there are two conflicting possible effects. On the one hand, when switching from SH to PD, the incentive of temptation, which favored cooperation in SH, changes radically and favors defection in PD. In this case, the new strategy structure would predict a rapid adjustment towards defection in the subsequent PD. On the other hand, after high cooperation in SH, the harmony feature increases, increasing the disposition to achieve the highest common payoff, increasing the probability of cooperating in the next PD. The prediction of the model of a fleeting positive transfer from SH to PD thus depends on the magnitude of these two conflicting effects. After a stage with high levels of cooperation in SH and with the same efficiency, the higher the temptation in the next PD, the more difficult the possibility of positive transfer. Vice versa, the presence of a lower temptation could favor the effect of the increased harmony and, consequently, the positive transfer.

Efficiency may also play a role. With the same temptation, greater efficiency may favor positive transfer by increasing cooperation in the previous stage of SH and encouraging cooperation in the subsequent PD.
Conversely, the prediction is different when we evaluate the persistence of the fleeting positive transfer from SH to PD in the subsequent rounds. In this case, the model predicts that, even after a possible initial higher cooperation in the PD, greed would continuously increase together with the disposition towards the highest payoff, and consequently, this higher cooperation would progressively reduce, preventing a permanent positive transfer from SH to PD.
In the fourth hypothesis, we look for possible order effects between treatments, i.e., we consider the average cooperation rate reached in the first stage PD (SH) and compare it with the overall cooperation rate reached by another group of subjects that play

the same version of PD (SH) but in the second stage, after having experienced a stage game of SH (PD).

Hypothesis 4: The groups that start experiencing a SH (PD) stage game before playing the PD (SH) stage game show overall a significantly greater (lower) level of cooperation than groups of subjects that do not have this previous experience and start playing the PD (SH) stage game.

In hypothesis 4, we consider the average cooperation rate reached by a group of subjects for a given version of SH or PD, and then we compare it with the average cooperation rate reached by another group of subjects after having experienced the other stage game.

Figure 3.4: In hypothesis 4, we compare, in each experiment, the average cooperation rate reached by a group of subjects for a given version of SH or PD with the average cooperation rate reached by another group of subjects after having experienced the other stage game.

3.6 Results

The analysis is conducted by jointly considering our results and those of D&F[19]. In particular, we first analyze the impact and the role of efficiency and temptation in determining the achieved levels of cooperation in the several versions of SH and PD,

[19]We would like to thank John Duffy and Dietmar Fehr, who kindly allowed us to use their *zTree* program to reproduce their experiment in an accurate way, thus helping the comparison of the results.

and then we study their possible role in the transfer effects between these two types of strategic games. To study the impact of efficiency, we compare our results against those of D&F. To study the impact of temptation, we analyze the results within each of the two experiments.

3.6.1 Efficiency promotes cooperation

To assess the impact of efficiency, we compare the average cooperation rates obtained in our experiment against the results of D&F, comparing stage games with the same values of the parameter x and played in the same order.

Treatment	**Experiment**	**Stage 1**		**Stage 2**		**Stage 3**	
		FIRST ROUND	**OVERALL**	**FIRST ROUND**	**OVERALL**	**FIRST ROUND**	**OVERALL**
PD30-SH10	**D&F**	0,60	0,11	0,90	0,99	0,20	0,09
	NEW	0,64	0,34	0,86	0,92	0,40	0,30
SH10-PD30	**D&F**	0,87	0,99	0,40	0,07	1,00	1,00
	NEW	0,88	0,93	0,54	0,38	0,98	0,98
PD25-SH15	**D&F**	0,43	0,10	0,68	0,63	0,35	0,12
	NEW	0,76	0,39	0,92	0,96	0,64	0,47
SH15-PD25	**D&F**	0,75	0,68	0,28	0,05	0,70	0,67
	NEW	0,88	0,70	0,42	0,32	0,80	0,85

Table 3.15: Frequency of cooperation in all rounds in our experiment (row "New") and in the experiment of Duffy and Fehr (row "D&F"), by treatment.

Table 3.15 reports, for each treatment, the average cooperation rate reached in the first round and overall in each stage game.

In each box of the table, the number on the upper side of the box is the average cooperation rate in the experiment of Duffy and Fehr (2018) (labeled "D&F"), while the number on the lower side of the box is the one from our experiment (labeled "NEW"). We start by considering the average cooperation rate reached in the overall PD stage game in the treatments with high temptation, where $x = \{10, 30\}$ (PDSH3010 and SHPD1030). According to the non-parametric Mann-Whitney test, cooperation in PD is significantly higher in our experiment than in D&F in each of the three stages (in the first stage: 0.34 vs 0.11, in the second stage: 0.38 vs 0.07 and in the third stage: 0.3 vs 0.09; for all the three stages $p < 0.01$).

We do not observe an increase in the level of cooperation in the SH in these two treatments (PDSH3010 and SHPD1030), but this was somehow expected as already Duffy and Fehr (2018) reached a convention of cooperation equal to 0.99, and this level is not lower than the one reached in our experiment.

Now we consider the average cooperation rate reached in the overall PD stage game in the treatments with low temptation, where $x = \{15, 25\}$ (PDSH2515 and SHPD1525). Subjects in our experiment increase the frequency of cooperation in the PD game in each stage of the experiment with respect to D&F when temptation is low (0.10 vs 0.39, 0.05 vs 0.32 and 0.12 vs 0.47; all three p values $p < 0.01$). Instead, subjects in our experiment increase the frequency of cooperation in the SH game in the second and third stage (0.63 vs 0.96 and 0.67 vs 0.85, both $p < 0.05$) but surprisingly *not* in the first and third stages. In the first stage, in fact, subjects in our experiment reach almost the same frequency of cooperation observed in D&F (0.68 vs. 0.70)[20].

Overall, our experiment provides evidence for Hypothesis 1; we find that the cooperation rate reached in our experiment is greater or equal in each stage game with respect

[20] In general, as confirmed by the next section, subjects have more difficulty learning to cooperate in SH with low temptation, both in our experiment and in DF. One reason why we do not observe an increase in cooperation in the first stage may be related to the fact that subjects need more rounds to learn to cooperate, in fact in stage 3 of the same treatment they cooperate significantly more, despite experiencing PD in stage 2.

to D&F. Figure 3.5 and Figure 3.6 show, respectively, the average cooperation rate in the treatments with high (PDSH3010 and SHPD1030) and low temptation (PDSH2515 and SHPD1525)[21]. In Figure 3.5 and Figure 3.6 each row represents a treatment, while each of the three graphs in a row is a stage game of a treatment.

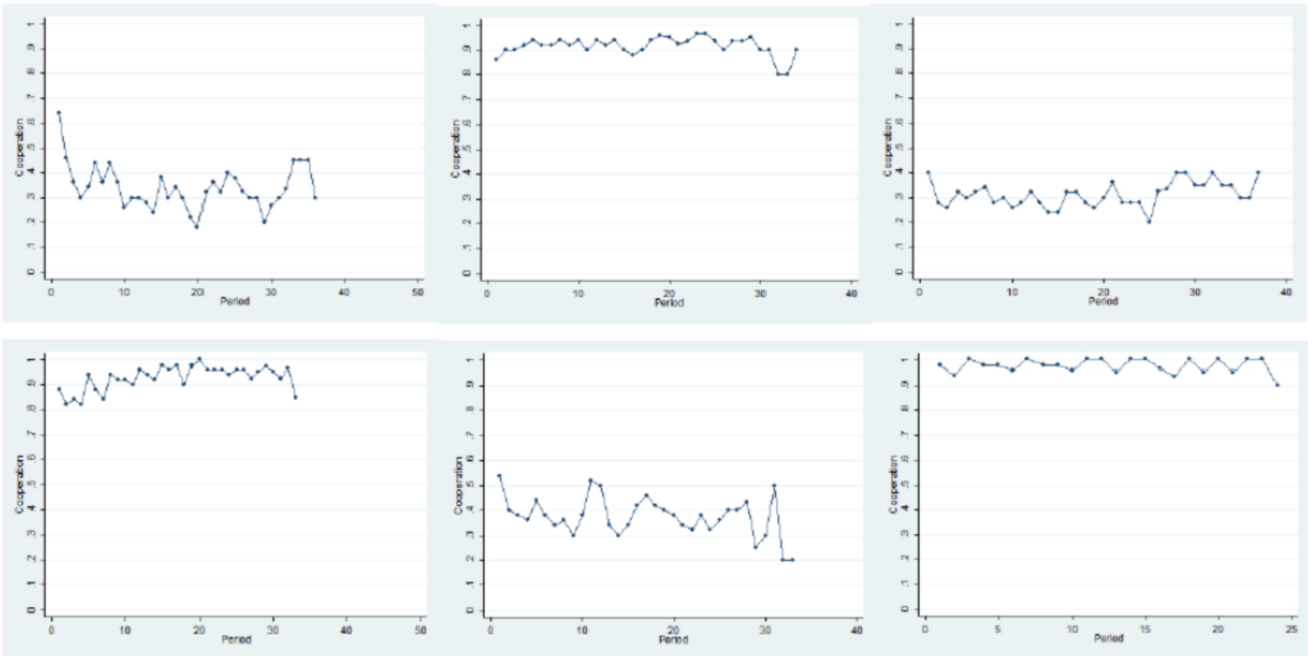


Figure 3.5: Aggregate frequency of cooperation by Period in our experiment in the treatments with high temptation: PDSH3010 (top panel) and SHPD1030 (bottom panel). Each graph, starting from the left, represents a stage game.

3.6.2 The impact of Temptation

In their experiment, D&F find that, in treatments with low temptation SH, subjects cooperate less than in treatments with high temptation, while they find no significant difference in cooperation in the PD. To assess the impact of temptation in our experiment, we refer to the regression in Table 3.14. The regression reports marginal effects after a probit on the relevant variables in the subjects' choice of whether to cooperate or not.

[21]In the Appendix 8.1, we also provide the average cooperation rate by treatments in the experiment of D&F in Figure 10 and 11; the average cooperation rate in each session of our experiment in Figure 6, Figure 7, Figure 8, and Figure 9 and in each session of the experiment of D&F in Figure 12 and in Figure 13.

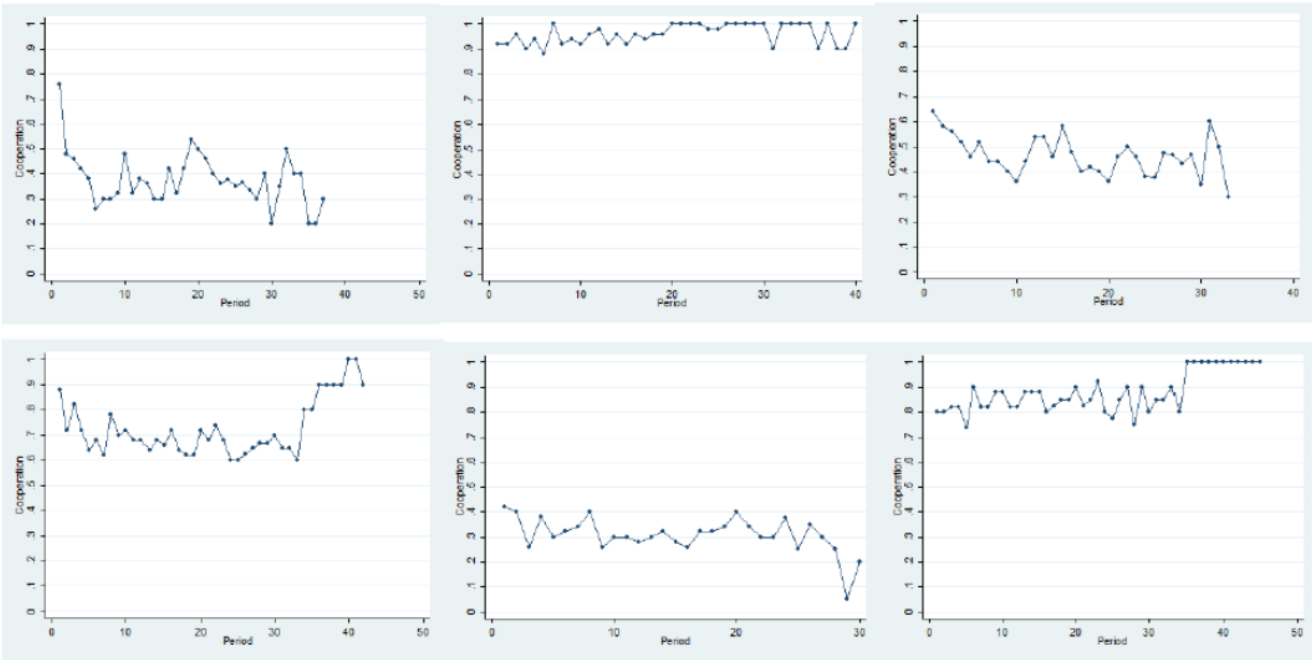


Figure 3.6: Aggregate frequency of cooperation by Period in our experiment in the treatments with low temptation: PDSH2515 (top panel) and SHPD1525 (bottom panel). Each graph, starting from the left, represents a stage game.

The dependent variable is *choice*, which is equal to 1 if the subject chooses to cooperate and 0 otherwise. The variable *choice* is analyzed in both the PD and the SH games. The first three columns (1)-(3) concern PD and consider only stage 1 and 3 of the treatments PDSH3010 and PDSH2515, the last three columns (4)-(6) concern SH and consider only stage 1 and 3 of the treatments SHPD1030 and SHPD1525. In both cases, the regressions progressively add control variables. The variables in the regressions in columns (1) and (4) are *belief*, the number of subjects that an individual think that will choose to cooperate at the beginning of a new sequence[22], *stage3*, a dummy equal to 1 in stage 3 and equal to 0 otherwise, *length*, which is a time trend that controls for the indefinite number of rounds in each session, *LowT*, a dummy variable equal to 1 for treatments with low temptation and equal to 0 otherwise. In columns (2) and (5), we add the control variables *age*, the age of the subjects and *female*, a dummy equal to 1 if the subject is a female and 0 if he is a male and we add *stage3lowT*, an interaction between *stage3* and *LowT*[23]. This interaction variable

[22]We asked each player, at the beginning of a new sequence, to guess the number of players, from 0 to 10, that she think will choose to cooperate in the forthcoming round.

[23]The magnitude of the interaction effect in nonlinear models does not equal the marginal effect

stage3lowT allows us to assess whether there are transfer effects from stage 2 to stage 3 that depend on the level of temptation, high or low. Finally, in columns (3) and (6), we add other control variables we took from an unincentivized questionnaire that the subjects answered after the experiment. In particular, we add *field* that represents a dummy variable relative to the academic field of study (equal to 1 if the subject is a student of Economics and equal to 0 otherwise) and *job*, a dummy equal to 1 whether the subject works. Moreover, *game theory basic* and *game theory adv* that are dummies equal to 1 if the subject has already taken a course in basic or advanced game theory, *voluntary*, if the subject carries out voluntary activities *act pol*, a dummy equal to 1 if the subject takes part in political activities and to 0 otherwise. Finally, *incomefamily*, *trust*, *helpothers*, *risk*, *reducing attitude*, *deservingness*, *effort* and *life satisfaction* in a scale from 1 to 10 measure respectively how much the subject perceives the income of her family, how the subject believes she can trust other people, how the subject believes helping other people represents a moral obligation, subject's level of risk aversion, how much important the subject perceives to reduce income inequality, how much the subject perceives deservingness as relevant for income inequality, how much the subject perceives effort as relevant for income inequality and how the subject is satisfied with her life.

The negative and significant coefficient of *LowT* in columns (4)-(6) indicates that subjects cooperate less in the SH game in the low temptation treatments in the first stage. The same coefficient is not significant in columns (1)-(3), meaning that high or low temptation has no significant effect on cooperation in the PD game in the first stage. These results confirm the findings of D&F; low temptation induces less cooperation in SH, while we do not find a significant effect in the PD. The positive and significant coefficient of *belief* indicates that subjects increase cooperation the greater the number of cooperators they think in that round will cooperate. The positive and significant coefficient of *length* in the last three columns is in line with what was observed in the

of the interaction term, can be of opposite sign, and its statistical significance is not calculated by standard software (Ai and Norton, 2003). We therefore use the `inteff` command on STATA to compute the correct marginal effect as well as the correct standard errors (see Norton et al., 2004).

previous paragraph about the impact of efficiency on cooperation; the difference in the first stage is not significant because subjects learn to cooperate as the rounds pass. Finally, older subjects cooperate significantly more in the PD while females cooperate significantly less in both games[24].

Table 3.14: Cooperation choice in PD and SH.

	PD			SH		
Choice	(1)	(2)	(3)	(4)	(5)	(6)
LowT	0.103*	0.022	0.052	-0.197***	-0.190***	-0.189***
	(0.057)	(0.052)	(0.086)	(0.039)	(0.035)	(0.069)
stage3	0.006	-0.055	-0.039	0.040	0.045	0.048
	(0.051)	(0.056)	(0.045)	(0.032)	(0.036)	(0.033)
stage3lowT		0.109**	0.103**		-0.008	-0.005
		(0.045)	(0.043)		(0.045)	(0.042)
length	0.000	0.001	0.000	0.002***	0.003***	0.002***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
belief	0.034***	0.034***	0.035***	0.012***	0.012***	0.012***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)
age		0.024**	0.024**		-0.001	-0.001
		(0.011)	(0.012)		(0.006)	(0.005)
female		-0.079	-0.135***		-0.131***	-0.121***
		(0.057)	(0.050)		(0.039)	(0.038)
field			-0.031			-0.004
			(0.063)			(0.043)
job			-0.013			0.005
			(0.035)			(0.023)
voluntary			-0.023			-0.009
			(0.038)			(0.030)
game_theory_basic			-0.023			0.055*
			(0.055)			(0.032)
game_theory_adv			0.111			-0.017
			(0.090)			(0.040)
act_pol			-0.003			-0.030
			(0.048)			(0.038)
incomefamily			0.014			-0.005
			(0.014)			(0.008)
trust			0.050***			0.003
			(0.013)			(0.009)

[24]In line with our results, previous findings show that women are more cooperative than the average man when risk is low and less cooperative when risk is high (see Mengel, 2018).

helpothers			0.033***			0.001
			(0.012)			(0.009)
risk			-0.004			0.005**
			(0.002)			(0.002)
reducing_attitude			-0.042***			-0.003
			(0.012)			(0.010)
deservingness			-0.012			0.005
			(0.012)			(0.009)
effort			-0.012			0.002
			(0.012)			(0.009)
life_satisfaction			-0.002			-0.007***
			(0.003)			(0.003)
Log likelihood	-3797.376	-3708.288	-3373.430	-2034.315	-1956.331	-1906.970
Wald chi2	77.89	111.79	216.47	75.61	82.45	130.30
Prob >chi2	0.000	0.000	0.000	0.000	0.000	0.000
Observations	5,900	5,900	5,900	5,420	5,420	5,420

Notes. Columns (1), (2), (3), (4), (5) and (6) report results of the marginal effects from a probit regression with standard error clustered at the group level. Columns (1), (2) and (3) consider the stage 1 and 3 of treatments PDSH3010 and PDSH2515, columns (4), (5) and (6) consider the stage 1 and 3 of treatments SHPD1030 and SHPD1525. The dependent variable is *choice*, a dummy that assumes value 1 if the subject chooses to cooperate and 0 otherwise. Significance levels are denoted as follows: * $p < 0.1$, **$p < 0.05$, and *** $p < 0.01$.

