Essays on Social Preference

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by

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Abstract

This thesis consists of six essays related to experimental investigation of social preference. We investigate the effects of a pure income effect on social preference in the first essay. In the second essay we explore the effects of gender in altruism and the corresponding anticipation behavior. The third essay discusses the effects of different type of rebate schemes on altruistic behavior. We study the effects of a real and a minimal identity on initiation and escalation of conflict in the fourth essay. The fifth essay investigates the effects of social cues in (anti) social behavior. The final essay tests the effects of pure framing on altruistic behavior.

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Chapter 1 Introduction

The thesis consists of six essays investigating the social preference.

In Chapter 2, Impure Altruism or Inequality Aversion?: An Experimental Investigation Based on Income Effects, we investigate the consequences of a pure income effect on the altruistic behavior of donors. Inequality aversion theories predict either no effect or a decrease in giving, whereas impure altruism theory predicts an increase in giving with an increase in the common income of donor and receiver. Theoretical predictions being contradictory, we run a dictator game in which we vary the common show-up fee of both the dictator and the recipient, while keeping an extra amount to be shared the same. The results are in line with the prediction of the impure altruism theory.

Chapter 3, Altruism, Anticipation, and Gender, discusses Altruistic behavior, anticipation and the gender difference. Overall wellbeing depends both on payoffs and related anticipation, but it is not explored whether altruistic behavior as well as anticipation about the same may differ across gender and across income levels. We study altruistic behavior and the corresponding anticipation under income effect with a focus on gender. In a dictator game we vary the common show-up fee of both the dictator and the recipient, keep the amount to be shared the same, and incentivize recipients to anticipate the amount given. Overall, female dictators give more than their male counterparts. Male recipients, on average, anticipate higher amount than what males dictators give; females do not show such pattern. The results reiterate the differences in altruistic behavior and in the sense of entitlement across gender.

In Chapter 4, An Experimental Investigation of Charity Rebates, we experimentally investigate the effects of the sources of rebates for charity donation. Subjects first play a repeated public good game (PGG) with either a low or a high endowment and then have an option to donate to a charity. They may receive rebate on their donation either exogenously (from the experimenter) or endogenously (from the public account of the PGG), or rebate might not be available. When the PGG Endowment level is low, the endogenous rebate scheme has a negative effect on giving. The exogenous rebate scheme, however, does not have any such effect. If the Endowment level is high and the rebate is endogenous, then other-regarding preferences become salient and boost up charity donation. Females donate more than males, but only under the endogenous rebate scheme. These results shed light on

the effects of the rebate schemes on different income and demographic factors, and provide with relevant policy implications.

Chapter 5, Identity and Group Conflict, experimentally investigate the hypotheses formulated by Sen (2007) that the salience of an identity increases conflict but salience of a mere classification may not do so. Accordingly, we test the effects of real and minimal identity in group conflicts. In the baseline treatment two three-player groups - one consisting of East Asians, and the other consisting of Whites - play a group contest, but no information about the group composition is revealed. In the minimal identity treatment the same experiment is run, but each group is arbitrarily given a different color code. In the real identity treatment the racial composition of the groups are revealed. We find that in all three treatments subjects expend significantly more effort than the Nash prediction. However, efforts are significantly higher in the real identity treatment than the baseline. Minimal identity does not show such an increase in efforts. This result is derived mainly due to the increase in effort expended by females across racial groups, but there is no difference in incremental effort by race for either minimal or real identity.

In Chapter 6, Eye-image in Altruism Experiments: Social Cue or Experimenter Demand Effect?, we focus on the effect of the presence of an image of eyes on altruistic behavior in a lab. It is observed both in economics and in psychology experiments that the presence of an image of a pair of eyes may result in higher level of altruistic behavior by subjects. It is hence concluded that the eye-image serves as a 'social cue'. We test this against an alternative hypothesis that the higher altruism may occur since the eye-image triggers an experimenter demand effect that is in the same direction with perceived altruism. We run a 'Taking game' with and without eye-image in which the recipient owns some amount, and the dictator can take a part of that. In such a case the social cue and the experimenter demand effect go in opposite directions. We find no overall difference in the amount taken in those treatments. However, males take significantly less and females take insignificantly more under the treatment with eye-image. We conclude that the presence of eyes can have both the social cue and the experimental demand effect, and the net effect depends on the magnitude and the direction of the two. For males, the social cue effect is more prominent.

Lastly, in Chapter 7, Giving, Taking and Gender in Dictator Games, we investigate whether a pure framing has an effect in the decision made in a dictator game. We run a giving and a taking dictator game while keeping the strategy space the same. To ensure any possible gender effect we balance the number of male and female dictators in each treatment. Complying with the literature we find no overall difference in the amount allocated to the

recipient across treatments. Adding to the literature we find that females are not only more altruistic than males, they also allocate significantly more amount to the recipient in the taking game compared to the giving game. Males do not show such treatment effect.

Chapter 2

Impure Altruism or Inequality Aversion?: An Experimental Investigation Based on Income Effects[1]

2.1 Introduction

The literature on social preferences, since its inception, has displayed a significant interest in understanding altruism – defined as the principle or practice of concern for the welfare of others. Both theoretical and experimental studies continue to analyze and explain the possible components that affect altruistic decisions. It is intuitive that along with other factors, one's altruistic behavior can be influenced by income effects. Except a few recent developments, the existing literature, however, has abstracted away from this issue. Specifically, how altruistic behavior is affected by a change in income – that has no effect on inequality – has never been investigated. In this paper we aim to fill this gap. We modify relevant existing theoretical models and run a simple dictator game to answer this question. It turns out that in cases where inequality is not salient, income effects are explained with impure altruism.

In a standard dictator game a subject (the dictator) decides how much money to allocate between himself and another passive subject (the recipient). Both the dictator and the recipient are given a show-up fee, and the dictator is then asked to divide an extra amount between himself and the recipient. It is observed that a substantial proportion of dictators allocate a non-trivial share (Kahneman et al., 1986; Forsythe et al., 1994; Camerer, 2003; List and Cherry, 2008; Oxoby and Spraggon, 2008). Since its introduction in the present form, this game has often been used to understand altruism, as the dictator does not otherwise have any incentive to share the money with the recipient. Altruism and social preference theories (Andreoni and Miller, 2002; Charness and Rabin, 2002; Fehr and Schmidt, 2006) such as pure altruism (Becker, 1974), inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), impure altruism (Andreoni, 1989, 1990) and conditional altruism (Konow,

1 This Chapter is a joint work with S. M. Chowdhury. A version of this is published in the Journal of Public Economics as: Chowdhury, S.M., & Jeon, J. (2014). Impure Altruism or Inequality Aversion?: An Experimental Investigation Based on Income Effects, Journal of Public Economics, 118, 143-150.

2010) explain this seemingly non-rational behavior of dictators. Whereas pure altruism assumes that the donor gets utility purely from the well-being of the receivers, inequality aversion theories hypothesize that donors incur disutility from inequality and that, in turn, motivates altruism. Impure altruism theory, on the other hand, hypothesize that donors incur utility from the wellbeing of the recipient, but also earn a 'warm-glow' utility from the giving itself. Conditional altruism theory, in addition, incorporates social norms and includes social preference theories. Please see Konow (2010) for a broader discussion and comparison of each of these theories.