3.6.3 Positive transfer when temptation is low

We find that in the treatments where the temptation is low (PDSH2515 and SHPD1525), there is a positive within-subjects fleeting transfer effect, both in our experiment and in the experiment of Duffy and Fehr (2018). According to the non-parametric Mann-Whitney test, the same subjects that start playing a PD in the first stage game significantly cooperate more when playing the first rounds in the third stage game after having learned to coordinate in the SH stage game (0.39 vs. 0.64, $p < 0.05$ in our experiment and 0.10 vs. 0.35, $p = 0.05$ in the experiment of D&F).

In the treatments where temptation is high (PDSH3010 and SHPD1030), there are no significant fleeting transfer effects; according to a non-parametric Mann-Whitney test, the level of cooperation reached in the first stage (both in the PD and in the SH game) is not different from the level of cooperation reached in the first round of the third stage.

A possible criticism of this result could be that it is actually driven by a "restarting effect". In fact, as previously observed, the subjects in the first stage of PD, tend to show average levels of cooperation and then quickly return to defect. We cannot completely exclude this possibility; however, the fact that a fleeting positive transfer occurs in both experiments only in treatments with low temptation supports the interpretation of the significant role of temptation. Assuming the presence of a restarting effect, we would expect to see a fleeting transfers in all treatments, while we observe that it occurs only in treatments where temptation is low, that is consistent with the theoretical approach we adopt. In fact, according to the theoretical approach we adopt, positive transfer is more likely in the presence of lower temptation and, in fact, we find this fleeting transfer effect in both experiments only in treatments with low temptation. However, the fleeting positive transfer that we find in the treatment PDSH2515, both in our experiment and in D&F, is not persistent. In fact, we also confirmed a stylized fact in the literature that prevents the presence of possible permanent transfers: in the PD game, subjects tend to decrease cooperation over time; the average cooperation reached in the first rounds of each PD stage game is always greater than the average cooperation rate reached when considering the average of the entire PD stage game.

However, even if it is true that cooperation declines over time, the speed of decrease is not uniform. The positive and significant coefficient of the interaction variable *stage3lowT*, in the three columns (1)-(3), means that, even if the cooperation is decreasing over time, in the PD game played in the third stage of the PDSH2515 treatment, the subjects cooperate overall on average significantly more than in the first stage, even after controlling for all the other relevant factors[25]. Instead, the positive and non-significant coefficient of the interaction variable *stage3lowT*, in the three columns (4)-(6), indicates that in the SH game played in the third stage of the PDSH2515 treatment, the subjects do not cooperate overall on average significantly more than in the

[25]The `ineff` test performed in STATA after the probit regression confirms that the interaction coefficient is significant.

first stage[26]. In particular, it is possible to notice this dynamic also comparing the first and the third graph of the top panel of Figure 5. In both cases, the PD starts with a cooperation rate greater than 0.5, but in the first stage, the reduction in cooperation is steeper than in the third stage.

We conclude that even if the fleeting transfer in the PDSH2515 treatment is not persistent, the greater efficiency in our experiment delays the return to defection with respect to the same treatment in Duffy and Fehr (2018). This effect of the fleeting positive transfer is also particularly strong in single sessions 2, 14, and 20[27].

As D&F, we never find any negative transfer, i.e., conventions to defect learned in the PD games never affected the first round behavior in the subsequent SH game. However, D&F, in two sessions, find some instances of negative transfer[28] while we never observe any; high efficiency also helps to prevent negative transfer, even in single sessions.

3.6.4 Order effects between treatments

In this section, we focus our attention on possible order effects. Specifically, we ask whether groups of subjects playing one type of game, either PD or SH, in stage 1 achieve different levels of cooperation on average than other groups of subjects playing the same game, but in stage 2, after experiencing the other game in stage 1. Consistently with the findings in Duffy and Fehr (2018), we do not find order effects between subjects, either positive or negative. Independently from the level of temptation and efficiency, we can conclude that, when we consider the overall average cooperation rate of a stage game, subjects that start playing the SH stage game do not cooperate more in the subsequent PD stage game with respect to other subjects that start playing the PD stage game directly without having experienced the SH.

The same is true the other way around; subjects that start playing the PD stage game do not cooperate less in the subsequent SH stage game with respect to other subjects that start playing the SH stage game directly without having experienced the PD.

[26]The `ineff` test performed in STATA after the probit regression confirms that the interaction coefficient is non-significant.

[27]see Figure 8 in Appendix 8.1.

[28]see Group 9 and Group 11 in Figure 13 in Appendix 8.1.

3.7 Conclusion

In this experiment, we disentangle the relative role of temptation and efficiency in promoting cooperation and transfer of experience between the SH and PD games.
We provide new experimental evidence, and we also exploit the results of Duffy and Fehr (2018) to support our analysis.
Overall, we confirm the main results of D&F, adding on the role of efficiency on cooperation and transfer between SH and PD. We conclude that efficiency has a positive and significant role in increasing the ability of subjects to cooperate both in SH and PD games.
Temptation, instead, significantly promotes the ability of subjects to coordinate on the efficient equilibrium in the SH while it does not have a significant impact on the ability of subjects to cooperate in the PD. The fact that efficiency plays a significant role in both types of game and temptation does not in the PD, may also be due to the fact that the variation considered in efficiency (equal to 19/20 in our experiment, and equal to 1/2 in DF) is greater than that considered in temptation (in the PD versions in both experiments it goes from 1/3 to 1/5 in both experiments).

The transfer between SH and PD is also rather limited. In general, subjects tend to cooperate in SH and to increase cooperation as the rounds go by, whereas in PD, the tendency is to start with average levels of cooperation and then quickly tend towards non-cooperative equilibrium.

In both experiments, we found that temptation can play a role in a fleeting positive transfer from SH to PD. Efficiency, on the other hand, has a lower relevance in the transfer, where we see that it contributes to reinforcing the effect of temptation, delaying the return to defection in our experiment where efficiency is higher than in DF. Indeed, when temptation is low, we observe that in the first round of PD in stage 3, after the second stage of SH, cooperation is higher than the average cooperation achieved by the same subjects playing the same PD in stage 1.

Another issue, raised by D&F themselves, is related to a possible "demand effect" that may have been triggered by the characteristics of the experimental design, especially by changing the color of the table from black to red when switching from PD to SH and vice versa, leading some participants to encourage a change in their behavior that may have further limited the transfer between SH and PD. While acknowledging this possibility, we decided to stick to their experimental design to make our results comparable, with the choice of the color changes due to our willingness to avoid “habituation effects”.

Overall, our findings are consistent with the theoretical model of LiCalzi and Mühlenbernd (2021). In the model, efficiency is an external feature that incentivizes cooperation in both SH and PD, while temptation is an external feature that incentivizes cooperation in SH and defection in PD. The model then admits transfers of experience between one game and another but is also consistent with and open to the rapid adjustment that we observe when we move from SH to PD (and vice versa) due to the change in strategic features between the two games.

Finally, the cases in which we find transfer effects are also consistent with the dynamics of the model; low temptation and high efficiency may favor a positive experience transfer from SH to PD.

These experiments confirm that the transition of experience from SH to PD is not easy and can be sensitive to the structural details of the payoffs of the two games. Understanding what can favour positive transfers and what can promote their durability can be of great interest, not only academically but also in public policy or real-world contexts.

The results of this work suggest that working further on efficiency and temptation, increasing the former and reducing the latter, can provide additional support for positive transfer between SH and PD. In addition, future work may also try to disentangle the role of risk.

3.8 Appendix

3.8.1 Further Material

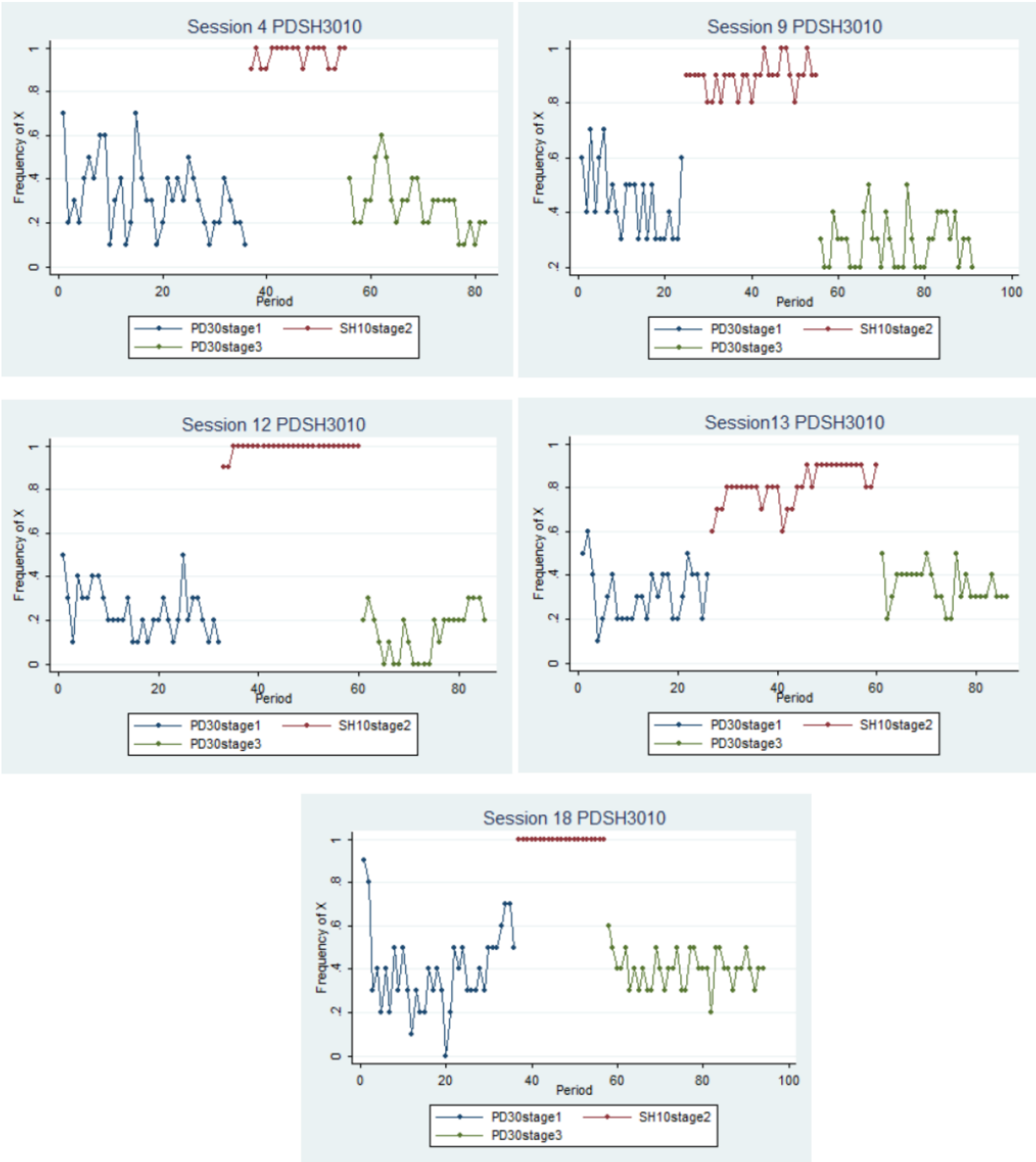


Figure 3.7: Frequency of cooperation in our experiment in the PDSH3010 treatment, by session.

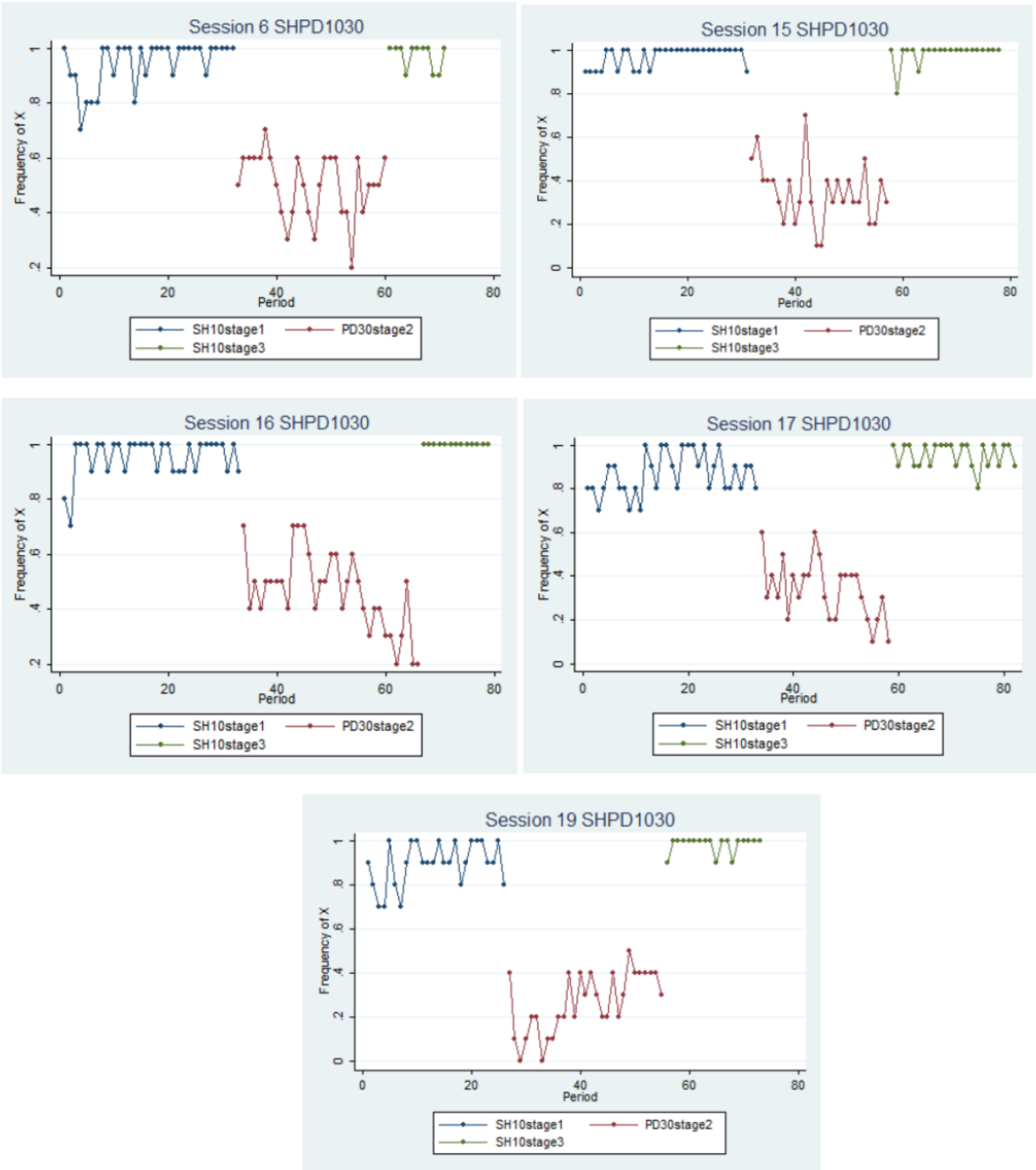


Figure 3.8: Frequency of cooperation in our experiment in the SHPD1030 treatment, by session.

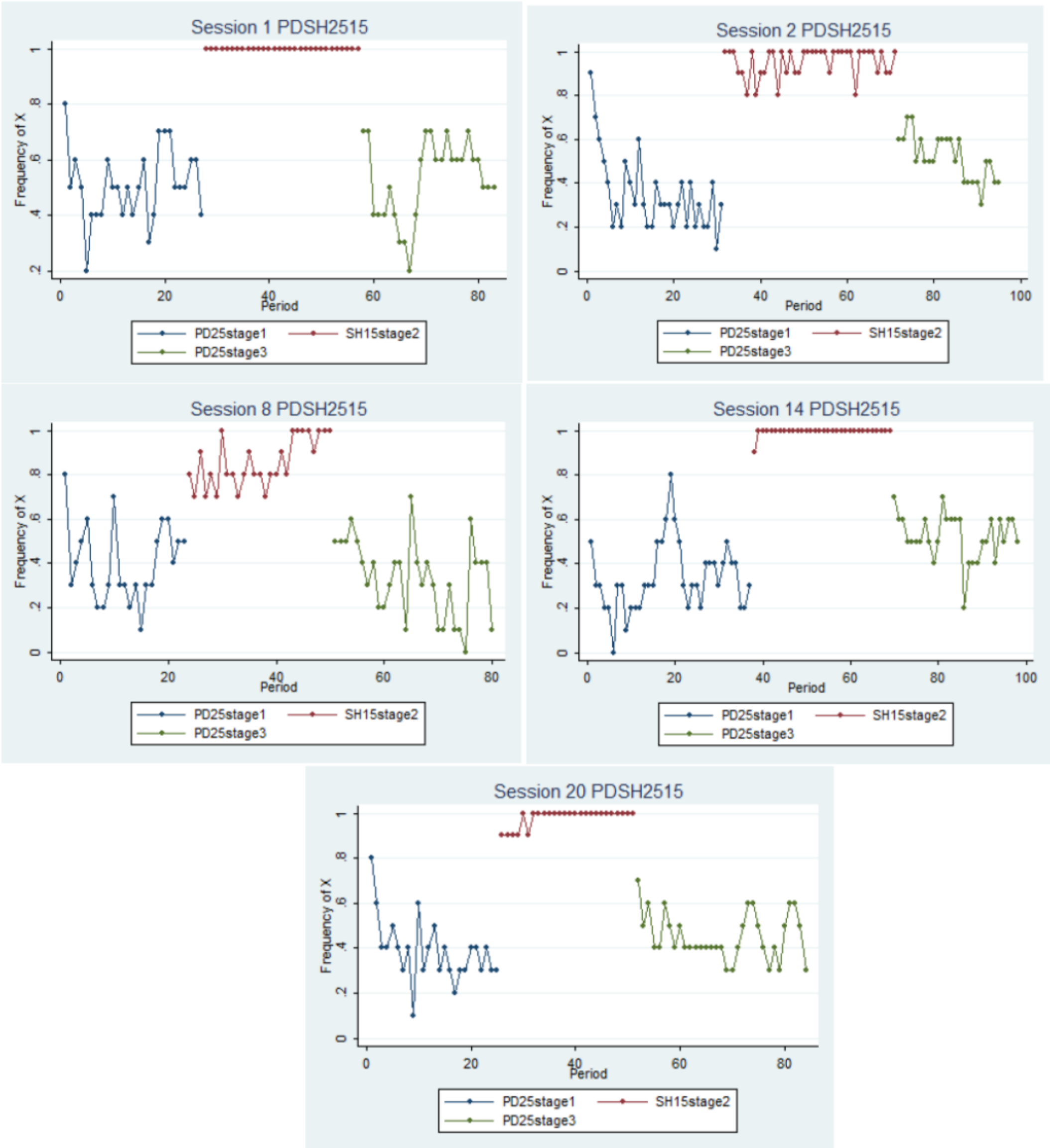


Figure 3.9: Frequency of cooperation in our experiment in the PDSH2515 treatment, by session.

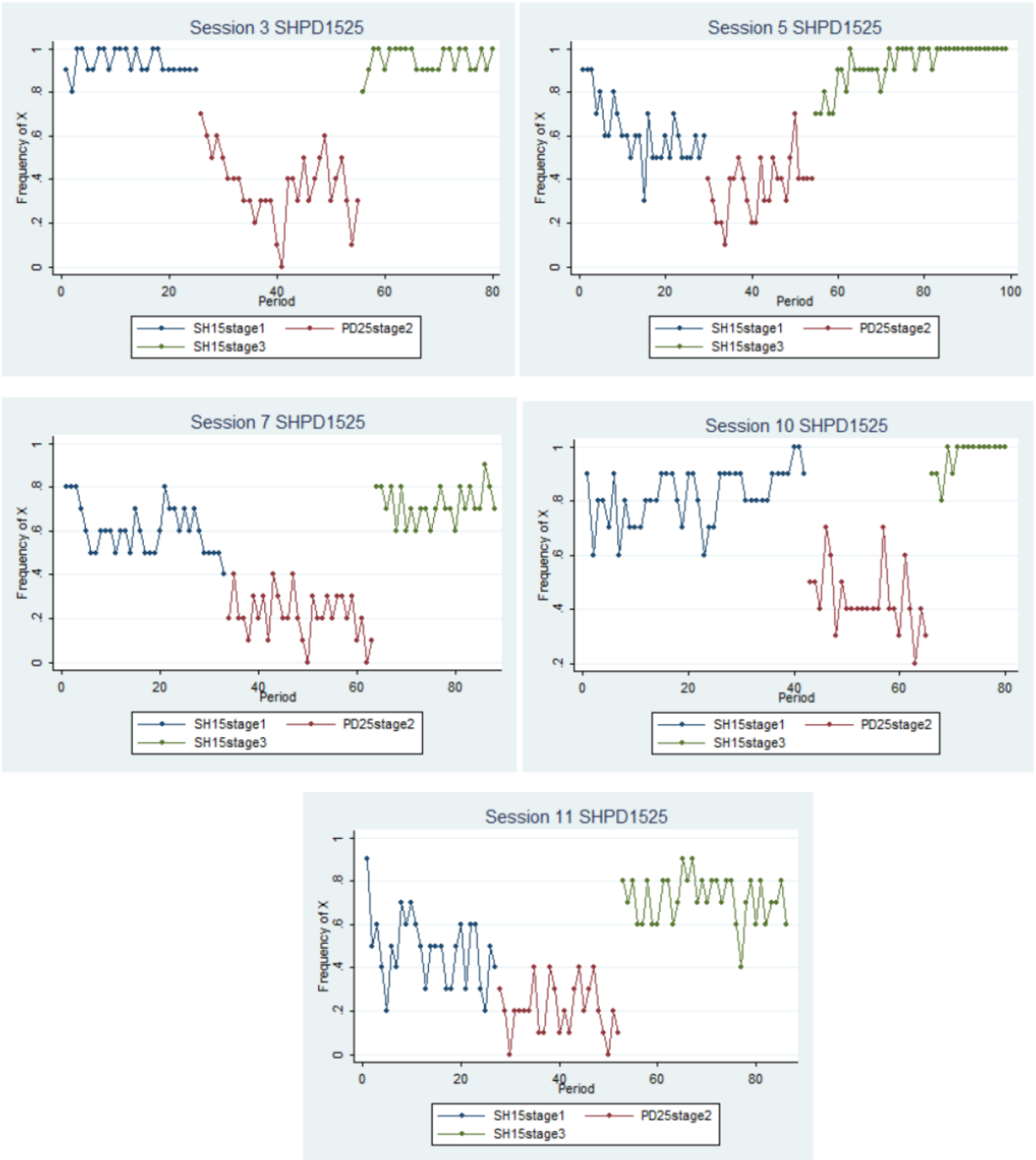


Figure 3.10: Frequency of cooperation in our experiment in the SHPD1525 treatment, by session.

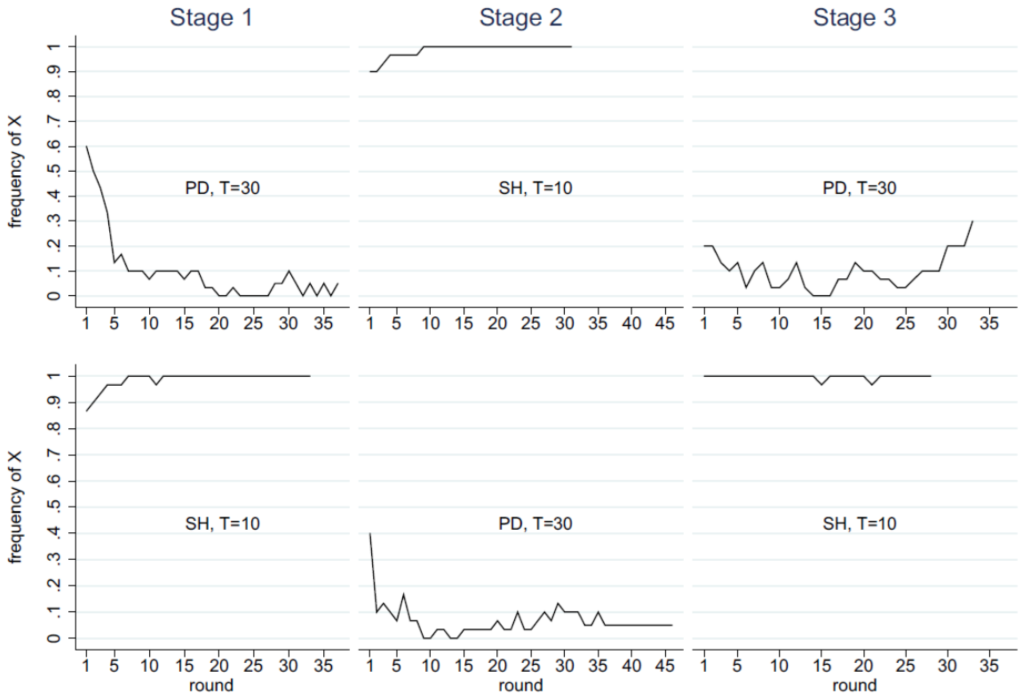


Figure 3.11: Aggregate frequency of cooperation in Duffy and Fehr (2018) in each stage of PD30-SH10 (top panel) and SH10-PD30 (bottom panel)

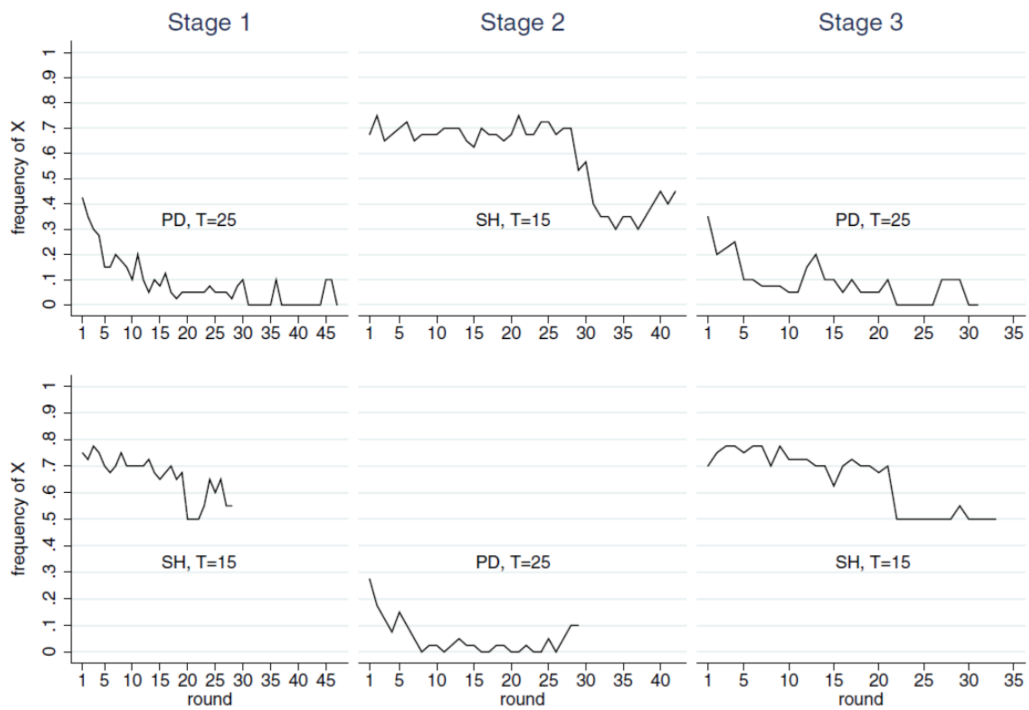


Figure 3.12: Aggregate frequency of cooperation in Duffy and Fehr (2018) in each stage of PD25-SH15 (top panel) and SH15-PD25 (bottom panel)

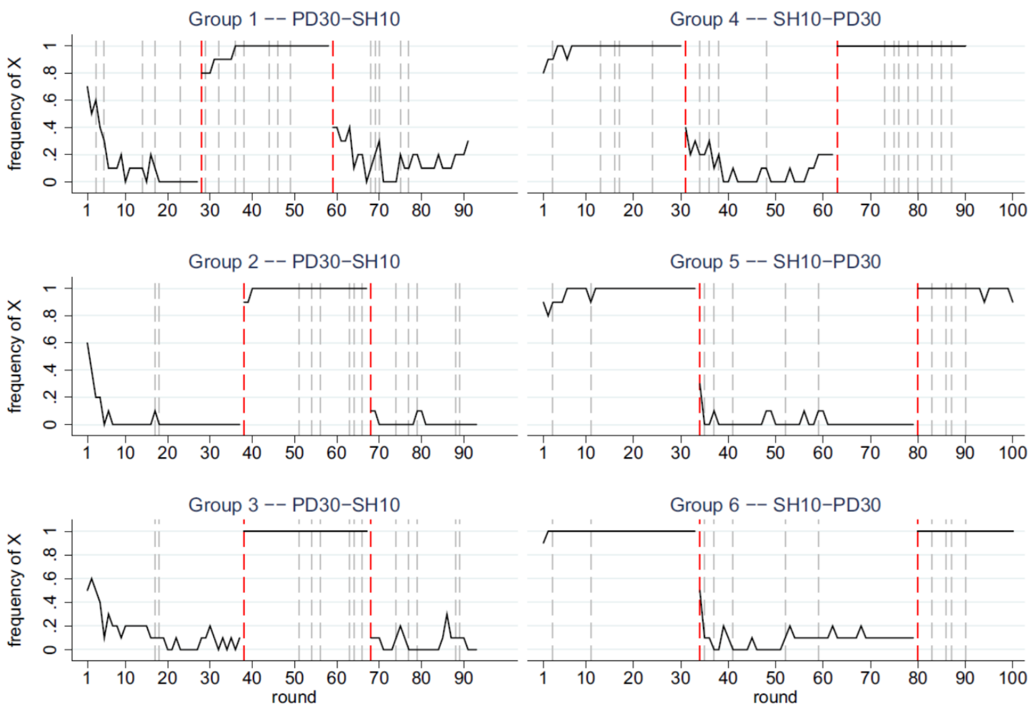


Figure 3.13: Frequency of cooperation Duffy and Fehr in the SHPD1030-PDSH3010 treatments, by session.

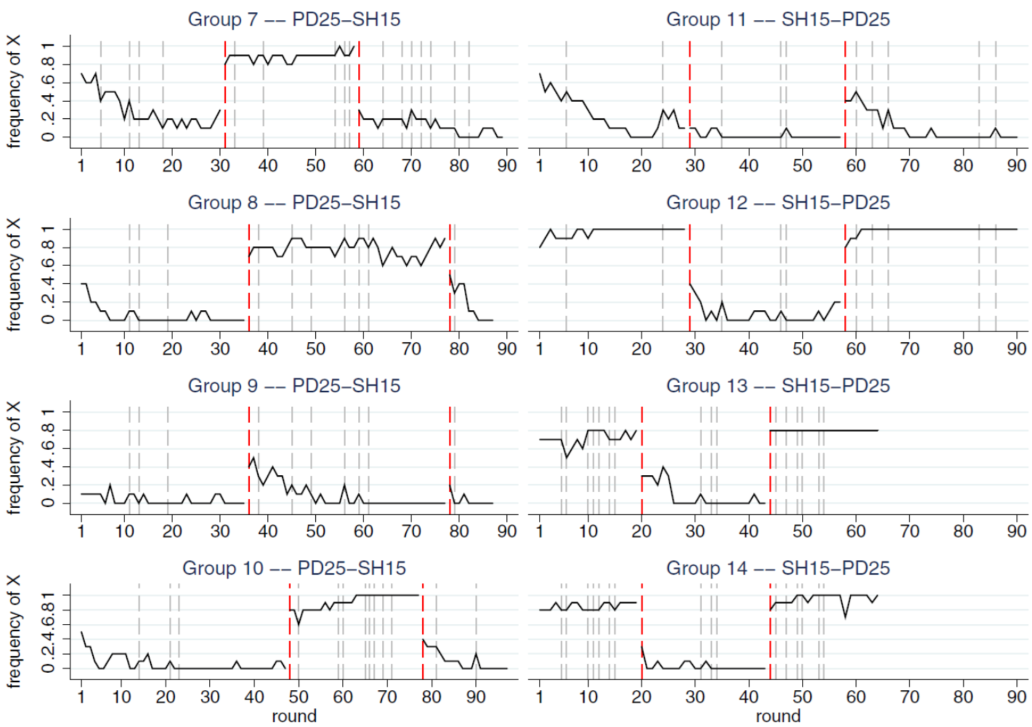


Figure 3.14: Frequency of cooperation Duffy and Fehr in the SHPD1525-PDSH2515 treatments, by session.

3.8.2 Instructions

AN EXPERIMENT OF ECONOMIC DECISIONS

Welcome and thank you for participating in this experiment

The experiment will last about an hour and a half. For your participation you will receive 3 Euros and you will have the opportunity to earn more money depending on the decisions you make during the experiment. Your final earnings will be paid to you immediately upon completion of the experiment. Your decisions will be kept anonymous and earnings will be kept confidential.

We will now start with an experimenter who will read the instructions aloud. At the conclusion of these instructions, you and the other participants can ask questions. Then the experiment will begin with the decisions made on the computer. At the end of the experiment a short questionnaire will be conducted.

During the experiment it is not allowed to speak or communicate in any way with the other participants. We also encourage you and other attendees to turn off their cell phones or other mobile devices. If you have any questions, at any time during the experiment, write in the chat and one of the experimenters will answer you personally.

Instructions

There are 10 participants in today's session with whom you will interact during the experiment.

The experiment consists of a series of "sequence". Each sequence is made up of an indefinite number of "rounds". In each round you will be grouped into 5 pairs randomly and anonymously and asked to make a decision. Grouping with one of the other 9 participants is completely random, therefore, in each round, you will have 1/9 chance of being paired with any of the other 9 participants.

At the beginning of the first sequence you will be assigned an identification number (ID) which will remain the same for the entire duration of the experiment and will only be used at the end to obtain the final payment. You will never be given the identification number (ID) of the people you interact with in the various rounds and none of them will be given your ID. In each round you and the other participant with whom you will be randomly grouped, will have to choose between two possible options called "X" and "Y". Each of the two grouped participants will make their decision independently and without knowing the decision of the other participant.

To choose, simply select the option button you prefer and then click the "OK" button. After all participants have made their choice, you will be informed of the decision of the participant you were grouped with in the round. Likewise, only the participant grouped with you will be aware of your choice.

Your decision, together with the decision of the other participant in the group with you, will determine one of four possible scenarios: (X, X), (X, Y), (Y, X) and (Y, Y). In these scenarios, the first letter in brackets corresponds to your decision, the second to that of the other participant in the group with you. The type of scenario that occurs will determine your gain and that of the other participant in the group with you in the round.

There are two possible ways in which the four scenarios turn into earnings and are presented below by Tables 1 and 2.

Table 1	Other's decision: X	Other's decision: Y
Your Decision: X	Your Payoff: 20 Other's Payoff: 20	Your Payoff: 0 Other's Payoff: **25**
Your Decision: Y	Your Payoff: **25** Other's Payoff: 0	Your Payoff: 1 Other's Payoff: 1

Table 2	Other's decision: X	Other's decision: Y
Your Decision: X	Your Payoff: 20 Other's Payoff: 20	Your Payoff: 0 Other's Payoff: **15**
Your Decision: Y	Your Payoff: **15** Other's Payoff: 0	Your Payoff: 1 Other's Payoff: 1

Note that if you both make the same choice (you both choose X or you both choose Y), the gains of the two tables are identical. The only difference corresponds to the gain you get if you choose X and the other Y and vice versa if you choose Y and the other X (in both tables only the bold number changes).

Which table?

In all rounds of each sequence ONLY ONE TYPE of Table will be used. The Table used in the sequence will be shown, at each round, before making the choice and will determine the earnings in that round based on the choices of the participants. To facilitate the recognition of the two different tables, Table 1 will be shown in black and Table 2 in red.

Each time a new sequence starts, the Table MAY change from the one used in the previous sequence (but it could also remain the same!). The Table can ONLY change at the beginning of a new sequence and will remain the same for all rounds of a sequence.

When does a sequence end?

Each sequence consists of at least one round. At the end of each round, a six-sided virtual die will be rolled. If the number resulting from the roll is equal to 1, 2, 3, 4 or 5, then the sequence will continue and a new round of the same sequence and therefore with the same Table will begin. If the number resulting from the launch is equal to 6, then the sequence will end and a new sequence will begin. The total number of sequences is indefinite and at a certain point, at the end of a sequence, you will be informed that the experiment is over. When starting a new sequence, the table used may or may not change.

To recap, each sequence consists of an indefinite number of rounds in which the Table used will always be the same for the duration of the sequence. The number of rounds depends on the virtual die roll: a sequence ends if the number 6 is rolled. Therefore, at each round, the probability that the sequence continues is equal to 5/6 and the probability that the sequence ends is equal to 1 / 6. In each round you will be grouped with another participant in a completely random and anonymous way.

Expectations at the start of a new sequence

In the first round of each sequence you will be asked "How many of the other 9 participants (excluding you) do you think will choose" X "in this round?". Under this question, in a blue box, you will have the opportunity to enter the number from 1 to 9 of participants that you think will choose the "X" option in that round. You will be asked to express this expectation only during the first round of each sequence.

Results and Earnings Table

At the end of each round you will see a "feedback" screen that will report your decision, your profit and the decision and earnings of the other participant paired with you. At the same time, you will be provided with a summary table of the earnings of your previous rounds.

Payments

Your profit in each round corresponds to a number: 0, 1, 15, 25 or 30. This number represents your profit in the round in HUNDREDS of Euros, so your possible earnings are: 0.00 €, 0.01 €, 0.15 €, 0.20 € or 0.25 €. At the end of the session you will be paid a total final earnings equal to the sum of your earnings obtained in the individual sessions and € 3 for your participation.

What happens now?

Now you will have to answer a few questions to check that everything is clear to you about the functioning of the experiment. As soon as all the participants have answered the questionnaire, the first round of the experiment will begin.

If you have any questions, write in the chat and one of the experimenters will answer you personally.

Chapter 4

Coordination, Cooperation, and Welfare in a Multiple Public Good experiment with heterogeneous agents

4.1 Introduction

Charitable and political giving is ubiquitous across socio economic groups, with people from all walks of life contributing to religious and educational institutions, community projects, international development efforts, political campaigns, and social movements. While many organizations rely on donations from across the income spectrum, a wealthy donor can have a disproportionate effect in determining the allocation of funding across projects, charities, candidates, or causes.