We are interested in analyzing the relationship between a pure income effect and altruism, and in understanding the underlying theoretical mechanism behind the relationship. To study this in a dictator game, one needs to vary the common show-up fee equally for both the dictator and the recipient. Interestingly enough, the effects of show-up fees in dictator game has seldom been the focus of analyses.[2] Whereas a small number of existing studies are interested in understanding the effects of show-up fee inequality (between the dictator and the recipient) on altruism, this particular design has never been studied in the literature. In this study, in different treatments we vary a show-up fee common to both the dictator and the recipient (£0.5, £5, £10, £15, and £20), but keep an extra amount (£10) – that is to be allocated by the dictator – the same across treatments. This frame is also a stylized representation of situations in which an economic agent has the opportunity to be generous to another agent of the same social or income stratum – be it rich to rich, or poor to poor. It resembles circumstances in the field such as sending remittances to family of similar income status (Rapoport and Docquier, 2006), comparison of local charities in high income and low income geographical areas (countries or states), family transfers (Laferrere and Wolff, 2006), inter-generational benevolent behavior such as behaving in an eco-friendly manner to leave a better environment for future generations (Popp, 2001) etc.

Theoretical and behavioral predictions of this framing can be derived from the standard social preference theories and from the observations in the meta-analysis of Engel (2011). In the course of this paper we derive that the inequality aversion theories suggest a

2 Income/endowment effect in the ultimatum game (Knetsch, 1989; Bolton et al., 1998; Armantier, 2006) is well observed. In dictator game, dictators are more self-interested if they earn the amount to be allocated, and are more generous if recipients earn it (Ruffle, 1998; Cherry et al., 2002; Oxoby and Spraggon, 2008). The stake of giving also exhibits a significant effect on giving behavior (List and Cherry, 2008; Johansson-Stenman et al., 2005; Carpenter et al., 2005). The effect of different initial split of the pie has also been investigated (starting with Bolton and Katok, 1998) and it is found that with higher initial share to the recipient, dictator giving decreases. However, only Konow (2010) and Korenok et al. (2012) explicitly introduce the saliency of show-up fees in a dictator game.

non-increasing and sometimes strictly decreasing relationship between the common show-up fee and dictator giving, whereas the impure altruism theory suggests the opposite. Combining the existing experimental studies, Engel (2011, pp. 595), in his meta-analysis, observes

> *"In the standard dictator game, the recipient is poor while the dictator is rich. If the recipient also receives an endowment upfront … this strongly reduces giving… if the recipient has received a positive endowment at the start of the interaction, the reduction is almost perfectly proportional to the size of the endowment..."*

Complying with the impure altruism theory, and contrasting with the inequality aversion theories (or the results stated in the meta-analysis above), we observe a monotone increase in dictator giving with an increase in the common show-up fee.

This analysis is closely related to the research by Korenok et al. (2012). They employ a strategy method in which each dictator makes eight decisions for varying show-up fees. When the show-up fee of the dictators is constant but that of the recipients' increase from zero to the same amount of dictator's, dictators steadily decrease the amount passed to the recipients. It is concluded, hence, that the main motivation of altruism is other-regarding preferences and not warm-glow. This is extended in Korenok et al. (2013). Introducing a price of giving and an endowment to the recipient, they show that a vast majority of the behavior of the dictator can be explained with a theory of impure altruism. The current study is also related to the idea of conditional altruism (Konow, 2010) that incorporates disutility out of deviation from moral norms, and effects similar to warm-glow that relates to long term utility such as prestige or social approval. Konow (2010) employs a subsidy frame among others and shows, again, that the recipient show-up fee has significant effects on the dictator giving. He concludes support for conditional altruism.

2.2 Experimental Design

We ran 5 treatments with 3 sessions under each treatment. 16 subjects participated in each session. All the subjects were students at the University of East Anglia, UK, recruited through the online recruitment system ORSEE (Greiner, 2004). Our design is a variant of the Forsythe et al. (1994) Dictator game. The only difference is that the subjects were given a common show-up fee and that was common and salient knowledge. The treatments differed only in the show-up fees given to the subjects. Dictators were then given an additional £10 and were allowed the choice to allocate the additional amount between him/herself and his/her co-participant (i.e., the recipient). Table 2.1 summarizes the treatment description.

Table 2.1. Treatment description

Treatment	Common show-up fee	Additional amount to be divided	Number of subjects per session	Number of sessions	Number of independent observations
Treatment 1	£0.50	£10	16	3	24
Treatment 2	£5	£10	16	3	24
Treatment 3	£10	£10	16	3	24
Treatment 4	£15	£10	16	3	24
Treatment 5	£20	£10	16	3	24

Although our designs are similar, there also are several differences between Korenok et al. (2012) or Konow (2010) and the current study. First, the existing studies focus on the effects of the dictator-recipient show-up fee difference on dictator giving, but our focus is on the effect of the change in common show-up fee on dictator giving. Thus, whereas those frames are appropriate to study giving behavior when inequality is salient, ours is more appropriate to understand the impact of a pure income effect on altruism. We employ a between-subject design, whereas Korenok et al. (2012) use a strategy method. Our design also differs with that of Konow (2010) in terms of decision space, and we find that the experimental results can be explained by the theory of impure altruism.

In each session, subjects were randomly and anonymously placed into one of 8 pairs and were assigned the role of either a dictator or a recipient. They then received information about their show-up fees, which was the same for all participants in a particular session. Each session consisted of two parts. In the first part, dictators were asked to allocate the additional £10 between themselves and the recipient, up to a fraction of 1 penny. In the second part, recipients had to guess the amount they would receive from the dictator. The instruction of the second part was given only after the decisions of the first part were made, and it was mentioned beforehand, in the instruction of the first part, that recipient's decision is payoff irrelevant to the dictator. This was done to ensure no strategic interaction between dictators' choices with recipient's guesses. Demographic information such as age, gender, nationality, study area of each participating subjects were collected after the experiment. The experiment was run manually and each subject's decision was anonymous to the experimenters. Subjects could participate in only one session. On average, each session took about 45 minutes and the average earnings of subjects (dictator and recipient together) across treatments were £15.10.

However, average earnings varied over treatments between £5.5 (Treatment 1) and £25 (Treatment 5). The instructions are included in the Appendix.

2.3 Theoretical predictions

In this section we derive analytical predictions regarding dictator giving with the theories of inequality aversion and impure altruism, proofs of which are given in the Appendix. We also briefly discuss the theory of pure altruism, and compare the results of conditional altruism with impure altruism theory, but do not provide corresponding proofs.

According to the theory of pure altruism (Becker, 1984; Andreoni, 1989), the utility of a donor depends only on the final payoffs of himself and the receiver. However, the predictions of this model are often not clear. In the current context, it can easily be shown that the pure altruism theory does not provide a specific prediction for an income effect. Giving may stay the same, go up, or go down as a result of an increase in the common show-up fees. Moreover, the predictions of the pure altruism theory are tested and rejected in the literature by Andreoni (1993) and several others over the course of time; and hence we focus on the alternative theories in the current study.

2.3.1 Linear form inequality aversion

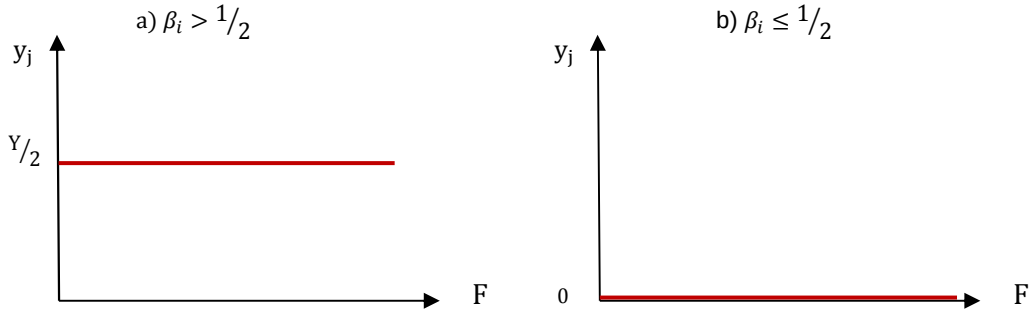
Inequality aversion theories capture the preference of the agents for fairness and defiance to inequality. Fehr and Schmidt (1999) suggest a linear model of inequality aversion in which a donor's utility decreases with the difference in donor and receiver payoff. For a two-player case, this model can be described as

$$u_i = x_i - \alpha_i max\left[x_j - x_i, 0\right] - \beta_i max\left[x_i - x_j, 0\right], \quad i \neq j \qquad (2.1)$$

Where u_i is the utility of subject i; x_i, x_j are payoffs of i and j respectively; and α_i, β_i are inequality aversion parameters with $\alpha_i \geq \beta_i$, and $1 > \beta_i \geq 0$. Let F_i and F_j be the show-up fees and y_i and y_j be the allocations of the pie, Y, for a dictator and a recipient respectively. Hence, $y_i + y_j = Y$, $x_i = F_i + y_i$ and $x_j = F_j + y_j$. We further impose $Y > F_j - F_i$. For a common show-up fee $F_i = F_j = F$, Lemma 1 states the predicted relationship between the equilibrium amount given and the show-up fee. Figure 2.1 summarizes this in a diagram.