To explore the flow of donations in such an environment, we introduce donor heterogeneity into a threshold public goods game experiment, where a potentially diverse set of donors choose how to donate across a potentially diverse set of recipients (e.g., projects, charities, candidates, or causes). In a threshold public goods game, to be successful, an individual public good needs to attract total donations above a given threshold, which requires attracting contributions from multiple donors (Andreoni, 1998). Such an environment captures many of the nuances of fundraising environments, in which multiple recipients, including charities (Corazzini et al., 2019) or crowdfunding projects (Corazzini et al., 2015), vie for a limited pool of donor funding. In the current paper, we expand the earlier settings, which have focused on a homogeneous donor pool, to allow donors to differ in both their wealth and which public good (e.g., projects, charities, candidates, or causes) they would most like to succeed.

We show that the wealthiest donors tend to set the contribution agenda for all donors. They not only direct their own contributions to funding their favorite public goods, but their focus on these goods also attracts the contributions of other donors to the same opportunities. Less-wealthy donors tend to contribute to the options preferred by the wealthiest donor even if the less-wealthy would have preferred the group to collectively focus on different opportunities.

We refer to this result as the "Gates Effect" through which wealthy donors or special interests have a determinant influence over the philanthropic or political agenda. This influence goes beyond being able to provide greater financial support to the causes or candidates they prefer, but also stems from how they pull in contributions of others to their preferred recipients. In our environment, this is true even in the absence of seed money or matching grants, but rather it results because the preferences of the wealthy serve as a focal point enabling the broader donor base to coordinate their support on recipients where contributions are less-likely to be wasted.

The public good preferred by the wealthiest donor is more likely to succeed compared to any other good not only because the wealthy donor contributes more, but also because other donors also coordinate their contributions on the good (even if it isn't their own preferred option). On the surface, this result appears socially inequitable, favoring the wealthy at the expense of others. We show, however, that such outcomes can be Pareto improving across the entire donor pool. This happens when the wealthy donor takes on more of the funding burden when the focus is on their own preferred recipient. When the wealthiest donor contributes, in absolute terms, a greater share of the costs of their preferred good than the other subjects and their preferred public goods are successfully financed, all donors are better off.

These results provide insights into a variety of real world interactions. In a political campaign, less-wealthy party members may coordinate their support on the party candidate preferred by the wealthier members of the party. While this means the party puts forward a candidate that is more beneficial for wealthier party members, other party members may also end up being better off because the wealthier party members provide a more substantial share of funding. Similarly, in philanthropic giving, smaller donors may contribute to the projects or causes that are more likely to excite larger donors, with the expectation that such initiatives may be more likely to get off the ground or effect social change. Even though they would prefer the focus to be on a different opportunity, they recognize that their individual contribution will not unilaterally effect change unless coordinated with others. Furthermore, the focus on the causes or projects preferred by the wealthy leads to wealthy to contribute more.

The insights may extend to settings with institutional donors and stakeholders, such as foundations, NGOs, and government funders. In international development and global health, for example, the largest funders such as the Gates Foundations (or other large private foundations and western country donor organizations such as USAID) directly control which causes, projects, or approaches to support with their own funding. But, in doing so, they also indirectly steer the funds and efforts of other smaller foundations, organizations, or local governments who recognize that their own initiatives are more likely to succeed when they are aligned with the funding priorities of the larger donors. As McCoy and McGoey (2011) explain, "other donors look to the Gates Foundation in order to decide whether to fund a particular project or programme."[1] Such arguments are consistent with the results from our experiment; but, our results also show how a shift in focus to the priorities of the wealthiest stakeholders may be good for others as well if it leads the wealthiest to fund a greater share of the initiatives.

The key contributions of our paper are as follows. First, we are the first to consider a threshold public goods environment with multiple contribution options and donor heterogeneity (in wealth and preferences), exploring the role of donor heterogeneity through several experimental treatments. Second, we present experimental evidence

[1] See also Orbinski (2009); Rushton and Williams (2011); Faubion et al. (2011); Birn (2014) and Martens and Seitz (2015).

that the wealthiest donors have a determinant role on the contribution agenda, attracting the contributions of other donors to their own preferred cause. This is true even in an environment without seed funding, matching grants, or other financial mechanisms that wealthier donors or organizations often use to attract additional funding, but rather because the preferences of the wealthy help coordinate the contributions strategies of other donors. We refer to this newly identified phenomenon as the Gates Effect. Third, we show how the collective focus on the donation recipients preferred by the wealthiest donors can be Pareto improving, benefiting all donors because it leads the wealthiest to provide additional funding.

The paper is presented as follows. Section 2 provides an overview of the literature. Section 3 describes the theoretical framework and experimental design. Section 4 presents the results from the analysis. Section 5 concludes with a discussion of some of the real world environments into which our analysis provides insights.

4.2 Literature Review

Traditional public goods games involve groups of donors individually choosing how much of their endowments to contribute towards the funding of a public good, which provides benefits to all donors based on total funding and independent of any individual contribution. Threshold public good games extend the traditional public goods framework to introduce a minimum contribution threshold that must be reached before a public good provides benefits (e.g., whether the "save the clocktower" fund did collect enough contributions to effectively "save the clocktower"). This framework has been used to model charitable organizations or fundraising projects, as for these projects, to be realized, total funding must reach a minimum threshold (Andreoni, 1998).[2]

Our framework involves multiple public goods in the threshold public good environment, capturing settings in which multiple charities or crowdfunded projects vie for the same pool of donor funding. Such a framework was first considered in Corazzini et al. (2015) (CCV henceforth), which explores the coordination problems among

[2]See Andreoni (2006) for an overview of the literature on philanthropy and charitable fundraising.

donors in such a framework, showing how a greater number of charities or crowdfunded projects can discourage donations, and Corazzini et al. (2019) (CCR henceforth), which explores the role of donation intermediaries like the United Way in overcoming coordination issues between donors.[3] While both CCV and CCR consider environments with homogeneous donors, who have the same endowments and preferences across public goods. The current paper extends the multiple threshold public goods framework to consider donor heterogeneity, with differences in donor endowments and preferences over which good is best. This heterogeneity allows us to explore how wealth differences across donors affect the flow of contributions and may influence which public goods succeed.

Several experiments have analyzed the impact of donor heterogeneity in preferences or income in the provision of public goods. However, they have *never* done so in an environment with multiple threshold public goods. A few papers, including Marks and Croson (1999) and Rappoport and Suleiman (1993), have considered heterogeneity in an environment with a single threshold public good, where Marks and Croson (1999) considers heterogeneity in payoffs from the good and Rappoport and Suleiman (1993) considers heterogeneity in donor wealth. Notably, Rappoport and Suleiman (1993), find that the contributions of subjects of different wealth levels contribute a similar portion of their endowments to the good.[4] They also find that homogeneous groups are more likely to succeed in funding a good than heterogeneous groups. In contrast, we show that when there are multiple threshold public goods vying for funding, donor heterogeneity in wealth and preferences can help facilitate coordination and public good success and that wealthier donors tend to contribute a larger share of their income.

Cherry et al. (2005) explores the impact of endowment heterogeneity in a standard public good experiment, but they do not consider threshold public goods. They find that contribution levels are significantly lower in groups with heterogeneous endow-

[3]CCR finds that when the intermediary is formally committed to direct donations, the presence of an intermediary increases public good success. However, without this commitment, the presence of an intermediary has even a negative impact on contributions and coordination above the threshold.

[4]Also in Brekke et al. (2017) higher endowed subjects contribute more in absolute terms, but this difference disappear in relative terms.

ments rather than homogeneous endowments. Levati et al. (2007) study the effects of leadership on the private provision in a sequential public good when group members are heterogeneously endowed, and they show that the presence of a leader increases average contributions levels but less so than in the case of homogeneous endowments. Also, Bernard et al. (2012) study heterogeneity in endowments in a threshold public good setting; their findings show that heterogeneous groups are less successful than homogeneous groups in providing the public good, and heterogeneous groups are less stable. Chan et al. (1999) consider standard public goods setting with communication and find in contrast a positive effect of heterogeneity on aggregate contributions, but its effects interact unexpectedly with communication. They consider a non-linear public good experiment. In a no-communication environment, heterogeneity in two dimensions (income and preferences) increases contributions substantially, while heterogeneity in a single dimension (income or preferences) has little effect. In the communication environment, they find the reverse. So they conclude that heterogeneity increases voluntary contributions, but communication unexpectedly reverses the relative importance of single and double heterogeneity.

The differences among these findings could be partly explained by the differences in the details of the experimental designs across the literature. Our paper brings two sources of novelty; we explore the role of heterogeneity in the multiple threshold public goods setting, and we also study the interaction between both heterogeneity in income and preferences.

4.3 Experimental Design and Theoretical Framework

Our experimental design extends the original threshold public goods game with multiple viable alternatives (CCV). In a series of extensions, we introduce donor heterogeneity in endowments and preferences for different public goods. The experiment includes four distinct treatments using a between subject design. Our main experimental treatment is one in which donors differ in both their endowment and preferences, allowing for us to consider questions related to donor coordination on goods that are, for example, preferred by the wealthiest donors. We also run treatments involving endowment heterogeneity and preference heterogeneity alone and a treatment with homogeneous donors as points of comparison to understand how the distribution of donations across donors and recipients depends on both types of heterogeneity.

A total of 240 subjects participated in the experiment, with 60 individuals participating in each of the 4 treatments. During each treatment, subjects were divided into unchanging groups of 4 people, resulting in a total of 15 independent groups per treatment. Each group of 4 people interacted for 12 sequential periods, in each period playing a threshold public good game with each other. Between periods, participants received feedback about their group's contributions during the previous period.

At the beginning of each experiment, group members are assigned an endowment level, which represents their budget in each period. Total endowments across all individuals equal 220 token in each period of each treatment, but the distribution of these tokens across individuals depends on the treatment. In each period of each treatment, each subject simultaneously chooses how much of their individual endowment to contribute to each of eight available public goods. Any amount of their endowment that they do not contribute to a public good goes into a private account, which provides an individual payout of two points per token at the end of the experiment. Any amount contributed to a public good potentially provides a benefit to each group member, but only if total contributions to that public good reached the threshold of 132 tokens (60 percent of the total group allocation) in a given period. If the total number of tokens contributed by the group to a collective account is lower than 132, then the subjects do not receive any points from that account, and contributions to that account are forfeited. If the overall number of tokens contributed to a collective account is at least 132, each group member receives one point for every token contributed into that account plus an additional bonus. When we introduce preference heterogeneity, it will come through differences in the size of the individual bonus subjects receive with the success of different public goods.

4.3.1 Donor heterogeneity (main treatment)

The main treatment, $HetE\&HetP$, involves donor heterogeneity in both endowments and preferences over the public goods. At the beginning of the experiment, before the first period of interaction, participants are randomly assigned to one of four possible endowment levels (34, 48, 62, or 76), defining the endowment they receive in each of the 12 periods of play. Each group involves one subject assigned to each of the four endowment levels. The total group endowment is 220. The initial assignment remains unchanged throughout the 12 periods of the experiment. The endowment distribution used in the experiment presents a relatively large variance, with the highest endowment in the group more than doubling the one assigned to the poorest group member.

Additionally, at the beginning of the experiment, each of the four subjects is assigned a separate one of the eight available public goods to be their 'preferred' good throughout the experiment. When contributions to a subject's preferred public good reach its threshold in a period, that subject receives a bonus payment of 39 points, and the three other group members receive bonus payments of 27 points in addition to the uniform payout to all group members equal to one point per token contributed to that good's account in that period. If one of the four public goods preferred by none of the subjects is funded at or above its threshold, then each subject receives a uniform bonus of 20 points in that period, plus the payout of one point per token contributed to that good. The differences in bonus payments represent a relatively small-magnitude difference in preferences. If, for example, the public good preferred by one subject is funded at its threshold, that goodwill return a total payout of 171 points to one subject that prefers it and a payout of 159 points to each of the other group members. It should be clear that subjects prefer to coordinate their contributions on one public good and reach the threshold, even if coordination takes place on a public good preferred by one of the other subjects.

Each subject's endowment and preferred good is observable by other group members.

4.3.2 Homogeneous donor and one-dimensional heterogeneity treatments

In addition to the full-heterogeneity treatment described above, we conduct three other treatments.

In a homogeneous donor treatment, *homE&homP*, all four donors in each group have the same endowment (55 tokens) each period and share preferences over the public goods. In this treatment, four of the public goods are 'selected' goods, any of which will provide a uniform bonus of 30 points plus one point for each contributed token to each of the four group members in any period in which it reaches its threshold of 132 tokens. If one of the other four ('non-selected') goods has total contributions above its threshold, the bonus from that good is only 20 points combined with one point per contributed token. The bonuses are calibrated so that the total group bonus across all four groups members is the same as for the four preferred goods in the *hetE&hetP* treatment.

The homogeneity treatment is most similar to the baseline treatment in CCV, where homogeneous donors faced four public goods none of which stood out as strictly preferred for the group. In this environment, the multiplicity of reasonable donation options makes coordination among donors more difficult to achieve compared to the case of a single public good.[5] In the homogeneous treatment, four goods stand out as equally reasonable options.

Additionally, we run both *hetE&homP* and *homE&hetP* treatments, which represent environments in which only one type of donor heterogeneity is present. In *hetE&homP*, donors differ in their endowments in the same way they did in the fully heterogeneous treatment but have the same preferences in the way that they did in

[5] In contrast, CCV showed that the coordination problems that arise from the multiplicity of public goods is reduced if one of the goods stands out as the best available option for all donors. Such an alternative environment would leave little room for endowment or preference differences to improve coordination across goods.

the homogeneous donor treatment. In *homE&hetP*, donors have the same endowments as in the homogeneous donor treatment but differ in their preferences as in the heterogeneous donor treatment.

4.3.3 Procedures

The experiment was run in February 2021. In accordance with the lockdown restrictions in force to contrast the COVID-19 outbreak, all sessions were run online. In particular, in order to participate in the experiment, subjects were required to join a Zoom session from a computer with a well-functioning internet connection, webcam, microphone, and audio. They were also asked to connect from an isolated and quiet room and to remain seated throughout the experiment. At their arrival, subjects were initially moved to a virtual waiting room that guaranteed their anonymity. Subjects accessed the virtual welcome room one by one, keeping their microphone and webcam switched on. After ascertaining participants' identity and checking the quality of their digital infrastructure, experimenters disabled subjects' webcam and microphone and made their zoom profiles entirely anonymous by removing any possible distinctive element (such as pictures, colors, initials) and assigning a random identification number. Then, subjects were moved to the experimental room, and, in case of necessity, they could communicate through the zoom chat. In particular, the chat allowed subjects to send private messages to the experimenter only, being any further possibility to interact with the other participants disabled. At the beginning of the experiment, experimenters shared their video and read the instructions aloud (see the online Supplementary Material for the instructions in *HetE&HetP*). Before the first period started, subjects were asked to answer sample questions at their terminal. When necessary, answers to the questions were privately checked and explained through the chat. At any time during the experiment, subjects had the possibility to click a button and access a table summarizing the main instructions of the experiment.

At the beginning of each period, the computer showed each subject nine boxes, one for the private account and eight for the collective accounts. In order to avoid frame effects, the eight collective accounts were presented to subjects using neutral color names. Moreover, the order in which the collective accounts appeared on the screen was randomly determined by the computer for each subject. Finally, each of the four boxes of the collective accounts showed the threshold and the size of the corresponding bonus. Given the nine boxes, in each period, every subject chose how to allocate her endowment entirely over the alternative accounts.

In treatments with heterogeneity, the assignment of endowments and preferences was common knowledge. In particular, at the beginning of each session, subjects were randomly assigned one of four letters, either A, B, C, or D. In $HetE\&HomP$ and $HetE\&HetP$, the order of the letters matched the order of the endowments, with A and D being respectively associated with the lowest (34 tokens) and highest (76 tokens) endowments. To facilitate subjects' assimilation of the information, a summary table reporting, for each letter, the corresponding endowment and, in $HetE\&HetP$ and $HomE\&HetP$, the corresponding preferred collective account was included in the screen used by subjects to make their choices.

At the end of every period, each subject was informed about the number of tokens allocated by the group to (each of) the collective account(s), whether the corresponding threshold was reached, and any bonus paid. Additionally, following each period, subjects learned the number of points they received from each account and in total. At the end of the experiment, subjects were privately paid using a payment rate of one euro per 100 points.

On average, they earned 11.42 euros for sessions lasting about 90 min, including the time for identification, instructions, a post-experimental questionnaire. All payments were made through PayPal. Participants were drawn from the subject pool of the VERA-lab of the University of Venice, "Ca' Foscari" (Italy), including more than 2,500 subjects. Participants were mainly undergraduate students in Economics, Management, Language studies, Philosophy, and they were recruited using ORSEE Greiner (2015a). The experiment was computerized and executed online employing z-Tree Unleashed Duch et al. (2020b).

4.3.4 Theoretical Framework

We begin by considering a one period public goods contribution game with multiple threshold public goods. There are J agents, indexed $j \in \{1, ..., J\}$. At the beginning of the game, every agent simultaneously decides how much of her private endowment, $y_j > 0$, to contribute to each of N public goods. We denote with $c_{j,n} \geq 0$ the contribution made by agent j to public good n. Let $C_n \equiv \sum_j c_{j,n}$ and $c_j = \sum_{n=1}^{N} c_{j,n}$ represent the aggregate contributions to public good n and the total contributions made by agent j, respectively. The total contributions made by agent j cannot exceed her endowment, $c_j \in [0, y_j]$.

Function $B_{j,n}(C_n)$ determines the benefit each agent receives from public good n. The benefit depends on whether the overall amount contributed by the J agents reach a threshold level, τ. Specifically, for each good n,

$$B_{j,n}(C_n) = \begin{cases} 0 & \text{when } C_n < \tau \\ C_n + b_{j,n} & \text{when } C_n \geq \tau. \end{cases} \tag{4.1}$$

By the previous expression, if agents fail to reach the threshold level, then the public good does not return any benefit, and the contributions are lost. Instead, when the threshold is reached, the public good returns a benefit to player j that is increasing in total contributions, plus a bonus of $b_{j,n}$ the size of which depends on the agent's preferences for that good. Any unit of endowment not contributed to a public good gets directed to private consumption, where it returns a marginal benefit of two (implying a marginal per capita return to the public good is 1/2 that from private consumption). Therefore, player j earns total payoff:

$$u_j(c_j) = 2(y_j - \sum_{n=1}^{N} c_{j,n}) + \sum_{n=1}^{N} B(C_n) \tag{4.2}$$

Independently from the heterogeneity manipulations, parameters in our experiment are set to assure that group members can fund at most one public good at its threshold, that players are unable and unwilling to unilaterally fund a good at its threshold, and that players prefer to contribute to a public good only if they expect that others are also contributing to the same public good.

In all treatments, we set $J = 4$, $N = 8$ and $\tau = 132$. In the homogeneous donor treatment, *HomE&HomP*, agents are homogeneous in both endowments and preferences, such as $y_j = y = 55$ and $b_{j,n} = b_n$, with $b_n = 20$ for four of the goods and $b_n = 30$ for the other four goods. In the treatments with heterogeneity, $y_j \in \{34, 48, 62, 76\}$ and $b_{j,n} = 20$ for four of the goods, and $b_{j,n} =\in \{27, 39\}$ (with one player j having the higher bonus) for each of the other four goods.

As discussed in CCV, the baseline setting admits two types of equilibria. First, there exists an equilibrium in which agents make no contributions to any of the public goods. Second, for each of the public goods, there exist equilibria in which agents successfully fund a public good by contributing an amount to it equal to the threshold while providing no contributions to any other good. There are $N + 1$ symmetric-contribution equilibria: one in which $c_{n,j} = 0$ for all n and j, and one for each good n in which each player contributes $c_{n,j} = \tau/J = 33$ and $c_{m,j} = 0$ for all $m \neq n$. There are also many asymmetric equilibria in which players contribute unequal amounts to the same public good such that total contributions equal the threshold and contribute nothing to the other $N - 1$ goods. In each of these equilibria, $C_n = \tau$ for one $n \in N$, and $C_m = 0$, $\forall m \neq n$. The multiplicity of equilibrium introduces the potential for coordination problems among donors, who risk contributing to the "wrong" good than other donors when they do contribute, contributing when others choose not to; in both cases, effectively wasting their contribution.