Proposition 1. According to the hypothesis of the linear form inequality aversion, the amount given remains the same across treatments ($\frac{dy_j^*}{dF} = 0$).

Figure 2.1. Show-up fee- Dictator giving relationship: Linear form inequality aversion



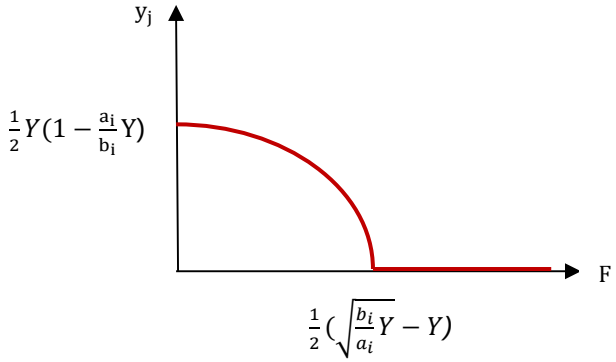
2.3.2 Ratio form inequality aversion

Bolton and Ockenfels (2000)'s ratio form model assumes a decrease in donor's utility with the asymmetry in the ratio in donor and receiver payoff. Following the same notation as earlier, for a two-player case with $F_i = F_j = F$, this model turns out to be

$$u_i = a_i x_i - b_i\left[x_i/(x_j + x_i) - 1/2\right]^2 \qquad (2.2)$$

Where $a_i \geq 0$ and $b_i > 0$ are inequality aversion parameters, $y_i + y_j = Y$, $x_i = F + y_i$ and $x_j = F + y_j$. Proposition 2 and Figure 2.2 summarize the show-up fee – giving relationship.

Figure 2.2. Show-up fee-Dictator giving relationship: Ratio form inequality aversion



Proposition 2. According to the hypothesis of the ratio form inequality aversion, the dictator gives a positive amount at zero common show-up fees. However, giving decreases with an increase in the show-up fee ($\frac{dy_j^*}{dF} < 0$), until a point after which the dictator keeps the whole amount for himself.

2.3.3 The theory of Impure Altruism

The theory of impure altruism (Andreoni, 1989, 1990) considers dictator utility with components of own wellbeing, wellbeing of the recipient, and a warm-glow component (that reflects the joy of giving) through the amount given. For two players, specify this model as

$$u_i = u_i(F_i + Y - y_j, F_j + y_j, y_j) \quad (2.3)$$

Assume that the utility function $u_i(.)$ to be strictly quasi-concave and strictly increasing in all arguments. Proposition 3 and Figure 2.3 describe the derived relationship between equilibrium giving and the common show-up fee for this model.

Proposition 3. According to the theory of impure altruism an increase in the show-up fee strictly increases dictator giving ($\frac{dy_j^*}{dF} > 0$).

Figure 2.3. Show-up fee-Dictator giving relationship: Impure altruism

It is to be noted that the prediction of a positive relationship between the dictator giving and the common show-up fees can also be derived from models introduced later in the literature with structure similar or richer to the theory of impure altruism. Here we discuss one of such richer models. The theory of conditional altruism (Konow, 2010) considers moral norms in donor decisions and also provides a refined structure of effects that a warm-glow component supposed to capture. When the moral norm is considered to be the half of the total wealth, then under $F_i = F_j = F$ the utility function should be

$$u_i = f_i(F + Y - y_j) - g_i\left(F + y_j - \frac{1}{2}(2F + Y)\right) + w_i(y_j)$$

Where the first component of the function represents own wellbeing, the last component is the warm-glow part, and the middle one shows disutility coming through the deviation of recipient's payoffs from the moral norm. It is easy to show that under appropriate assumptions this model's prediction is qualitatively similar to Proposition 3, and we do not provide a formal proof of the same.

2.4 Results

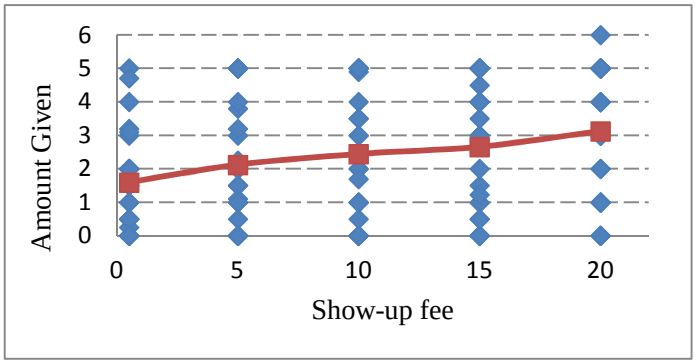
As the treatments are run between-subjects, there are 24 independent observations in each treatment. We run standard non-parametric tests and regressions to assess the conflicting hypotheses arising from the theoretical models.

We start with Table 2.2, which describes the mean and median of giving in each treatment. It also shows the number of subjects giving zero and giving £5 as a measure of pure selfish or pure egalitarian behavior. Only one subject in the whole experiment allocated more than £5 to a recipient. The proportion of pure selfish subjects varies between 12.5% to around 20%, whereas the proportion of egalitarian subjects varies from 4% to 1/3 over treatments. If we consider giving less than £1, too, as selfish behavior, then the total number of selfish subjects goes up to 32, and becomes 42% in the 50p treatment. Given the sessions were run manually, these observations are in line with the results from the existing experiments (Engel, 2011).

Table 2.2. Descriptive statistics of amount given: total

	Show-up fee					
# of obs: 120	0.5	5	10	15	20	Total
Mean	1.59	2.12	2.44	2.66	3.12	2.39
Median	1.25	2	2.25	3	3.50	2
Zero	5	5	5	3	3	21
0<giving<half	18	15	14	15	12	74
Half	1	4	5	6	8	24

Figure 2.4. Show-up fee - amount given scatter plot



One immediate observation from Table 2.2 is that the central tendency of the amount given is steadily increasing with an increase in the show-up fee. This is true for both mean and median giving. Figure 2.4, showing the scatter plot of giving with the average giving per treatment, further supports this observation. However, it is still to be confirmed if this increase in giving is statistically significant.

To test the same, we first run non-parametric tests on the hypothesis of same distribution of amount given over different show-up fees. This hypothesis is rejected at 10%

level with a Kruskal and Wallis (1952) test. Moreover, with two-sided Wilcoxon rank-sum (Mann and Whitney, 1947) tests it is rejected that giving in treatments with high show-up fees is same as giving in treatments with lower show-up fees.

To test whether the increase in amount given across treatments is significant and robust to other controls, we first run a linear regression with amount given as the dependent variable and show-up fee as the explanatory variable. The first column in Table 2.3 shows the result of the regression. The coefficient for show-up fee is positive and significant at 1% level. It shows that a £1 increase in show-up fee increases giving by 7.3 pence on average. In the second model we control for gender, nationality and study areas but show-up fee remains significant with similar impact (6.8 pence increase in giving for a £1 increase in the show-up fee). Out of all the control variables only gender turns out to be significant and females on average are more generous than their male counterparts. Because almost a sixth of the dictators gave nothing, the third and fourth regressions are run with a left-censored Tobit model.[3] However, the direction and significance of the results still remain the same.

Table 2.3. Regression of amount given on show-up fee, gender and other controls

Dependent variable :amount given	(Linear 1)	(Linear 2)	(Tobit 1)	(Tobit 2)
Intercept	1.647***	1.563**	1.366***	1.352
	(0.293)	(0.765)	(0.350)	(0.889)
Show-up Fee	0.073***	0.068***	0.082***	0.076***
	(0.024)	(0.024)	(0.028)	(0.028)
Female		0.745**		0.873**
		(0.338)		(0.393)
Age		-0.007		-0.012
		(0.024)		(0.028)
UK Dummy		0.034		0.057
		(0.355)		(0.413)
Econ Dummy		-0.127		-0.119
		(0.563)		(0.652)
# of Observations	120	120	120	120
Adjusted R^2	0.066	0.076	0.017	0.028

Standard errors are in parentheses; ***,** and * indicates significance at the 1%, 5%, and 10% level

3 The reported values under the Tobit regressions are the coefficients, i.e., the marginal effects on the latent dependent variable. The signs are the same for the marginal effects on the expected values.

The results confirm that the average amount given increases robustly with the common show-up fee. Other variations of the controls (such as other country / study area dummies, age brackets, interaction variables), non-linear effects of the show-up fee, and other regression procedures such as a hurdle-model (Mullahy, 1986) did not come out to be significant, did not change the direction of the results and hence are not reported. In conclusion, a pure income effect – with no implication on income inequality – positively affects altruism. This result is in contradiction with Propositions 1 and 2, but not with Proposition 3. Hence, we conclude that the consequence of pure income effect on giving can be explained by the theory of impure altruism.

2.5 Discussion

We investigate how a pure income effect influences altruistic behavior. In a dictator game we vary the common show-up fee of the dictator and the recipient, but keep the amount to be shared the same. Contrary to the predictions of the standard inequality aversion models and derived results from existing experiments, but in line with the theory of impure altruism, the dictators give more with an increase in the common show-up fee.

If our results from the laboratory generalize to the world at large, then we would expect charity donations to be significantly lower at the time of a recession.[4] In addition, according to our results, (ceteris paribus) one would expect a higher amount of overall charity giving within a richer country compared to a poorer country, more family transfers within wealthier families compared to poorer families, and citizens from the richer countries to be more eco-friendly than their poorer counterparts (supporting the empirical observation by Popp (2001) about impure inter-generational altruism in terms of environmental issues). The current study is also in line with the result obtained by Holland et al. (2012) in their Anthropology field experiment. They left sealed and stamped letters in the streets of 50 neighborhoods in London and found that the likelihood of someone posting the letter in a nearby mailbox is positively correlated with the income-level of the neighborhood. Finally, the current results match, in spirit, with the empirical observation by Hoffmann (2011). He finds that even after controlling for various factors including abilities, richer German citizens saved more Jews people at the time of the holocaust compared to the poorer German citizens.

The main result of our analysis is of interest because existing experimental results to date (such as Korenok et al., 2012 and Konow, 2010) have shown that in a standard dictator

4 See http://news.bbc.co.uk/2/hi/uk_news/7946518.stm and the NCVO/CAF (2009) report on the effects of recession on charitable giving in the UK. Also see the *Giving USA* (2009) report regarding the same in the USA.

game, results can be explained with inequality aversion or conditional altruism theories. Crumpler and Grossman (2008) among others, on the other hand, have shown that impure altruism can explain results in dictator games with a charity frame. Our results imply that impure altruism theory can explain results even in a standard dictator game frame, when income inequality is less salient.

Finally, several studies (Bolton and Katok, 1998; Branas-Garza, 2006; Engel, 2011; Konow, 2010; Korenok et al., 2012) show that an increase in the recipient's income marks a negative impact on the amount given by the dictator, and explain the same through inequality aversion. We observe that if the increase in recipient income is accompanied by an increase in dictator income, then it can even increase giving. Presenting it in another way, unlike the existing studies, we observe that the dictator may even give less to the recipient if the recipient (and common) income is lower. In many of the existing designs, the warm-glow part of impure altruism and the inequality aversion components work in opposite ways in determining giving. If one of the effects is made less salient then it is offset by the other effect and the outcome changes. Hence, to conclude, in some settings inequality aversion may serve as a better underlying model than impure altruism, whereas in some other settings impure altruism may fit better. While, as we find, impure altruism works better in explaining income effects, the scope for exhaustive investigation in this broader topic remains open.

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Chapter 3

Altruism, Anticipation, and Gender[5]

3.1 Introduction

The existing experimental literature elicits divided opinion regarding the effects of gender on altruism. Some studies show females being more altruistic, but certain other studies find no such pattern. It is, however, never investigated which of the observation holds under income effects, i.e., which observation is true across income levels. Studies related to anticipation (expectations regarding physical outcomes that affect one's utility) about altruism, and its interplay with income and gender are also less than adequate. In this article we use a dictator game to investigate if individuals behave and anticipate the same way as a donor and as a receiver in the course of changing income, and whether gender plays any role in the same.

The literature on the effects of gender on altruistic behavior is well-established but dissented.[6] In a first attempt at investigation into this area, Bolton and Katok (1995) find no effect of a dictator's gender on the amount given. But Eckel and Grossman (1998) find that women donate more money than men and this gender difference is much more stable when the recipient is an actual charity rather than another anonymous student. This difference in result prompted further investigations in this area. Later studies such as Cadsby et al. (2010) find no difference, whereas Selten and Ockenfels (1998), Dickinson and Tiefenthaler (2002) find females to be more generous. Gong et al. (2014), however, report the opposite result for a matrilineal society, and no-difference for a patriarchal society in China. Dufwenberg and Muren (2004) find that irrespective of the gender of the dictator, female recipients receive more when recipient gender is revealed. On the contrary, Ben-Ner et al. (2004) show that female dictators give less to female recipients (compared to male or unknown gender recipients), but that male dictators do not exhibit such discriminatory behavior. In a survey Croson and Gneezy (2009) describe gender differences in various preferences, including altruism. They argue that "*the cause of these conflicting results is that women are more sensitive to subtle cues in the experimental context than are men.*" This was later supported in

5 This Chapter is a joint work with S. M. Chowdhury.

6 It holds also for other experiments. Huyck and Battalio (2002) observe no gender differences in a prisoner's dilemma type game, while Charness & Rustichini (2011) find males to be less cooperative. Gneezy et al. (2003) note that competitive environment makes only males more productive. However, Gneezy et al. (2009) show that men in a patriarchal society are more competitive than females, while it is the opposite in a matriarchal society.

Miller and Ubeda (2012), among others. The meta-analysis by Engel (2011) shows females, ceteris paribus, to be more altruistic in dictator games than their male counterparts.

Anticipation plays a major role in the overall wellbeing of economic agents (Elster and Loewenstein, 1992; Frey and Stutzer, 2002). Theoretical (Loewenstein, 1987; Kőszegi, 2010) and applied (Easterlin, 1995) studies show how anticipation regarding various decision making procedures and related outcomes affect utility. But existing experimental research on anticipatory beliefs regarding altruism is sparse, and a specific focus on gender is almost non-existent. Ellingsen et al. (2010) employ anticipation to test guilt aversion. They reveal recipient-anticipation to the dictator but find no effect of the same on dictator giving. Iriberri and Rey-Biel (2013) find introspective anticipatory behavior by the dictator, i.e., dictators who give more (less) anticipate others to give more (less) as well. McBride (2010) shows in an experiment that, ceteris paribus, high expectations result in lower satisfaction. Similar result is also found in the field. Specifically, an increase in income that is not matched with anticipation may fail to improve utility (Easterlin, 1995). On a different context, Stevenson and Wolfers (2009) explore the effects of anticipation on females. Using field data they show that over time the income and life standards of females have improved in terms of objective measures, but subjective well-being have seen declining. Aguiar et al. (2009) study recipient willingness to match with a male or a female dictator and find that females are more willing to be matched with female dictators as they anticipate females to be more generous.