CCV shows how donors benefit from coordinating their donations on a common good to fund it at its threshold, but the multiplicity of public goods adds to the coordination problem between donors, makes coordination (and the success of any public good) less likely, and discourages contributions to all goods. CCV also shows, however, how a focal point drawing the group's attention to one of the public goods can help overcome the coordination problem, encourage donations, and increase the probability of a public good successfully reaching its funding threshold.[6]

[6] CCV shows how such a focal point arises when one public good is preferred by all donors (or one good is singled out as "recommended").

Building on the insights from CCV and the theoretical framework, we present three hypotheses to help guide the experimental analysis.

Hypothesis 1 ***Coordination on more efficient public goods:*** *In all treatments, whenever agents succeed in reaching the threshold of a public good, they coordinate their contributions on one of the "selected" or "preferred" alternatives.*

Even in treatments in which no good stands out as unique, four of the eight goods have strictly higher bonuses compared to the other four goods. Hypothesis 1 is based on the expectation that agents who attempt coordination have no reason to expect coordination to occur on payoff-dominated equilibria, and will therefore work towards coordination on one of the goods with higher bonuses. This first hypothesis is consistent with the findings in CCV, where agents that do contribute direct their contributions to more-efficient (e.g., higher-bonus) goods, effectively ignoring relatively less-efficient (e.g., lower-bonus) contributions options.

Hypothesis 2 ***Endowment heterogeneity:*** *In treatments with heterogeneous endowments, the agent with the highest endowment makes larger contributions in the group to reach the threshold of a public good.*

The second hypothesis is based on the expectation that groups that successfully fund a public good will expect agents with a higher endowment to contribute a higher share of the public good funding. Whether or not such a hypothesis holds is far from certain, as the theory permits both symmetric-contribution and asymmetric-contribution equilibria, and while wealthier donors can afford to contribute more than other donors, they do not necessarily receive higher payouts from the public good (depending on treatment and which good is funded). Whether Hypothesis 2 holds is an empirical, rather than theoretical, question.

Hypothesis 3 ***Preference heterogeneity:*** *When agents have homogeneous endowments, then introducing preference heterogeneity reduces group coordination and payoffs. When agents have heterogeneous endowments, then introducing preference heterogeneity increases coordination.*

The third hypothesis builds on the idea that added complexity to the environment makes coordination more difficult, while introducing focal points can facilitate donor coordination.

The homogeneous donor treatment (*homE&homP*) in the current paper is a similar setting as the baseline treatment in CCV, where there is no natural focal point to facilitate donor coordination. As such, we expect coordination to be relatively difficult, and contributions and public good success rates (the rate at which they achieve their funding thresholds) to be relatively low compared to any environment in which a focal point exists. Extending the homogeneous donor framework to include either heterogeneity in preferences or heterogeneity in endowments will do little to introduce a viable focal point into the environment and therefore will do little to overcome the donor coordination problems. If donors differ in endowments but not preferences, there will still be four equally-preferred public goods, none of which stand out more than any other to the donors. If, alternatively, donors differ in their preferences but are otherwise homogeneous then there is no obvious reason for donors to focus the good preferred by one donor versus the good preferred by another donor. In these treatments, no clear focal point exists, and there is no reason to expect that contributions or public good success will be easier in such environments than in the homogeneous donor environment. If anything, the increased complexity of such environments may discourage attempts to coordinate on an equilibrium in which one of the public goods receives funding.

When donors differ in terms of both their endowments and their preferences, as is the case in our heterogeneous donor treatment *hetE&hetP*, potential focal points emerge. When donors have different preferred goods and can be ranked in terms of wealth, one good will be distinguishable as the public good preferred by the highest-wealth individual and another will be distinguishable as the public good preferred by the lowest-wealth individual. Either of these goods may serve as a viable focal point and facilitate donor coordination.

Hypothesis 4 ***Endowment and preference heterogeneity:*** *With heterogeneous* *endowments and preferences, groups tend to coordinate their contributions on the public good that is preferred by the agent with the highest endowment. When they successfully fund that good, the wealthiest agent contributes a disproportionate share of the good's funding.*

The fourth hypothesis is based on the expectation that groups will focus on the good preferred by the wealthiest agent. Even though the good preferred by the least-wealthy agent (or the second lowest-wealth agent, and so on) could theoretically serve focal points, the good preferred by the agent who can afford to contribute the most is more intuitive, as the pivotality of the contribution of the wealthiest agent in the group is likely to enhance salience on her preferred public good. Indeed, by the previous discussion, from the perspective of the other three group members, coordinating on the public good preferred by the wealthiest agent is not only made easier by the larger amount of group resources in her hand, but is also convenient as, without her contribution, there is very little chance to successfully fund any other public good and obtain the corresponding returns. The notion that the wealthiest agent contributes disproportionately to that good follows from the arguments behind Hypothesis 2.

While the theoretical discussion focuses on a single period interaction, subjects in our experiment have repeated interactions over a finite number of periods with the same group members. These experimental features substantially increase the set of subgame-perfect equilibria. Indeed, in all periods but the last, a range of contribution profiles in which group members contribute strictly more than the threshold is consistent with equilibrium because subgame perfect strategies can credibly threaten to revert to no contributions in future if anyone deviates from contributing in an earlier period. In the last period, however, the equilibrium profiles of contributions coincide with those of the one-shot game described above. Despite the number of equilibrium profiles, the considerations about payoff-dominance made above to compare the zero-contribution equilibrium with any of the positive contribution outcomes can be easily extended to the dynamic setting, and the general hypotheses are unchanged.

4.4 Experimental Results

We mainly aimed at assessing how heterogeneity in both endowment and preference and their interaction affects coordination, i.e., the probability that a group reaches the threshold, contribution, and profit. Thus, we start by looking at differences in coordination, absolute contributions, and profits across treatments. Then, by focusing on the treatments with endowment heterogeneity, $HetE\&HomP$ and $HetE\&HetP$, we study how the presence of endowment heterogeneity affects the ability of subjects to coordinate and how this interacts with heterogeneity in preference, introducing what we defined as "Gates effect". The non-parametric tests discussed in the analysis are based on 12 independent observations at the group level per treatment. Similarly, in order to account for potential dependence across periods, the estimated coefficients in the parametric regressions are based on standard errors clustered at the group level.

4.4.1 Coordination

The first result concerning coordination is related to hypothesis 1: no group coordinated in a non-selected public good in any treatment.

Result 1 In all treatments, whenever agents succeed in reaching the threshold of a public good, they always coordinate their contributions on one of the "selected" and more efficient alternatives.

Figure 1 shows the mean coordination rate (i.e. the total number of groups that achieved coordination out of the total number of groups) for each period and for each treatment. The red line represents the treatment $HomE\&HetP$, that is the treatment in which we would expect subjects to have more problems coordinating as they have different preferences on the selected public goods but the same endowment. From the figure, it can be seen that, in fact, $HomE\&HetP$ is the treatment in which the subjects take the most periods to learn to co-ordinate and is almost always below the other three.

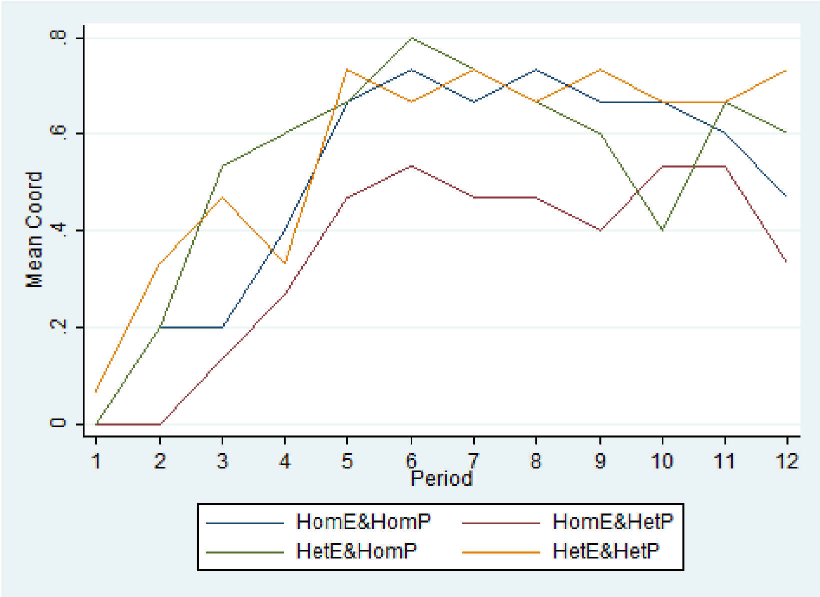


Figure 4.1: Coordination by Treatment and Period.

Over all periods, the average rate of coordination is 0.500 in $HomE\&HomP$, 0.344 in $HomE\&HetP$, 0.539 in $HetE\&HomP$ and 0.567 in $HetE\&HetP$. The ability of subjects to reach the threshold is formally analyzed in columns (1) and (2) of Table 4.1 that reports results of the marginal effects from a probit regression with standard error clustered at group level. The dependent variable is *coord*, a dummy that assumes value 1 if the group reaches the thershold and 0 otherwise, while $HomE\&HetP$, $HetE\&HomP$ and $HetE\&HetP$ are treatment dummies.

Column (1) compares the ability to coordinate in $HomE\&HomP$ with those observed in the other three treatments $HomE\&HetP$, $HetE\&HomP$ and $HetE\&HetP$ and shows that there are no significant differences. However, according to linear combination of Wald tests on the estimates above, we can reject the hypothesis of equality of coordination between $HomE\&HetP$ and $HetE\&HetP$ ($\chi^2(1) = 4.29, p = 0.038$)[7]. Also, no significant difference is detected by comparing coordination between $HomE\&HetP$ and $HetE\&HomP$ ($\chi^2(1) = 3.40, p = 0.065$) and between $HetE\&HomP$ and $HetE\&HetP$ ($\chi^2(1) = 0.08, p = 0.776$). Column (2) shows how results change when we control for *trend*, the linear time trend that starts from 0. The coefficient

[7]According to a non-parametric two-side Mann-Whitney rank-sum test, this difference is still slightly significant ($z = -1.923, p = 0.054$).

of *trend* is positive and highly significant, suggesting that subjects' ability to coordinate and reach the threshold increases as the periods pass. The inclusion of *trend* does not significantly change the results of column (1); according to a Wald test, the difference in coordination between $HomE\&HetP$ and $HetE\&HetP$ is still significant ($\chi^2(1) = 4.40, p = 0.036$) and no significant difference is detected by comparing coordination between any other couple of treatments.

Result 2 Overall, the only difference between treatments in coordination is between $HomE\&HetP$ and $HetE\&HetP$. This result partly rejects hypothesis 2; it is not true that introducing only heterogeneity in preferences drastically reduces coordination.

4.4.2 Overall contribution

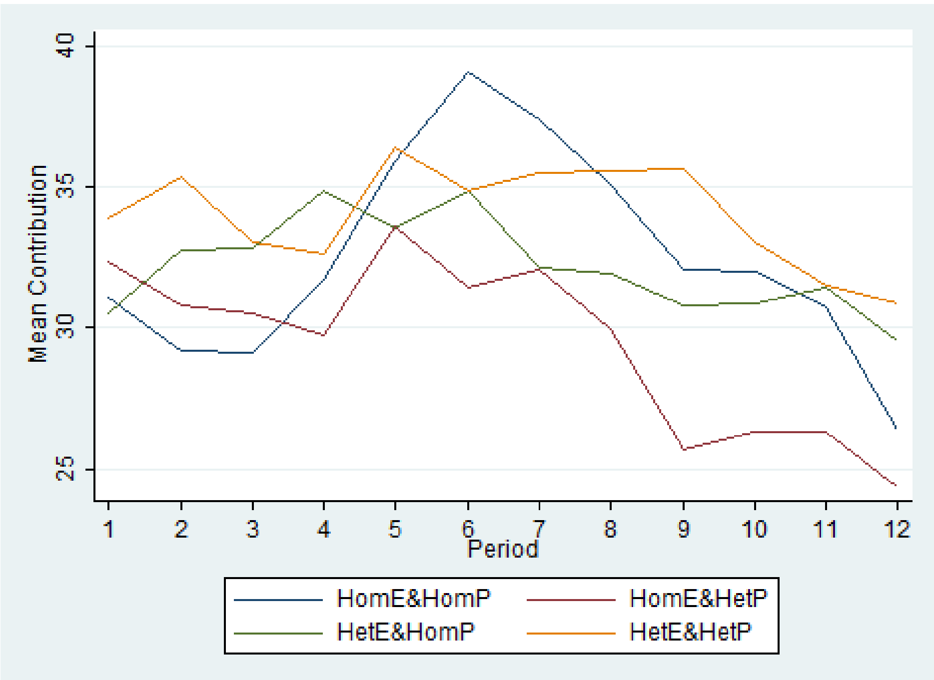


Figure 4.2: Mean Contribution by Treatment and Period.

Averaging over all periods, subjects contribute 32.474 tokens in $HomE\&HomP$, 29.435 tokens in $HomE\&HetP$, 32.169 tokens in $HetE\&HomP$ and 34.022 tokens in $HetE\&HetP$. Figure 4.2, reports the mean contribution, in absolute value, by treatments and periods. In every period, mean contributions in $HomE\&HetP$ are lower than in $HetE\&HetP$.

Table 4.1: Coordination, contribution and profits in *HomE&HomP*, *HomE&HetP*, *HetE&HomP* and *HetE&HetP*: parametric results.

VARIABLES	(1) coord	(2) coord	(3) contribution_total	(4) contribution_total	(5) Profit	(6) Profit	(7) Profit
HomE&HetP	-0.157	-0.169	-3.039	-1.559	-24.928*	-30.515***	-30.515***
	(0.104)	(0.110)	(3.572)	(2.860)	(13.810)	(9.484)	(9.484)
HetE&HomP	0.039	0.042	-0.304	-0.642	6.836	3.572	3.572
	(0.097)	(0.105)	(3.572)	(2.857)	(13.810)	(10.719)	(10.719)
HetE&HetP	0.067	0.074	1.549	0.858	9.914	4.890	4.890
	(0.099)	(0.107)	(3.572)	(2.857)	(13.810)	(11.316)	(11.316)
hcgroup						55.969***	55.969***
						(7.942)	(7.942)
$hc * HomE\&HetP$						19.968	19.968
						(12.414)	(12.414)
$hc * HetE\&HomP$						11.660	11.660
						(12.632)	(12.632)
$hc * HetE\&HetP$						7.536	7.536
						(13.023)	(13.023)
lagcoord				11.875***			
				(0.745)			
trend		0.046***		-1.004***			8.383***
		(0.008)		(0.098)			(0.377)
Constant			32.474***	32.651***	140.642***	107.060***	60.956***
			(2.526)	(2.095)	(9.765)	(7.003)	(7.303)
Loglikelihood	-1952.235	-1819.2331	-12303.933	-11110.382	-16661.266	-16622.666	-16395.578
Waldchi2	4.900	39.910	1.71	274.830	7.86	185.700	681.290
Prob>chi2	0.1794	0.000	0.634	0.000	0.049	0.000	0.000
Observations	2,880	2,880	2,880	2,640	2,880	2,880	2,880
Numberofgroups			60	60	60	60	60

Notes. Columns (1) and (2) report results of the marginal effects from a probit regression with standard error clustered at the group level. The dependent variable is *coord*, a dummy that assumes value 1 if the group reaches the threshold and 0 otherwise, *HomE&HetP*, *HetE&HomP* and *HetE&HetP* are treatment dummies. Columns (3), (4), (5), (6), and (7) report coefficient estimates (standard errors in parentheses) from two-way linear random effects models accounting for both potential individual dependency over periods and dependency within the group. *hcgroup* is a dummy that assumes value 1 if the subject belongs to a HC-group and 0 otherwise; $hc * HomE\&HetP$, $hc * HetE\&HomP$ and $hc * HetE\&HetP$ are interactions between treatment dummies and *hcgroup*. *lagcoord* is a dummy that assumes value 1 if the subject's group reached the threshold on one public good in the previous period; *trend* is a linear time trend that starts from 0; Significance levels are denoted as follows: * $p < 0.1$, **$p < 0.05$, and *** $p < 0.01$.

In all treatments, contributions tend to decline over periods, with this effect being particularly relevant in $HomE\&HetP$.

These preliminary observations are formally analyzed in columns (3) and (4) of Table 4.1 that reports result from parametric, random effects panel regressions. Column (3) compares contributions in $HomE\&HomP$ with those observed in the other three treatments $HomE\&HetP$, $HomE\&HetP$ and $HomE\&HetP$ and shows that there are no significant differences. According to a linear combination of Wald tests on the estimates above, we can not reject the hypothesis of equality of contributions even between $HomE\&HetP$ and $HetE\&HomP$ ($\chi^2(1) = 0.59, p = 0.444$), between $HomE\&HetP$ and $HetE\&HetP$ ($\chi^2(1) = 1.65, p = 0.199$) and between $HetE\&HomP$ and $HetE\&HetP$ ($\chi^2(1) = 0.27, p = 0.604$). Column (4) shows how results change when we control for the ability of the group to reach the threshold in the previous period. The coefficient of *lagcoord* is positive and highly significant, suggesting that contributions increase when the group successfully reached the threshold in the previous period. The inclusion of *lagcoord* does not change the results of column (3) significantly, and the differences between the treatments are still not significant.

Result 3 There are no significant differences in contribution between treatments.

4.4.3 Profit

Figure 4.3 plots the mean profits by treatment and period. Averaging over all periods, subjects earn 140.642 tokens in $HomE\&HomP$, 115.714 tokens in $HomE\&HetP$, 147.477 tokens in $HetE\&HomP$ and 150.556 tokens in $HetE\&HetP$. In every period, mean profits in $HomE\&HetP$ are lower than in $HetE\&HetP$.

Profits between treatments are formally analyzed in columns (5), (6), and (7) of Table 4.1 that report results from parametric, random effects panel regressions. Column (5) compares profits in $HomE\&HomP$ with those observed in the other three treatments $HomE\&HetP$, $HomE\&HetP$ and $HomE\&HetP$. The negative and significant ($p < 0.10$) coefficient of the treatment dummy $HomE\&HetP$ indicates that subjects in

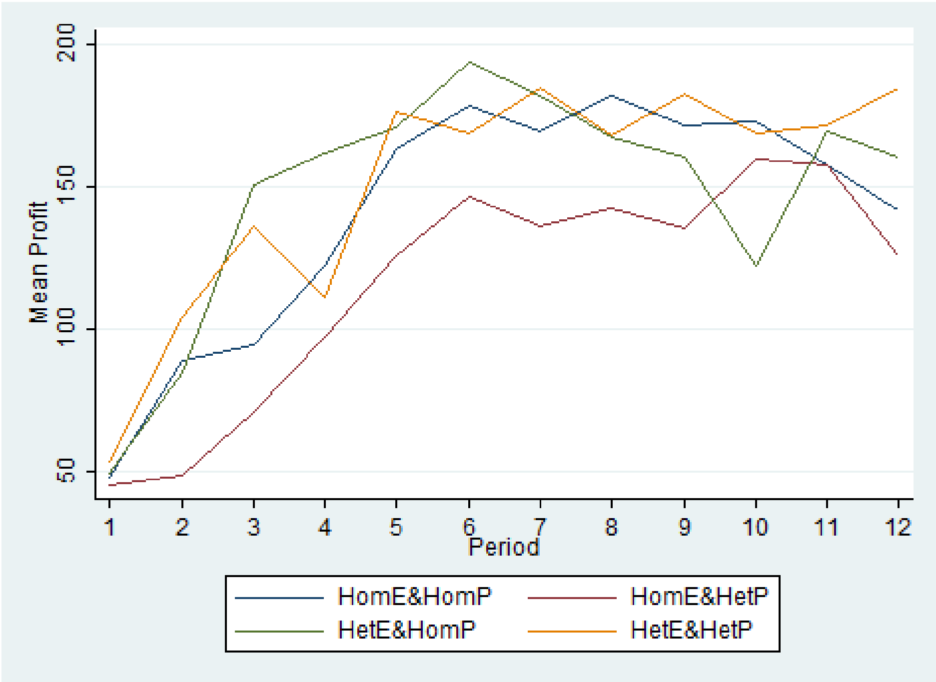


Figure 4.3: Profits by Treatment and Period.

HomE&HetP earn less than in *HomE&HomP*, while there are no significant differences between *HomE&HomP* and *HetE&HetP* and between *HomE&HomP* and *HetE&HetP*. Moreover, according to a linear combination Wald tests performed on the above estimates, subjects in *HomE&HetP* earn also less than subjects in *HetE&HomP*[8] ($\chi^2(1) = 5.29, p = 0.021$) and less than subjects in *HetE&HetP*[9] ($\chi^2(1) = 6.37, p = 0.012$) while there are no significant differences between *HetE&HomP* and *HetE&HetP* ($\chi^2(1) = 0.05, p = 0.824$).

We classify each group of each treatment in two classes: Low Coordination (LC) or High Coordination (HC) groups. We consider the median number of times that a group was able to coordinate within each treatment as the threshold level to distinguish between the two groups. This value is the same, and it is equal to 7 for the treatments *HetE&HetP*, *HetE&HomP* and *HomE&HomP* and equal to 4 for the treatment *HomE&HetP*.[10] The independent variable *hcgroup* is a dummy variable

[8] According to a non-parametric two-side Mann-Whitney rank-sum test, both these differences are still slightly significant (($z = -1.722, p = 0.085$ and $z = 1.846, p = 0.065$).

[9] According to a non-parametric two-side Mann-Whitney rank-sum test, this difference is still significant ($z = 2.219, p = 0.027$).

[10] As we would expect, the median coordination is lower in the *HomE&HetP* treatment than in the other treatments. However, as analysed in section 4.1, the only significant difference is between *HomE&HetP* and *HetE&HetP*.

that is equal to 1 for the groups that belong to the HC-groups and equal to 0 for the grous that belong to the LC-groups. The independent variables *hc*HomE&HetP*, *hc*HetE&HomP* and *hc*HetE&HetP* are interactions between the dummy *hcgroup* and the treatment dummies *HomE&HetP*, *HetE&HomP* and *HetE&HetP*.

Column (6) extends the regression in column (5) and controls for *hcgroup* and the interactions between *hcgroup* and the treatment dummies. According to a Wald test, in each of the four treatments HC groups earn more than LC groups; *HomE&HomP* ($\chi^2(1) = 49.67, p = 0.000$), *HomE&HetP* ($\chi^2(1) = 53.99, p = 0.000$), *HetE&HomP* ($\chi^2(1) = 41.38, p = 0.000$) and *HetE&HetP* ($\chi^2(1) = 33.85, p = 0.000$)[11]. The negative and significant ($p < 0.01$) coefficient of *HomE&HetP* indicates that subjects in the LC groups in the treatment *HomE&HomP* earn more than subjects in the LC groups in *HomE&HetP*. According to a linear combination of Wald tests on the estimates, also the subjects in the treatments *HetE&HomP* ($\chi^2(1) = 10.00, p = 0.002$) and *HetE&HetP* ($\chi^2(1) = 9.69, p = 0.002$) earn more than subjects in the LC groups in *HomE&HetP*[12]. No difference is detected by comparing *HetE&HomP* and *HetE&HetP* ($\chi^2(1) = 0.01, p = 0.913$). Column (7) considers the same regression and controls of column (6) but also adds the *trend* variable. The positive and significant coefficient ($p < 0.01$) of the *trend* coefficient indicates that subjects' earnings increase as the periods pass. The inclusion of *trend* does not change significantly the results of column (6).

Result 4 In *HomE&HetP* subjects earn less than in any other treatment. In each treatment subjects in HC-groups earn more than subjects in LC-groups. Subjects in the LC-groups of *HomE&HetP* earn less than subjects in the LC-groups of all the other treatments.

[11]According to a non-parametric two-side Mann-Whitney rank-sum test, this difference is still significant; for each treatment $p = 0.01$.

[12]According to a non-parametric two-side Mann-Whitney rank-sum test, the differences between *HomE&HetP* and *HetE&HetP* ($z = 2.714, p = 0.07$) and between *HomE&HetP* and *HetE&HomP* ($z = 2.747, p = 0.06$) are still significant, the difference between *HomE&HetP* and *HomE&HomP* is not significant ($z = -1.286, p = 0.199$).

The fact that, in general, subjects gained less in the $HomE\&HetP$ treatment than in the other treatments is attributable to the fact that in this treatment, characterized by agents with the same endowment but complicated by heterogeneous preferences; subjects had to waste more tokens in an attempt to signal to the group where to coordinate successfully. The fact that, moreover, LC-groups in $HomE\&HetP$ gained less than LC-groups in all other treatments is consistent with this dynamic; in the groups that struggled most to coordinate, they struggled even more in the $HomE\&HetP$ treatment than in the others.

4.4.4 Gates' effect

So far in the analysis, we have focused our attention on differences between treatments in the main variables of interest to us: coordination, contribution, and profit. We did not find many differences, which induces us to conclude that heterogeneity, both in earnings and preferences and their interactions, does not significantly impact overall, on the ability of groups to contribute and coordinate; some larger differences were found only in profits.

This second part of the analysis concentrates on the effects that the heterogeneity introduced in our experiment has within treatments with endowment heterogeneity and the consequences in terms of coordination, contribution and profit, distinguishing the analysis also between LC-groups and HC-groups.

Rich subjects contribute more

Figure 4.4 and 4.5 show the mean contribution in absolute terms by subject in the two treatments with income heterogeneity; $HetE\&HomP$ and $HetE\&HetP$.

It is possible to notice that rich subjects (C and D, green and yellow line respectively) contribute more than poor subjects (A and B, blue and red line respectively). This difference disappears when we evaluate contributions in relative terms (see Figure 4.8 and 4.11 in the Appendix); subjects contribute in proportion to their endowments.

In the Appendix, we also provide, in Figure 4.6 and 4.7 the mean contribution, in absolute terms, in the treatments with homogeneous income $HomE\&HomP$ and $HomE\&HetP$, where we do not notice systematic differences in how different subjects contribute in absolute terms.

These preliminary observations are confirmed in Table 4.2, that reports results from parametric, random effects panel regressions. In each column of Table 4.2 we perform, for each of the four treatments, the same regression in which we compare contributions of player A with the contributions of the other three players; B, C, and D. The dependent variable is the total contribution and B, C and D are subject dummies for players B, C and D.

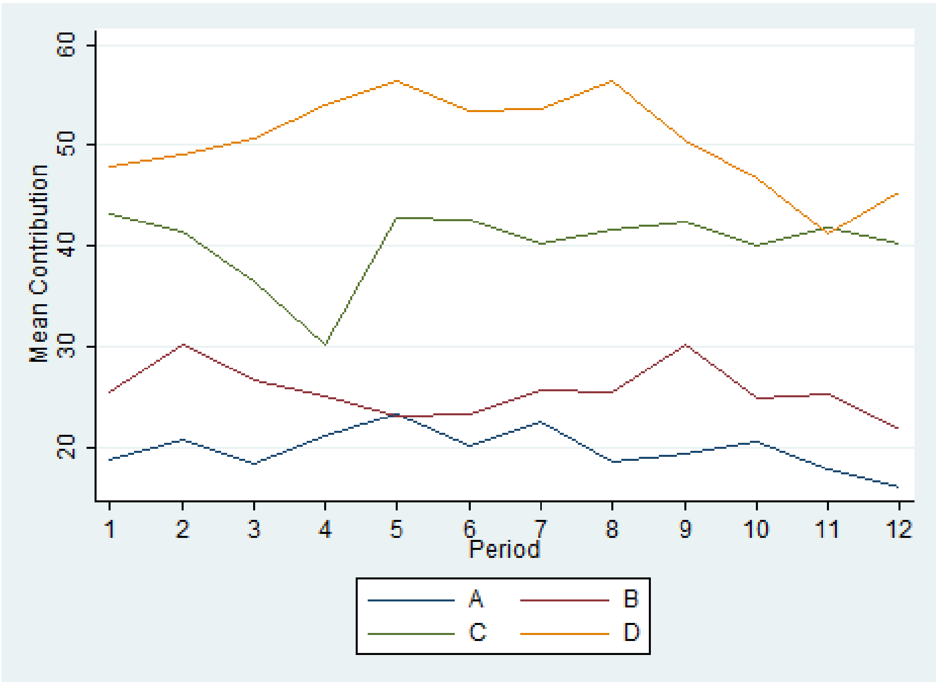


Figure 4.4: Contributions by Subject and Period in $HetE\&HetP$.

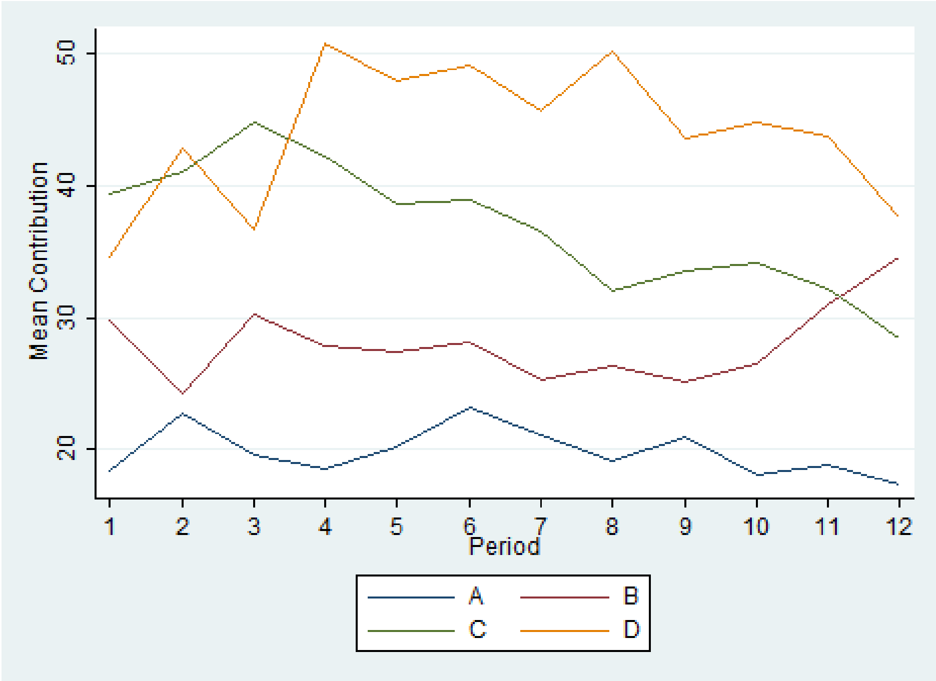


Figure 4.5: Contributions by Subject and Period in *HetE&HomP*.

In treatments with income heterogeneity (*HetE&HomP* and *HetE&HetP*), the positive and significant ($p < 0.01$) coefficients of the player dummies C and D indicates that rich players C and D contribute significantly more than player A. According to a linear combination Wald tests on the estimates above, in the treatment *HetE&HetP*, player C contributes more than player B ($\chi^2(1) = 12.71, p = 0.000$) while player D contributes more than player B ($\chi^2(1) = 36.37, p = 0.000$) and also more than player C ($\chi^2(1) = 6.08, p = 0.014$)[13]. In the treatment *HetE&HomP* player C contributes slightly more than player B ($\chi^2(1) = 3.31, p = 0.069$), player D contributes more than player B ($\chi^2(1) = 10.82, p = 0.001$) while we can not reject the hypothesis of equality of contributions between players D and C ($\chi^2(1) = 2.16, p = 0.141$)[14].

In the column *HomE&HomP* the negative and significant coefficients ($p < 0.05$) in Table 4.2 indicate that players B and C contribute less than player A but, according to a linear combination of Wald tests on the estimates, we can not reject the hypoth-

[13]According to the non-parametric Wilcoxon signed-rank test the differences between players D and A ($p = 0.001$), C and A ($p = 0.001$), C and B ($p = 0.006$) and also the differences between D and B ($p = 0.001$) and D and C($p = 0.036$) are all still significant.

[14]According to the non-parametric Wilcoxon signed-rank test the differences between players D and A ($p = 0.003$) D and B ($p = 0.023$) and C and A ($p = 0.005$) are still significant ($p = 0.023$), but between C and B it is not ($p = 0.173$).

esis of equality of contributions between player B and C ($\chi^2(1) = 0.04, p = 0.838$), B and D ($\chi^2(1) = 1.51, p = 0.219$) and C and D ($\chi^2(1) = 1.05, p = 0.305$). It is true that player A in the *HomE&HomP* treatment contributes more than B and C but not more than D. Furthermore, there are no systematic differences among the other players and the size effect of the differences is much smaller than in treatments with endowment heterogeneity. This leads to the conclusion that the higher contribution of A is due to random factors. In column *HomE&HetP* there are no significant differences in the coefficients and, according to a linear combination of Wald tests on the estimates, we can not reject the hypothesis of equality of contributions between player B and C ($\chi^2(1) = 0.26, p = 0.608$), B and D ($\chi^2(1) = 0.00, p = 0.984$) and C and D ($\chi^2(1) = 0.28, p = 0.594$).

Result 5 In treatments with homogeneous endowments (*HomE&HomP* and *HomE&HetP*) group members contribute the same amount to public goods. Instead, in treatments with heterogeneous endowments (*HetE&HetP* and *HetE&HomP*), the wealthiest agent makes, in absolute terms, substantially higher contributions.

Table 4.2: Contribution in *HomE&HomP* and *HetE&HetP*: parametric results.

contribution_total	(*HomE&HomP*) (1)	(*HomE&HetP*) (2)	(*HetE&HomP*) (3)	(*HetE&HetP*) (4)
B	-9.106**	0.089	8.194*	5.778
	(3.912)	(4.221)	(4.847)	(4.120)
C	-8.306**	-2.078	17.011***	20.467***
	(3.912)	(4.221)	(4.847)	(4.120)
D	-4.294	0.172	24.139***	30.622***
	(3.912)	(4.221)	(4.847)	(4.120)
Constant	37.900***	29.889***	19.833***	19.806***
	(3.260)	(3.912)	(3.758)	(3.613)
Log likelihood	-3047.931	-3084.697	-3064.208	-3063.017
Wald chi2	6.87	0.40	28.13	68.53
Prob>chi2	0.0762	0.9410	0.000	0.000
Observations	720	720	720	720
Number of groups	15	15	15	15

Notes. This table reports coefficient estimates (standard errors in parentheses) from two-way linear random effects models accounting for both potential individual dependency over periods and dependency within the group. *B*, *C* and *D* are subjects dummies; Significance levels are denoted as follows: * $p < 0.1$, **$p < 0.05$, and *** $p < 0.01$.

Coordination Device

Focusing on the treatment *HetE&HetP*, in which the two sources of heterogeneity interact, we notice that, when subjects are able to coordinate, they coordinate 96% of the times on the public good preferred by the richest subject D (see Table 3).

	HetE&HetP	*HomE&HetP*
coord_pgA	0.029	0.468
coord_pgB	0.000	0.435
coord_pgC	0.010	0.000
coord_pgD	**0.961**	0.097

Table 4.3: This table shows the proportion of times the groups coordinated on the public good preferred by A (*coord_pgA*), B (*coord_pgB*), C (*coord_pgC*), or D (*coord_pgD*), conditional on the fact that they coordinated, on treatments *HetE&HetP* and *HomE&HetP*.

The percentage of times that subjects coordinate in $HetE\&HetP$ on the public good preferred by the richest player D (96%) is statistically higher, according to a proportion test ($p = 0.000$), with respect to any other percentage of coordination reached in any other preferred public good in the treatments with heterogeneity in preferences.

Moreover, according to the non-parametric Wilcoxon signed-rank test, in the treatment $HetE\&HetP$, subjects A, B, and C contributes significantly more to the public good preferred by the subject D than what they contribute to their own preferred public good ($p < 0.001$).

From Table **??** it can be noted that in $HomE\&HetP$ the subjects, when they coordinate, do so with similar intensity on the public goods preferred by A and B, therefore, most likely using the letter as a focal point. The association of colors to the players is the same in the different treatments. This aspect confirms that the outcome of coordination in $HetE\&HetP$ on the richest's preferred good is entirely independent of both the colors associated with D's preferred good and the letter D itself.

Result 6 In $HetE\&HetP$, the public good preferred by the richest subject acts as a coordination device; subjects coordinate 96% of times on the public good preferred by the richest subject D, and each subject contributes more to the public good preferred by the richest subject rather than the good preferred by herself.

4.4.5 Welfare

Pareto improvement and welfare implications

In table 4.4 the first two columns refer to the treatment $HetE\&HetP$, the last two columns to the treatment $HetE\&HomP$. The dependent variable is *Profit*, the how much subjects earn in each period, and all the four regressions compare the profits of subject A with the profits of the other subjects.

In all the regressions, the dummies *B*, *C*, and *D* are subject dummies. The independent variable *hcgroup* is a dummy variable that is equal to 1 for the HC groups and equal to 0 for the LC groups and *B*hcgroup*, *C*hcgroup* and *D*hcgroup* are interactions between *hcgroup* and the subject dummies.

Columns (2) and (4) further explore the differences between LC-groups and HC-groups in the treatments with income heterogeneity.

HetE&HetP

The positive and significant ($p < 0.01$) coefficient of the dummy D in column (2) indicates that in the treatment *HetE&HetP*, in the LC-groups, the player D earn more than A. According to linear combinations of Wald tests on the estimates, in the LC-groups of *HetE&HetP* player D earns more than B ($\chi^2(1) = 10.46, p = 0.001$) and C ($\chi^2(1) = 5.16, p = 0.023$), while there is no significant difference between B and C ($\chi^2(1) = 0.93, p = 0.336$).

According to linear combinations of Wald tests on the estimates, each player in the HC-groups of *HetE&HetP* earns significantly more than what she would have earned by adopting a 0-contribution strategy in all periods; A ($\chi^2(1) = 116.73, p = 0.000$), B ($\chi^2(1) = 98.51, p = 0.000$), C ($\chi^2(1) = 33.94, p = 0.000$) and D ($\chi^2(1) = 10.89, p = 0.001$)[15].

Profits in HC-groups are also significantly higher than profits in LC-groups for each type of player; A ($\chi^2(1) = 28.43, p = 0.000$), B ($\chi^2(1) = 31.68, p = 0.000$), C ($\chi^2(1) = 17.39, p = 0.000$), D ($\chi^2(1) = 5.94, p = 0.015$) [16].