However, the only studies on the particular issue of recipient-anticipation regarding amount given are by Brañas-Garza and Rodriguez-Lara (2014) and Rigdon and Levine (2011). Brañas-Garza and Rodriguez-Lara (2014) focus on the anticipation (termed as ‘expectation’, but we continue to use ‘anticipation’ to avoid any confusion with statistical expectation) of dictators regarding the amount given by other dictators, and also incentivize the recipients to guess the possible amount given to them. Treatments run in Spain and Mexico consistently show that dictators anticipate a near bimodal (zero and equal split) distribution of giving, whilst giving into the interior of the distribution themselves. Recipients robustly anticipate more than the Nash equilibrium with an average anticipation of around 40% of the endowment. Rigdon and Levine (2011) run a ‘Secret Santa’ (Andreoni and Vesterlund, 2001) dictator game in which subjects play both the roles of dictator and of recipient and various dictator choices are run through a strategy method. A baseline in which the giving amount is drawn randomly is also run. The anticipation task regarding the giving of other dictators is not incentivized. Females give more than males when the price of giving is high, and males give more otherwise. But the differences disappear in the baseline. It is

concluded that the gender difference in altruism is a result of the gender difference in anticipation.

These studies, however, are not designed to investigate the effects of varying income and its interplay with gender. As discussed above, change in income may also change the anticipatory behavior. Moreover, Chowdhury and Jeon (2014) show that although alternative theoretical explanations of altruism are sensitive to an income effect, empirically an increase in income is accompanied by a higher level of altruism – supporting the theory of impure altruism.[7] Hence, it is natural to expect gender effects in donation and anticipation behavior when income changes. Our experiment is designed to test this particular issue. In a standard dictator game (Forsythe et al., 1994), we provide the same show-up fee to the dictator and the recipient, but in different treatments we vary the common show-up fee. The dictator is then asked to divide an extra amount between him/herself and the recipient. We further incentivize recipients to anticipate the possible amount which will be given to them. The current study is different from that of Brañas-Garza and Rodriguez-Lara (2014), which does not consider either income effect or gender. It is also different from the study of Rigdon and Levine (2011), as we do not employ the same subject as dictator and recipient and save on any possible behavioral spillover, and instead of investigating price effect in a within subject design, we investigate income effect in a between subject design.

Consistent with the existing results, we find female subjects to be more altruistic than their male counterparts. However, this is driven by the higher show-up fee treatments. Hence, we reaffirm that females are not more generous universally, but only under certain conditions. We also find substantially higher anticipation than the Nash equilibrium predicts. Adding to the existing literature, we find no effects of changing the show-up fee on anticipation by the recipients. Gender, however, does affect anticipation differently. Male subjects on average anticipate more than what they give as dictators. Female subjects do not show such a pattern.

3.2 Experimental Design and Hypotheses

We ran 5 treatments with 3 sessions in each treatment. There were 16 subjects in each session, and each subject could participate in at most one session. The subjects were students at the University of East Anglia, UK and were recruited through an online recruitment system

7 Applications of this can come from anthropology (Holland et al., 2012) or economic history (Hoffmann, 2011). Brañas-Garza (2006), Konow (2010) and Korenok et al. (2012, 2013) also study income effects on altruism.

(ORSEE; Greiner, 2004). The treatments differed only through a common show-up fee that varied (50p, £5, £10, £15 and £20) depending on the treatment. In each session, subjects were randomly and anonymously placed into pairs and assigned the role as either a dictator or a recipient. Each subject played only one role and the roles remained the same until the end of the session. Since we are interested in the effects of own gender on giving and on anticipation, and not the effects of partner's gender; subjects were not informed about their partners' gender. They received salient information about the common show-up fees, which was the same for all subjects in a given session. Dictators had access to an additional £10 and had the choice to allocate it between themselves and the recipient. See Table 3.1 for a summary.

[Table 3.1 about here]

Each session consisted of two parts. In the first part, dictators were asked to allocate the additional £10 between themselves and the recipient, in denominations of 1 penny. In the second part, recipients had to guess the amount they would receive from the dictator. If the absolute difference between the actual amount given and the guess was within 50 pence, then the recipient received an extra £1.[8] The instructions for the second part was given only after the decisions in the first part were made. It was mentioned in the instruction of the first part that the recipient's decision was payoff irrelevant to the dictator. This was done to avoid any possible strategic interaction between dictator giving and recipient anticipation. Demographic information such as gender, age, nationality, subject area of each participating subject was also collected after the experiment. The instructions are included in the Appendix.

Given the existing literature and the design of the current study, the following testable hypotheses are stated regarding altruism, anticipation and gender.

Hypothesis 1: (*Gender-effects on altruism*). Male and female dictators give the same amount.

As discussed, existing results regarding the effects of gender are inconclusive. Hence, our null hypothesis is gender indifference in dictator giving (Bolton and Katok, 1995). The alternative hypothesis is to predict females to be more altruistic (Eckel and Grossman, 1998).
Hypothesis 1A (Female altruists): Females give more than their male counterparts.

8 We implement a linear incentive mechanism as in Brañas-Garza and Rodriguez-Lara (2014). It could also have been possible to implement a quadratic mechanism. But since the quadratic mechanism is rather complicated whereas the game itself is overtly simple, and since there is no strict empirical consensus over the applicability of the two mechanisms, we decided to stick to the linear mechanism.

Hypothesis 2: (*Impure altruism*). Anticipated amount increases with the show-up fee.

This hypothesis is extended to the recipient anticipation from the results of Chowdhury and Jeon (2014) in which abiding with the predictions of impure altruism, giving increases with an increase in the show-up fee. Alternatively, if anticipation does not follow impure altruism, this monotonic relationship will not hold.

Hypothesis 3: (*Gender-effects on anticipation*). Male and female recipients anticipate the same amount.

As in Hypothesis 1, here we hypothesize gender indifference in recipient anticipation. The alternative to this comes from the gender difference in entitlement. A steady stream of literature in social psychology (see, for example, Bylsma and Major, 1992) observes males to have much higher feeling of entitlement than that of females. Hence, male recipients are more likely to have higher amount anticipated.

Hypothesis 3A: (*Male entitlement*). Males anticipate higher amount than females.

Hypothesis 4: (*Comparison of altruism and anticipation*). Dictators give and recipients anticipate the same amount, overall and across gender.

If there is no effect of gender in giving, then there should also be no effect of gender in anticipation either. However, combining the results of the studies outlined earlier, the alternative hypotheses would be:

Hypothesis 4A: (*Gender effects*). Female recipients anticipate less than what the female dictators give and male recipients anticipate more than what the male dictators give.

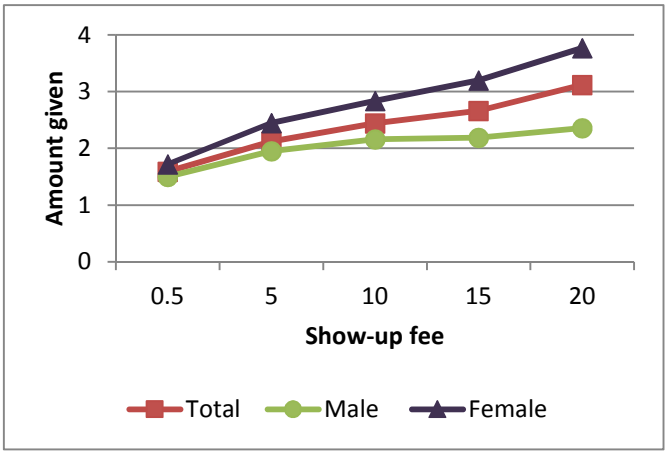
3.3 Results

To test the hypotheses stated above, we first present the descriptive statistics before running formal tests. Table 3.2 shows the mean and median of amount given, number of subjects giving zero and giving £5 (half of the additional endowment), by each show-up fee amount for all subjects, and separates results into males and females. It shows the central tendency for the amount given by female dictators to be higher in every treatment compared to their male counterparts. Below we report the first result.