Finally, it is interesting that in the HC-groups of *HetE&HetP* the differences between profits of the different players disappear; in particular player D does not earn more than player A ($\chi^2(1) = 3.53, p = 0.060$), B ($\chi^2(1) = 0.11, p = 0.741$) or C ($\chi^2(1) = 0.30, p = 0.582$)[17].

[15]This result is confirmed by a non-parametric Wilcoxon signed-rank test, for each type of player ($p < 0.008$).

[16]This result is confirmed by a non-parametric two-side Mann-Whitney rank-sum test, for each type of player ($p < 0.010$).

[17]According to the non-parametric Wilcoxon signed-rank test the differences between players D and A ($p = 0.086$) D and B ($p = 0.553$) and D and A ($p = 0.005$) and are still significant ($p = 0.023$), but between C and B it is not ($p = 0.173$).

Table 4.4: This Table reports coefficient estimates for the treatments *HetE&HetP* and *HetE&HomP* (standard errors in parenthesis) from two-way linear random effects model accounting for both potential individual dependency over periods and dependency within group.

	HetE&HetP		*HetE&HomP*	
Profit	(1)	(2)	(3)	(4)
B	16.244**	6.005	11.611	1.831
	(8.087)	(13.332)	(9.695)	(13.091)
C	14.933*	19.405	21.978**	10.386
	(8.087)	(13.332)	(9.695)	(13.091)
D	29.089***	51.038***	35.722***	54.525***
	(8.087)	(13.332)	(9.695)	(13.091)
hcgroup		69.779***		61.976***
		(13.087)		(11.713)
*B*hcgroup*		15.359		16.301
		(15.954)		(16.191)
*C*hcgroup*		-6.708		19.319
		(15.954)		(16.191)
*D*hcgroup*		-32.924**		-31.338*
		(15.954)		(16.191)
Constant	135.489***	93.621***	130.150***	97.096***
	(10.854)	(10.593)	(10.920)	(9.278)
Log likelihood	-4164.799	-4151.091	-4180.799	-4164.836
Wald chi2	13.00	59.79	14.74	82.22
Prob >chi2	0.005	0.000	0.002	0.000
Observations	720	720	720	720
Number of groups	15	15	15	15

Notes. This table reports coefficient estimates (standard errors in parentheses) from two-way linear random effects models accounting for both potential individual dependency over periods and dependency within the group. B, C and D are subjects dummies; *hcgroup* is a dummy that assumes value 1 if the group belongs to the HC groups and 0 otherwise; $B * hcgroup$, $C * hcgroup$ and $D * hcgroup$ are interactions between *hcgroup* and the subjects dummies; Significance levels are denoted as follows: * $p < 0.1$, **$p < 0.05$, and *** $p < 0.01$.

HetE&HomP

The positive and significant ($p < 0.01$) coefficient of the dummy D in column (2) indicates that in the treatment *HetE&HomP* in the LC groups, the player D earn more than A. According to linear combinations of Wald tests on the estimates, in the LC group of *HetE&HomP* player D earns more than B ($\chi^2(1) = 14.42, p = 0.000$) and C ($\chi^2(1) = 10.12, p = 0.002$), while there is no significant difference between B and C ($\chi^2(1) = 0.38, p = 0.538$)[18].

According to linear combinations of Wald tests on the estimates, each player in the HC group of *HetE&HomP* earns significantly more than what she would have earned by adopting a 0-contribution strategy in all periods; A ($\chi^2(1) = 107.80, p = 0.000$), B ($\chi^2(1) = 80.78, p = 0.000$), C ($\chi^2(1) = 51.40, p = 0.000$) and D ($\chi^2(1) = 11.22, p = 0.001$)[19].

Profits in HC groups are significantly higher than profits in LC group for each type of player; A ($\chi^2(1) = 28.00, p = 0.000$), B ($\chi^2(1) = 30.67, p = 0.000$), C ($\chi^2(1) = 33.08, p = 0.000$), D ($\chi^2(1) = 4.70, p = 0.030$)[20].

Finally, in the HC groups of *HetE&HomP* the differences between profits of the different players reduce and almost disappear; in particular, player D does not earn more than player B ($\chi^2(1) = 0.20, p = 0.655$) and C ($\chi^2(1) = 0.33, p = 0.565$) but only more than player A ($\chi^2(1) = 4.52, p = 0.034$)[21].

Result 7 Each player in the HC-groups of *HetE&HetP* and *HetE&HomP* earn significantly more than what she would have earned by adopting a 0-contribution strategy in all periods. Profits in HC-groups are also significantly higher than profit in LC-groups for each type of player in both the treatments *HetE&HetP* and *HetE&HomP*,

[18]According to a non-parametric Wilcoxon signed-rank test the difference between D and A (0.028), D and B (0.018) are still significant but not between D and C (0.128).

[19]This result is confirmed by a non-parametric Wilcoxon signed-rank test, for each player ($p = 0.012$).

[20]This result is confirmed by a non-parametric two-side Mann-Whitney rank-sum test, for each type of player ($p < 0.020$).

[21]According to the non-parametric Wilcoxon signed-rank test the differences between players D and A ($p = 0.049$) is still significant, between D and B ($p = 0.779$) and D and C ($p = 0.483$) are still not significant.

meaning that being in a HC-group is Pareto-improving. In the LC-groups of both $HetE\&HetP$ and $HetE\&HomP$, the wealthiest subject earns significantly more than all the other subjects, implying that in these groups inequality is high. In the HC-groups of $HetE\&HetP$ the differences between profits of the different players and thus inequality disappear; in $HetE\&HomP$ the differences between profits of the different players are reduced.

4.5 Conclusion

Our results are broadly consistent with the idea that wealthy donors, whether individuals, foundations, or government donor agencies (e.g., USAID in international development efforts), have influence over the philanthropic agenda that goes beyond simply their higher donations. We show how their presence in the philanthropic landscape can pull the donations of other donors to their own preferred causes and opportunities. We refer to this tendency as the Gates' Effect.

Although we see no evidence in our experiment that the effect makes any donors worse off, it does reduce the variety of public goods that receive contributions and successfully reach their funding thresholds. In real world donation environments, this reduction in variety could have important implications for social welfare, if for example the preferences of the wealthiest donors are nor representative of the broader needs of society. For example, this could be the case if donor preferences are driven by visibility or financial interests (or potentially national strategic interests in the case of USAID) rather than the needs of society as a whole, including non-donors and marginalized groups. Such possibilities are discussed in surveys of wealthy donors (e.g., Konrath and Clark, 2020; Steuerle et al., 2018; Andrews et al., 2020) and political economy assessments of aid organizations (e.g., Rahman and Giessen, 2017).

It is important to recognize that no aspect of our study requires that the wealthiest donors are ultra-rich. In our experiment, for example, the wealthiest simply have moderately larger endowments than the next wealthiest donor, and yet the donor groups almost always focus on the good preferred by the wealthiest and ignore the good preferred by any others. This suggests that our results may give insights into a variety of settings, whether they involve several high net worth donors, or local fundraising efforts where the budget any donor can contribute is much smaller.

While the paper has largely interpreted the results in terms of philanthropic giving, the analysis may also give insight into donations to political causes or candidates. The model fits well an environment where party members choose which potential party candidate to contribute to during the primary stage of an election campaign in which the party candidate is selected for the general election. While party members may have different preferences over which internal candidate is best to lead the party, their ultimate goal is to eventually coordinate support around a single candidate for the general election. Our results, interpreted literally, illustrate how the larger giving power of the rich attract donations from other party members to the rich-preferred candidates. This does not mean that the other party members are worse off, but it does have implications for the type or representativeness of the candidates who receive enough funding to mount a viable campaign.

The results have important implications for fundraising environments, whether in philanthropy, crowdfunding, or politics. We show how the focus of donors on the recipients preferred by the wealthiest contributors exists even when the wealthy donors do not have a first mover advantage or the ability to set up matching funds or seed money, and even when less-wealthy donors understand that they individually prefer other recipients.

Just as we abstract from several complexities of fundraising environments, we also abstract from several important factors that facilitate donor coordination on the causes or projects preferred by those other than the wealthiest donors. Future research should consider in more detail the potential of common preferences among the less wealthy, sequential giving, or communication among donors to facilitate grassroots efforts or otherwise bring the donor focus to other contribution options.

4.6 Appendix

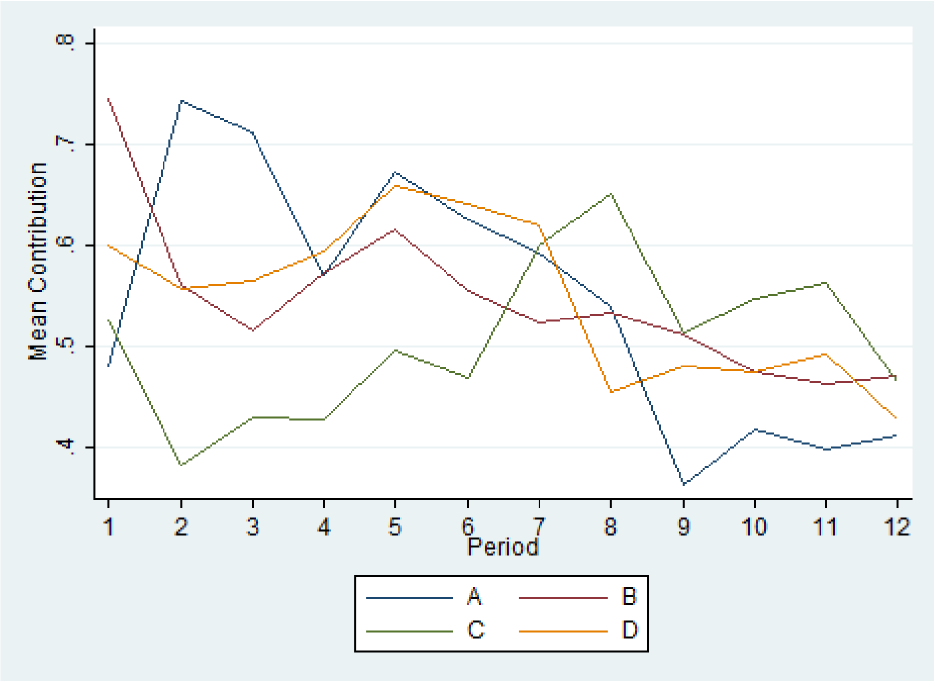


Figure 4.6: Contributions by Subject and Period in $HomE\&HetP$.

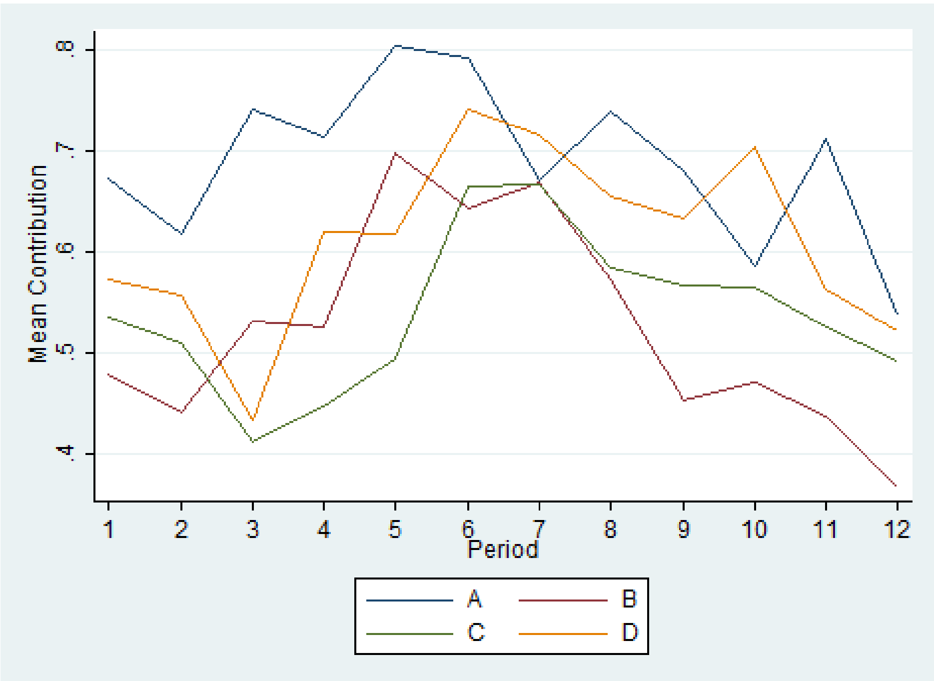


Figure 4.7: Contributions by Subject and Period in $HomE\&HomP$.

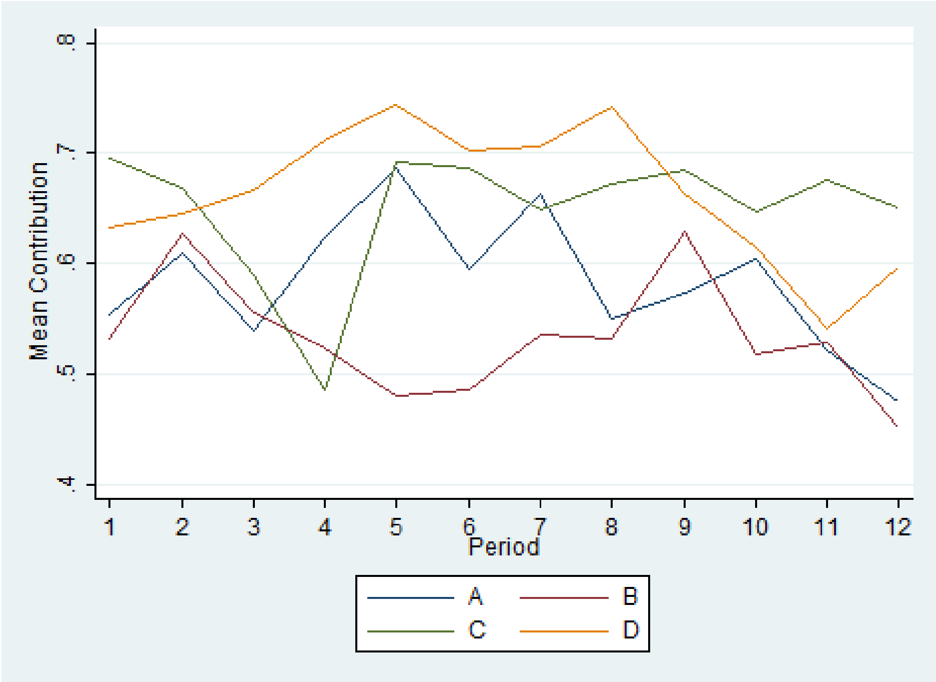


Figure 4.8: Relative contributions by subject and period in $HetE\&HetP$.

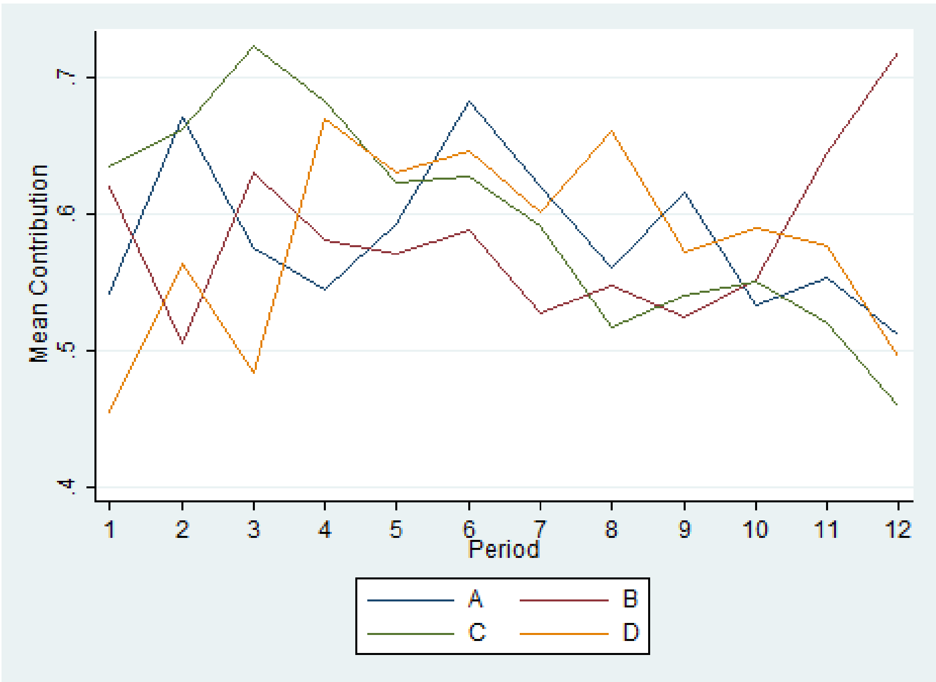


Figure 4.9: Relative contributions by subject and period in $HetE\&HomP$.

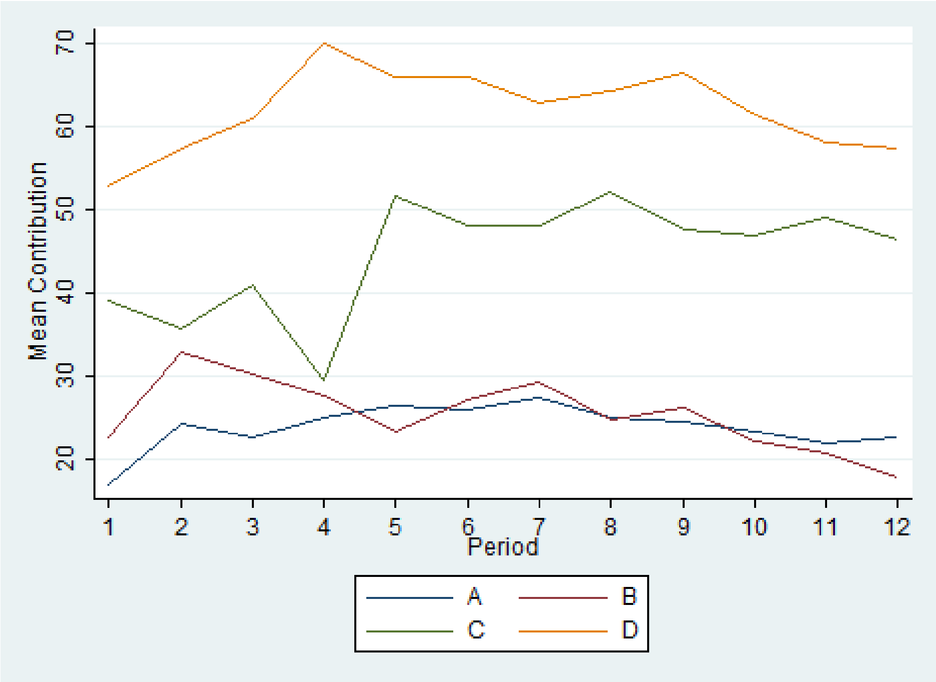


Figure 4.10: Absolute contributions by subject and period in the HC-groups of $HetE\&HetP$.

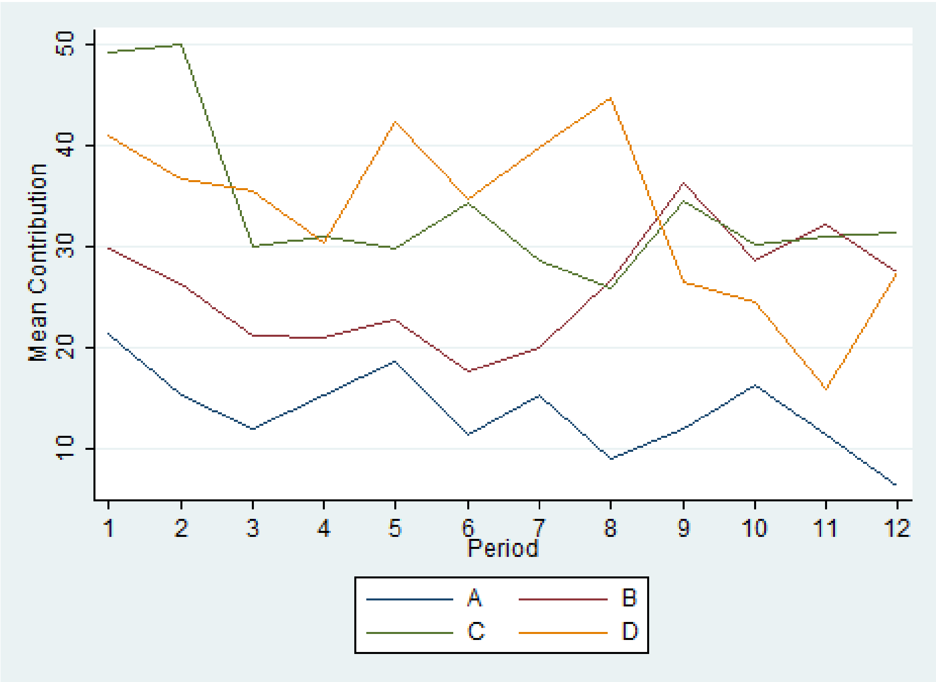


Figure 4.11: Absolute contributions by subject and period in the LC-groups of $HetE\&HetP$.

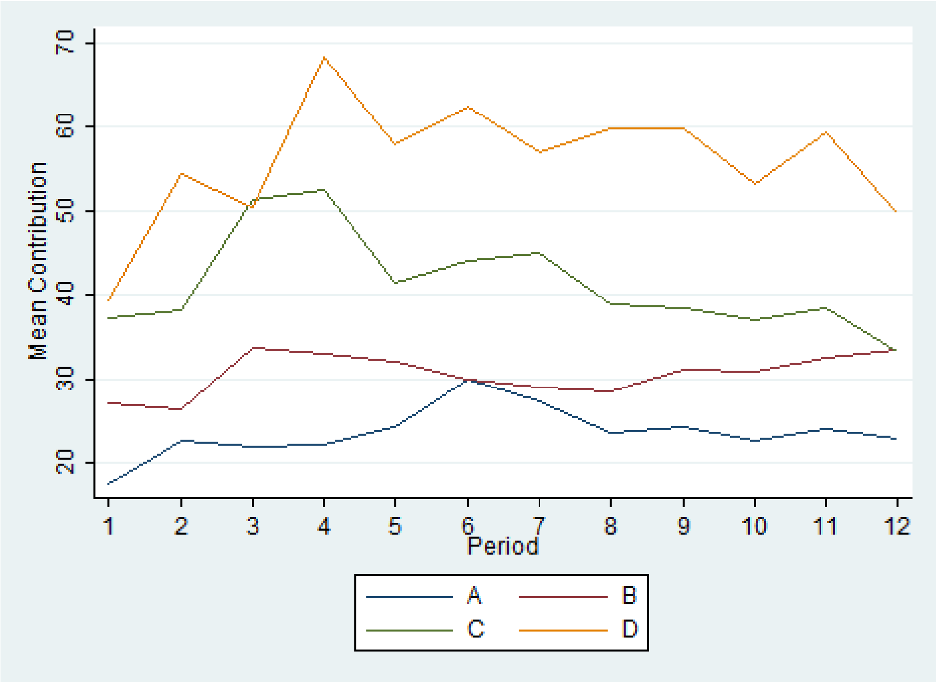


Figure 4.12: Absolute contributions by subject and period in the HC-groups of *HetE&HomP*.

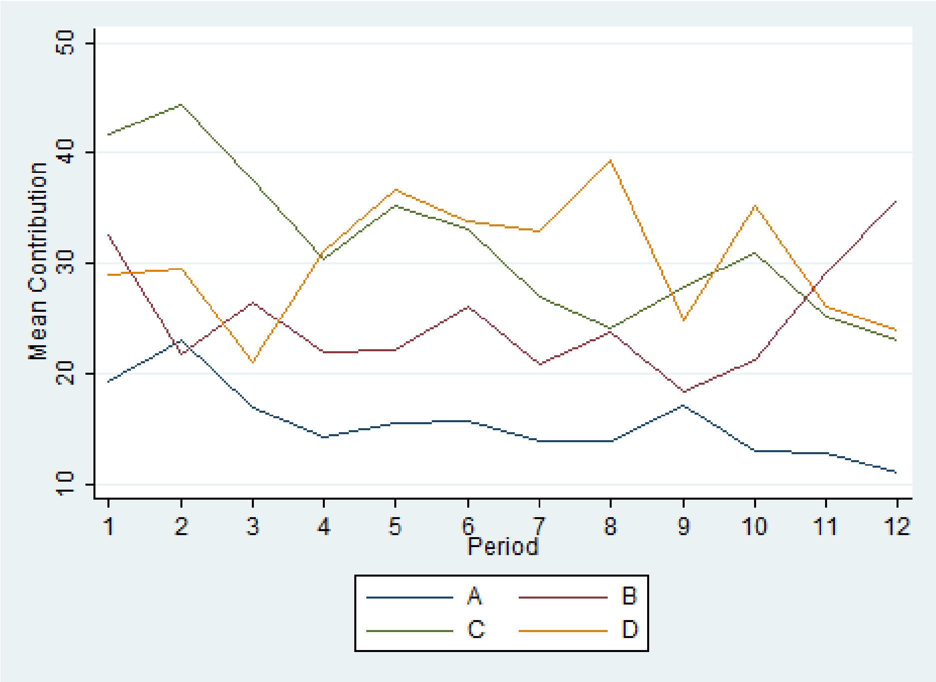


Figure 4.13: Absolute contributions by subject and period in the LC-groups of *HetE&HomP*.

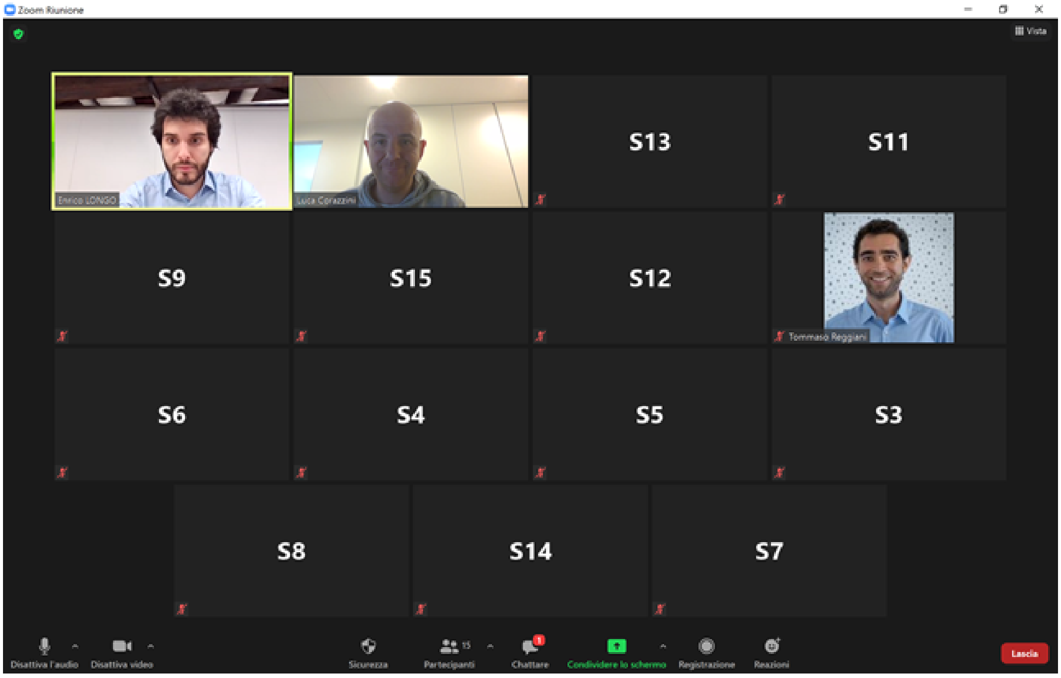


Figure 4.14: A live session of the experiment

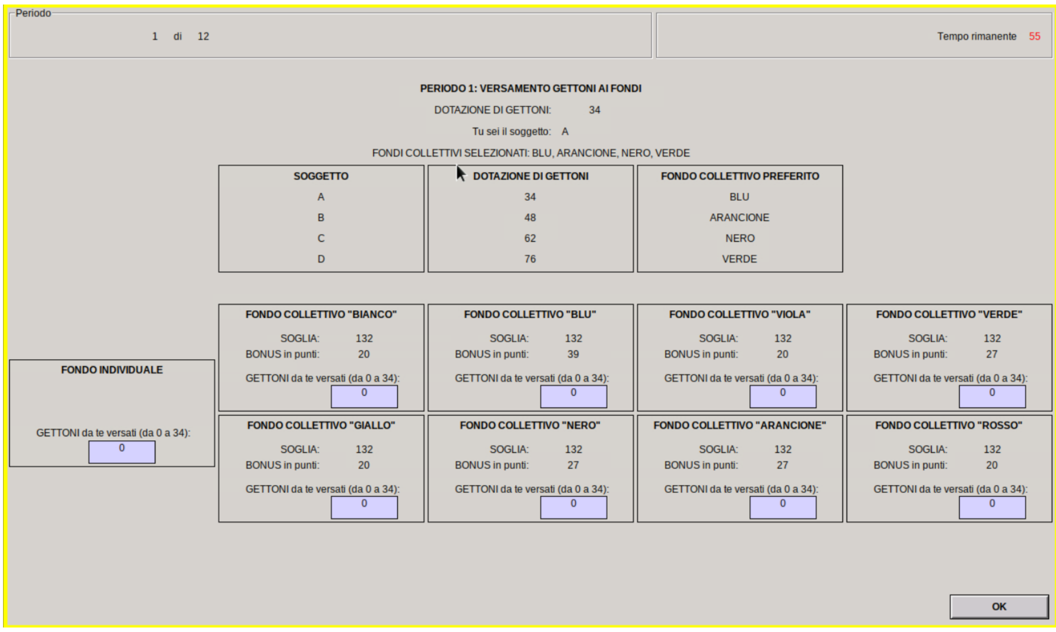


Figure 4.15: Choice Screen.

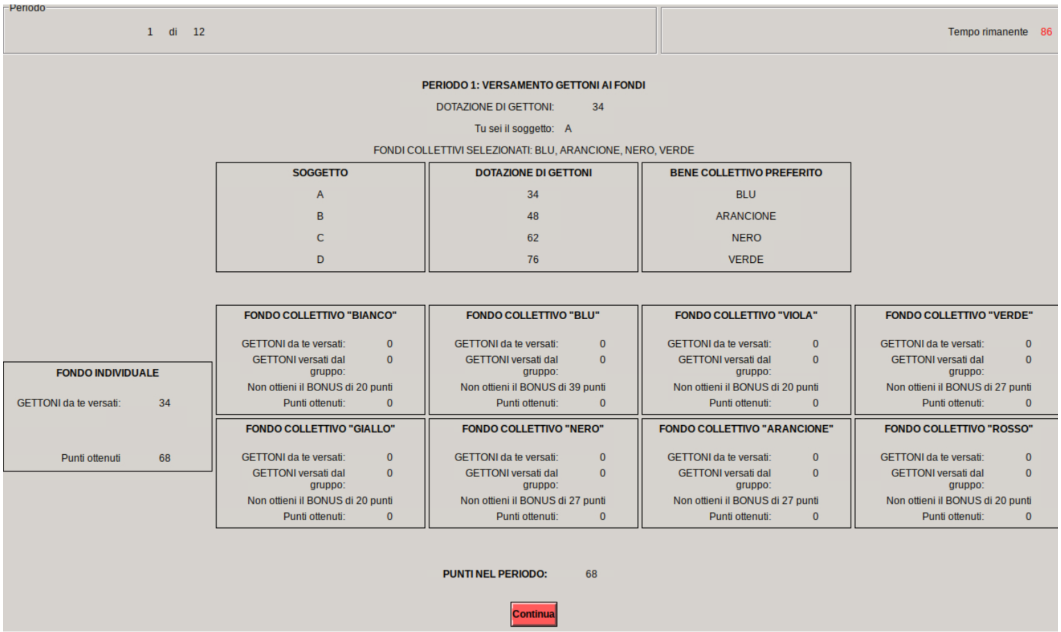


Figure 4.16: Feedback Screen.

	HOM INC & HOM PREF	HOM INC & HET PREF	HET INC & HOM PREF	HET INC & HET PREF	OBS.
Coordination	0.500	0.344	0.539	0.567	180
Coord_PG_A	0.211	0.161	0.156	0.017	180
Coord_PG_B	0.050	0.150	0.089	0.000	180
Coord_PG_C	0.039	0.000	0.028	0.006	180
Coord_PG_D	0.200	0.033	0.267	0.544	180
Contribution	32.474	29.435	32.169	34.022	720
	(19.722)	(21.608)	(22.696)	(23.766)	
Selected	31.200	28.574	31.674	33.519	720
	(20.171)	(21.525)	(22.649)	(23.879)	
Non Selected	1.274	0.861	0.496	0.503	720
	(6.975)	(5.385)	(3.369)	(3.151)	
Contr_A	37.900	29,889	19.833	19.806	720
	(17.966)	(20.297)	(13.080)	(13.097)	
Contr_B	28.794	29.978	28.028	25.583	720
	(19.166)	(21.265)	(18.998)	(17.908)	
Contr_C	29.594	27.811	36.844	40.272	720
	(20.808)	(21.915)	(21.058)	(21.253)	
Contr_D	33.606	30.061	43.972	50.428	720
	(19.657)	(22.967)	(27.590)	(27.204)	
Profit	140.642	115.714	147.478	150.556	720
	(86.217)	(81.750)	(86.663)	(85.315)	
Profit_A	129.789	115.706	130.150	135.489	720
	(87.965)	(84.741)	(90.313)	(88.182)	
Profit_B	148.000	115.394	141.761	151.733	720
	(84.040)	(81.940)	(90.881)	(92.056)	

	HOM INC & HOM PREF	HOM INC & HET PREF	HET INC & HOM PREF	HET INC & HET PREF	OBS.
Profit_C	146.400	117.928	152.128	150.422	720
	(85.110)	(75.850)	(88.139)	(81.120)	
Profit_D	138.378	113.878	165.872	164.578	720
	(87.191)	(84.776)	(72.711)	(77.323)	

Table 4.5: This table reports the descriptive Statistics (mean and standard errors in parenthesis) for Coordination, Contribution and Profit.

4.6.1 Translated Instructions for $HetE\&HetP$

Instructions

Welcome. Thanks for participating in this experiment. By following the instructions carefully, you can earn, based on your choices, an amount that will be paid to you in cash at the end of the experiment. During the experiment it is not allowed to speak or communicate in any way with the other participants. If you have any questions, do not hesitate to contact the researcher through the chat. The following rules are the same for all participants.

General rules

At the beginning of the experiment you will be assigned randomly and anonymously to a group of 4 people respectively indicated with the letters A, B, C, and D. Of each of the other three members of your group you will not know either the earnings. The composition of your group and the initial assignment of the letters will remain the same throughout the entire experiment. The experiment consists of 12 periods, in each of which you will interact exclusively with the subjects of your group. At the start of the experiment, you and every other subject in your group will be given one of four possible sets of tokens so that subject A will receive 34 tokens, B will receive 48 tokens, C will receive 62 tokens, and finally D will receive 76 tokens. This means that, overall, your group will therefore have a total of 220 tokens in each period.

How earnings are determined in each period of the experiment Given your token allocation, you must decide how to divide it between an INDIVIDUAL ACCOUNT and eight COLLECTIVE ACCOUNTS called respectively "WHITE", "YELLOW", "GREEN", "RED", "BLUE", "PURPLE", "BLACK" and "ORANGE". The nine ACCOUNTS generate a return expressed in points based on the following rules: INDIVIDUAL ACCOUNT. You receive points from the INDIVIDUAL ACCOUNT every time you pour tokens into it. In particular, for each token you paid into the INDIVIDUAL

ACCOUNT you will receive 2 points. “WHITE”, “YELLOW”, “GREEN”, “RED”, “BLUE”, “PURPLE”, “BLACK” and “ORANGE” COLLECTIVE ACCOUNT. Receive points from a COLLECTIVE ACCOUNT if and only if the total number of tokens paid into it by the subjects of your group is greater than or equal to a ”threshold” of 132 tokens.

In particular: If the number of tokens paid by your group into a COLLECTIVE ACCOUNT is below the threshold of 132 tokens, then you do not receive any points either from the tokens you paid or from those paid by your group to that COLLECTIVE ACCOUNT. If the number of tokens paid by your group into a COLLECTIVE ACCOUNT is greater than or equal to the 132 chip threshold, then: for each token paid by you or any other person in your group into that COLLECTIVE ACCOUNT you receive 1 point; in addition, you are awarded a ”bonus” in points whose size depends on the COLLECTIVE ACCOUNT to which the tokens were paid. What is the size of the bonus? In period 1, the computer will select four of the eight COLLECTIVE ACCOUNTS at random. The four COLLECTIVE ACCOUNTS selected by the computer will be called ”SELECTED”, while the remaining four will be called ”NOT SELECTED”. The bonus awarded to each person in the group by the four “NOT SELECTED” COLLECTIVE ACCOUNTS will be equal to 20 points. The bonus recognized by a ”SELECTED” COLLECTIVE ACCOUNT depends on whether the subject considers that COLLECTIVE ACCOUNT as ”FAVORITE” or ”NOT FAVORITE”: if for the subject that COLLECTIVE ACCOUNT is ”FAVORITE”, then the bonus awarded to the subject is of 39 points; if instead for the subject that COLLECTIVE ACCOUNT is “NOT FAVORITE”, then the bonus awarded to the subject is 27 points. At the beginning of the first period, the computer will assign each participant a ”FAVORITE” COLLECTIVE ACCOUNT from the four ”SELECTED” so that each ”SELECTED” COLLECTIVE ACCOUNT is preferred by only one person in the group.

How do you make your choices?

The computer will show you your token allocation and nine fields where you can enter your choices, one for the INDIVIDUAL ACCOUNT and one for each of the eight COLLECTIVE ACCOUNTS. In each of the eight fields, the computer will also show you the size of the bonus, 20, 27 or 39 points, awarded in the period to that COLLECTIVE ACCOUNT. A table will also show you which COLLECTIVE ACCOUNTS are PREFERRED by the other parties in the group and their token allocations. For each member of your group, the order in which the fields of the eight COLLECTIVE ACCOUNTS will appear on the screen will be determined randomly by the computer. The sum of the payments made by you in the nine ACCOUNTS must always be equal to your endowment of tokens; this means that in each period you will have to use the full amount of tokens at your disposal.

At the end of each period, the computer will show you how many tokens you have paid into the INDIVIDUAL ACCOUNT, how many tokens you have paid into each of the eight COLLECTIVE ACCOUNTS, how many tokens your group has paid into each of the eight COLLECTIVE ACCOUNTS, how many points you have obtained from the ACCOUNT INDIVIDUAL, how many points you have obtained from each of the eight COLLECTIVE ACCOUNTS and how many points you have gained in the period. At the end of the experiment, the points gained over the 12 periods will be converted into Euros at the exchange rate of 150 points = 1 EUR.

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