[Table 3.2 about here]

Result 1. Female dictators tend to be more generous, in the sense of amount given, than their male counterparts. This is specifically true in the case of higher common show-up fees.

Figure 3.1. Show-up fee - average amount given



[Table 3.3 about here]

Support. From Figure 3.1 and Table 3.2 initially observe that females on average give more. Moreover, the number of dictators giving zero for each show-up fee is higher for males and the number of dictators giving £5 or more is higher for females. To test statistically, Table 3.3 shows OLS and Tobit regression results for amount given on show-up fee, gender and other control variables.[9] In Table 3.3, a dummy variable indicating female turns out to be positive and significant in predicting amount given under various specifications and controls. Mann-Whitney tests show that females overall give more than males (p-value = 0.016), but treatment-wise this is driven by the treatment with show-up fee £20 (p-value = 0.075). □

Engel (2011) in his meta-analysis reports that females are more generous overall in terms of giving than males. We find support for the same. However, this is driven by the high show-up fee treatments: when the common show-up fee is not too high, male and female dictators behave in similar manner. This is in line with the findings that females are more responsive to social conditions and experimental treatments than men (Ben-Ner et al., 2004; Croson and Gneezy, 2009). This also is similar to the results that females' altruistic decisions (Andreoni and Vesterlund, 2001) and decision to trust (Cox and Deck, 2006) vary with the stake-size more than it does for males'. This, however, is different from the results obtained by Andreoni and Vesterlund (2001) and Rigdon and Levine (2011) who show females to be more altruistic than males when the relative opportunity cost (relative price) of giving is high. Here the opportunity cost of giving, in the sense of diminishing marginal value of money, is

9 The results are robust even without including the control variables. It is also possible to use treatment dummies instead of show-up fees, as it captures the same effect. But it has less predictive power since instead of the differential show-up fees in different treatment, the dummies only indicates a binary value.

lower for higher show-up fees. But, as females give more when the show-up fee is high, this means females are being more altruistic when the opportunity cost of altruism is low.

Next we investigate the effects of varying show-up fee on anticipation. Note that since anticipation is incentivized, show-up fee might have had a systematic effect on correct guesses, since the fixed incentive (£1) for a correct guess declines relatively as show-up fee increases. We did not find such evidence. For show-up fees 50p, £5, £10, £15 and £20 the correct guesses were 2, 9, 7, 6, and 5. Table 3.4 summarizes the anticipation results and shows no specific pattern over show-up fees.

[Table 3.4 about here]

Result 2. Show-up fee does not have a significant effect on the amount anticipated by the recipients in general and also separately for males and females.

Support. Table 3.4 shows the descriptive statistics for amount anticipated with changing show-up fee for all subjects and for female and male subjects. Observe that there is no monotonic pattern between amounts anticipated and show-up fee in any of the specifications. Table 3.5 shows OLS and Tobit regression results for amount anticipated on show-up fee and other controls.[10] Show-up fee is not significant in any specification for overall data as well as separately for males and females. Kruskal-Wallis tests also confirm the same result. (All p-value = 0.956; Male p-value = 0.978; Female p-value = 0.969) □

[Table 3.5 about here]

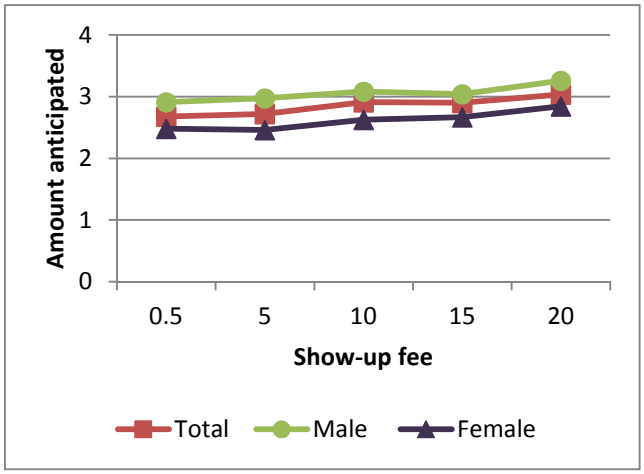
Result 3. Although male recipients on average seem to anticipate higher amounts than what female recipients anticipate, this result is not statistically significant at conventional level.

Support. Compare Table 3.2 and Table 3.4 and observe that males consistently anticipate higher amounts than females for every show-up fee level. Figure 3.2 summarizes the result. This is consistent with a general 'dovish' nature of female subjects. Rigdon and Levine (2011) observe similar outcome when dictator giving is not random and Eckel and Grossman (2001) find women significantly more likely to accept lower offers in an ultimatum game than men. The regression in Table 3.5, however, shows that although the coefficient on the female indicator variable has a negative sign, it is not significant at 10% level. Mann-Whitney tests also show no differences in male and female anticipation. (p-value = 0.174)

□

10 We also used a control namely the ratio of females to males in a session. This is introduced to control for possible observed gender effect in a session, but it did not come out to be significant.

Figure 3.2. Show-up fee - average amount anticipated

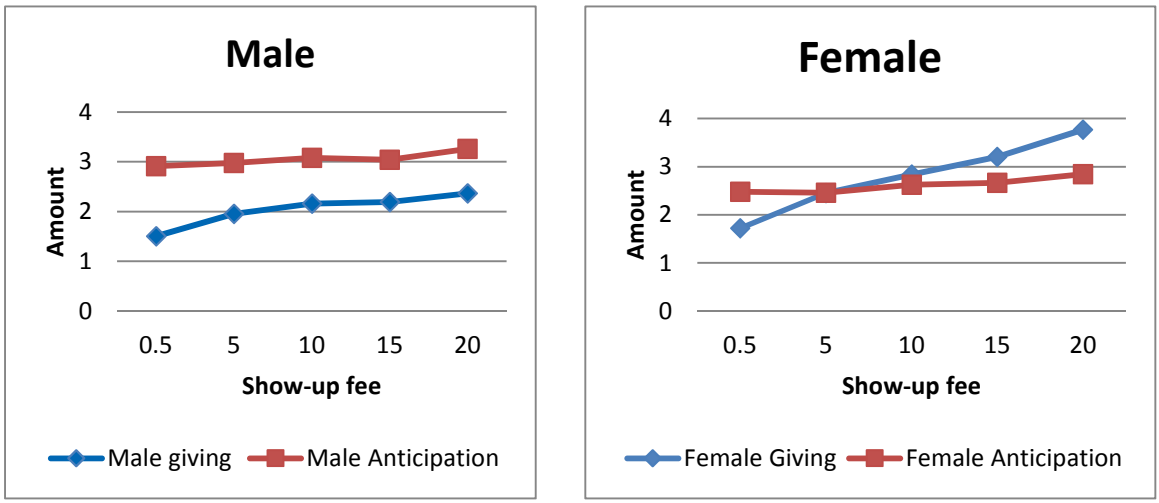


Finally, we compare giving and anticipation behaviors. It seems from the descriptive statistics and Figure 3.1 and Figure 3.2 that recipients on average anticipate more than what dictators actually give. This is consistent with the observations from Brañas-Garza and Rodriguez-Lara (2014). A Mann-Whitney test supports this result (p-value = 0.052). However, more interesting results are obtained by considering the effects of gender.

Result 4. Male recipients anticipate higher amount being given to them compared to the amount the male subjects give as dictators. Females do not show such significant pattern.

Support. Comparing Table 3.2 and Table 3.4, observe that on average males anticipate higher amounts being given to them than what they give to a recipient themselves. However, no such result is observed for females. Mann-Whitney tests support both observations (Male p-value = 0.001; Female p-value = 0.393). Figure 3.3 summarize the result in diagrams. □

Figure 3.3. Amount given and amount anticipated: Male vs. Female



This does not occur due to risk aversion, as both genders on average guess the same amount across show-up fees. Males, however, consistently guess higher amounts than females. This result reconfirms that there is a gender difference in entitlement (Bylsma and Major, 1992). Males, both as dictators as well as recipients feel more entitled to the amount to be shared. As a result male dictators give less, and male recipients expect more. This effect is not present for females and there is no difference in female giving and anticipation. Alternatively, since it is observed that males are on average overconfident compared to females (Deaux and Farris, 1977; Barber and Odean, 2001; Belafoutas et al., 2012) but as a consequence often wrong (Lundeberg et al., 1994; Barber and Odean, 2001), this result may also be interpreted in terms of male overconfidence.

3.4 Discussion

It is known from existing studies that altruism is positively correlated with a pure income effect. It is also pointed out that understanding anticipation behavior is important, and the behavior may vary across gender. However, the interaction of an income effect with gender and the corresponding effects in anticipation behavior regarding altruism is never investigated. We study this question through a dictator game in which the common show-up fee is varied, but the amount to be shared is kept constant. We compare the amount given by dictators and anticipated by recipients with a specific focus on the gender of the subjects. Our aim in this investigation is two-fold. First, we aim to contribute to the literature on the varied and disputed effects of gender on altruism with a focus on income effects. Second, we seek to understand the differential effects of gender on giving and anticipating behavior. Our study can be used as a first attempt to understand gender difference in anticipatory beliefs regarding altruism across income levels. This, in turn, can be explored further to understand the relationship between expectation and overall wellbeing.

We find that dictators give more as the common show-up fee increases, but that there is no effect of the show-up fee on recipients' anticipation regarding the possible amount received. This is true in general and separately for both male and female. This result asserts that the recipients do not follow a particular pattern due to the income effect. They rather focus only on the amount to be split, follow some type of rule of thumb and expect an average of a quarter of the amount.

In line with the results of Eckel and Grossman (1998), we find that female dictators give more than their male counterpart. But closer investigation shows that this is driven by

the cases of high show-up fees. This result is interesting since several existing studies investigating the effects of the price of giving observe that females give more when the opportunity cost (relative price) of giving is higher. We, however, find that females give more when the common show-up fee is higher, i.e., the opportunity cost of giving (the marginal value of money) is lower. Our result, in summary, states that the income effect and price effects work very differently in altruistic decision, especially across gender.

Finally, we observe that although on an average male dictators give less than females; male anticipation is not statistically different from that of female recipients. Male recipients, however, anticipate higher amounts being received compared to what the male dictators actually give. Females do not show such a pattern. Hence, overall females are better at introspecting about the amount given to them, compared to males. This result can be explained by the fact that males have a higher feeling of entitlement compared to females. Since the match (or mismatch) of anticipation with reality plays a significant role in overall wellbeing, it shows that females may actually be better off in terms of subjective wellbeing than their male counterparts. We term this feature as the *Baldwin Conjecture*, named after Stanley Baldwin, the former Prime Minister of the United Kingdom who famously quoted – "*I would rather trust a woman's instinct than a man's reason.*"

It would be possible to introduce interesting features such as allowing subjects to earn the show-up fee and/or the amount to be split (real effort tasks), changing the amount to be split over different treatment and investigate the anticipation behavior, inducing in-group out-group features among subjects, and revealing gender information in the current set-up to address further questions. It is also possible for us to use the current structure to understand the issues of gender stereotypes (Grossman and Lugovskyy, 2011; Grossman, 2013). We leave these issues for future research.

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Table 3.1. Treatment table

Treatment	Common show-up fee	Additional amount to be divided	Number of subjects per session	Number of sessions	Number of independent observations
Treatment ½	£0.50	£10	16	3	24
Treatment 5	£5	£10	16	3	24
Treatment 10	£10	£10	16	3	24
Treatment 15	£15	£10	16	3	24
Treatment 20	£20	£10	16	3	24

Table 3.2. Descriptive statistics of amount given

	Show-up fee																	
	0.5			5			10			15			20			Total		
	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female
Mean	1.59	1.50	1.72	2.12	1.95	2.45	2.44	2.16	2.84	2.66	2.19	3.20	3.12	2.36	3.77	2.39	2.02	2.88
Median	1.25	1.25	1.25	2	2	1.75	2.25	1.35	3	3	2	4	3.50	2	5	2	2	3
Zero	5	4	1	5	4	1	5	3	2	3	2	1	3	2	1	21	15	6
0<giving<5	18	10	8	15	10	5	14	8	6	15	9	6	12	7	5	74	44	30
Half	1	0	1	4	2	2	5	3	2	6	2	4	8	2	6	24	9	15
Total Observations	24	14	10	24	16	8	24	14	10	24	13	11	24	11	13	120	68	52

Table 3.3. Regression of amount given on Show-up fee, gender and other controls

Dependent variable :amount given	Total (OLS)	Male (OLS)	Female (OLS)	Total (Tobit)	Male (Tobit)	Female (Tobit)
Intercept	1.267***	1.167***	2.064***	0.896**	0.725	1.914***
	(0.348)	(0 .422)	(0 .527)	(0.411)	(0.522)	(0.563)
Show-up Fee	0.0732***	0 .056*	0.108***	0.0818***	0.069*	0.111***
	(0.024)	(0 .032)	(0.038)	(0.028)	(0.039)	(0.040)
Female	0.691**			0.812**		
	(0. 338)			(0.392)		
Age $\leq$ 21	0.507	1.293**	-0.640	0.637	1.537**	-0.636
	(0 .431)	(0 .532)	(0.704)	(0.500)	(0.644)	(0.745)
UK	-0.214	-0.181	-0.018	-0.242	-0.250	0.006
	(0 .411)	(0 .494)	(0.691)	(0.479)	(0.599)	(0.733)
ECO	-0.058	0.011	-0.793	-0.045	0.110	-0.855
	(0.559)	(0 .745)	(0.862)	(0.648)	(0.895)	(0.919)
# of Observations	120	68	52	120	68	52
Adjusted R^2	0.0863	0.0649	0.1042			
Chi^2 test				0.0106	0.0925	0.0778

Standard errors are in parentheses; ***,** and * indicates significance at the 1%, 5%, and 10% level respectively.

Table 3.4. Descriptive statistics of amount anticipated

	Show-up fee																	
	0.5			5			10			15			20			Total		
	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female
Mean	2.68	2.91	2.48	2.72	2.98	2.46	2.91	3.08	2.63	2.90	3.04	2.67	3.03	3.26	2.85	2.85	3.05	2.60
Median	3	3	3	3	3.5	2.75	3	3	3	3	3.43	2.5	3.17	3.33	3	3	3	3
Zero	2	1	1	3	2	1	1	1	0	0	0	0	1	0	1	7	4	3
0<giving<5	18	7	11	16	7	9	20	12	8	21	14	7	16	6	10	91	46	45
Half	4	3	1	5	3	2	1	0	1	3	1	2	6	4	2	19	11	8
> 5	0	0	0	0	0	0	2	2	0	0	0	0	1	1	0	3	3	0
Total observations	24	11	13	24	12	12	24	15	9	24	15	9	24	11	13	120	64	56

Table 3.5. Regression of amount anticipated on Show-up fee, gender and other controls

Dependent variable :amount anticipated	Total (OLS)	Male (OLS)	Female (OLS)	Total (Tobit)	Male (Tobit)	Female (Tobit)
Intercept	2.657***	2.723***	2.218***	2.570***	2.610***	2.159***
	(0.366)	(0.485)	(0.445)	(0.379)	(0.496)	(0.448)
Show-up fee	0.019	0.014	0.025	0.022	0.020	0.026
	(0.022)	(0.035)	(0.030)	(0.023)	(0.035)	(0.031)
Female	-0.385			-0.379		
	(0.313)			(0.323)		
Age $\leq$ 21	0.283	0.297	0.283	0.307	0.326	0.293
	(0.359)	(0.506)	(0.540)	(0.371)	(0.518)	(0.541)
UK	0.155	0.059	0.214	0.151	0.053	0.222
	(0.352)	(0.507)	(0.515)	(0.363)	(0.519)	(0.516)
ECO	0.035	0.249	-0.207	0.037	0.217	-0.163
	(0.452)	(0.664)	(0.653)	(0.466)	(0.678)	(0.654)
# of Observations	120	64	56	120	64	56
Adjusted R^2	-0.0078	-0.0522	-0.0463			
Chi^2 test				0.5326	0.9019	0.8115

Standard errors are in parentheses; ***,** and * indicates significance at the 1%, 5%, and 10% level respectively.

Chapter 4

An Experimental Investigation of Charity Rebates[11]

4.1 Introduction

The charity sector, also known as the voluntary sector, is an integral part of an economy and contributes significantly towards the overall welfare. In the UK, for example, the contribution of the voluntary sector into the gross value added (GVA) was £11.7 billion in 2011, while in the same year the contribution of the agriculture sector was only £8.3 billion.[12] Governments from around the world, as a result, are keen on understanding the possible incentive schemes to increase contributions to charity. Furthermore, as Dhami and Al-Nowaihi (2010) indicate, one of the main stylized facts in charity donations is that "*Individual private donors are the largest contributors*". Hence, it is not surprising that a substantial amount of research has also been carried out to understand the determining factors of individual charity giving and the ways to increase amount given by the individual donors.

While various topics are discussed in the charity donation literature, a popular issue that remains active is the role of charity rebate and its effects on the giving behavior. The theoretical literature on providing rebates for charity donations delineates two effects. First, a rebate will have a direct effect of increase in total charity donations through the channels such as warm glow (Andreoni, 1990) and conspicuous consumption (Glazer and Konrad, 1996). But it may also have a negative effect through crowding out (Warr, 1982). In the experimental literature, often a rebate is introduced into a dictator game and the giving is then compared with the case of no rebate in order to test the effects of framing the rebate on the charity donation (Eckel and Grossman, 2003, 2006, 2008; Davis et al., 2005; Davis, 2006). It, however, is rarely examined how the source of the rebate may affect charity donation.

This question is of high importance since it captures some specific mechanisms behind the decision to give and the amount given that are hitherto overlooked in the literature. When a subsidy or rebate mechanism is employed in the laboratory, it is provided by the experimenter and is thus exogenous to the system. This setting is contrary to most of the field situations. A tax reduction, for example, is equivalent to the rebate or subsidy in the

11 This Chapter is a joint work with E. Fatas and P. Ubeda.
12 See www.ncvo.org.uk for further detail.

experiment; but since it is budgeted by the tax revenue of the government, it actually is endogenous to the system. It is well possible that an endogenous rebate scheme result in a very different charity donation pattern than an exogenous one. It may also be possible that this variation itself is diverse across agents – in terms of financial and demographic factors. In this study we introduce an experiment to explore these issues.

To supplement the gap between real life and the experimental literature, we employ a rebate scheme in which the cost of giving to the charity is budgeted endogenously. Overall, we allow three alternative between-subject charity rebate schemes: no rebate, exogenous rebate and endogenous rebate. Subjects first play a repeated public good game (PGG) with either a low or a high endowment, and then have an option to donate to a charity in a dictator game (DG). When a rebate on donation is available, subjects receive rebate either exogenously (from the experimenter) or endogenously (from the public account of the PGG).

To the best of our knowledge, Chavanne et al. (2011) is the only other study that discusses the relationship between the source of rebate and giving. They demonstrate that the amount given increases only when the experimenters fund the rebate. There are, however, fundamental differences between this study and ours. To investigate the effects of the source of rebate, Chavanne et al. (2011) use a group dictator game in which dictators are allowed to spend other group members' endowments to their recipients. As a result, not only they share the cost of giving with others, but they also receive the identical payoff with group members. On the contrary, each subject in our experiment decides how to divide his own endowment between himself and a real charity although the cost of giving is shared by group members and the individual payoff is not identical across group members.

We employ two different endowment levels in the PGG to capture possible income effect on giving and any interaction of the rebate schemes with the same. It is well known that income level is one of the most important components to explain giving behavior. When an individual has higher income, it leads to a higher donation (Feldstein and Clotfelter, 1976; Andreoni, 1990; Randolph, 1995; Auten et al., 2000, 2002; Andreoni and Vesterlund, 2001; Eckel and Grossman, 2003; Buckley and Croson, 2006; Chowdhury and Jeon, 2014). Hence, it might be possible that a particular rebate scheme affect the giving behavior of agents at a certain income level but not with another.

Incorporating the discussions above, we intend to answer the following questions. Is the endogenous rebate scheme as effective in increasing the total level of donations as the exogenous rebate scheme? Does an increase in income increases donation, as has been identified in the literature, irrespective of the different type of rebate schemes? Furthermore,

it is observed in the literature that females are often more generous in terms of charity giving than their male counterpart (Engel, 2010). It is, therefore, of interest to investigate whether the alternative rebate schemes affect male and female donors any differently.

The main results of this study are as follows. The endogenous rebate scheme affects Rich (high PGG endowment) and Poor (low PGG endowment) subjects differently. For Poor subjects the endogenous rebate has a significant negative impact on DG giving, whereas this has no significant effect on Rich subjects. Additionally, the exogenous rebate scheme, the traditional way to adopt the rebate in an experiment, does not have any significant effect on giving. This is in contradiction to a large part of the existing literature.

We further employ the level of contribution in the first round of the PGG as an index of other-regarding preference. In the high endowment treatments under the endogenous rebate scheme, the social type subjects in PGG are also more benevolent in DG giving than others. But under similar treatments when the rebate scheme is exogenous, there is no difference between the DG giving of social type and selfish type subjects. These results show that other-regarding preference becomes salient under endogenous rebate scheme for the Rich subjects. Our results also shed light on the gender difference with respect to the rebate schemes. Females are more generous than their male counterparts, but only when the rebate is endogenous – indicating a possible need for demographic specific rebate policy.

The rest of the paper proceeds as follows. In Section 2 we describe the experimental design and procedures. Section 3 presents the main results and Section 4 concludes.

4.2 Experimental design and procedure

We employ a 2×3 factorial design in which in one dimension the endowment is varied in the PGG and the rebate scheme is varied in the DG in another. Table 4.1 summarizes the design.

Table 4.1. Experimental Design

Rebate / Endowment	Baseline (No Rebate)	Exogenous Rebate	Endogenous Rebate
Low Endowment (100 ECUs)	BSL-LOW 32 Obs.	EXO-LOW 36 Obs.	END-LOW 36 Obs.
High Endowment (1000 ECUs)	BSL-HIGH 36 Obs.	EXO-HIGH 36 Obs.	END-HIGH 36 Obs.

Each treatment consists of two parts. The first part is a four-player PGG with partner matching repeated for 20 rounds, and the second part is an individual DG in which the recipient is a real charity.[13] The subjects know that the experiment consists of two parts, but they are not aware of the contents of the second part when they are involved in the first part. This is done to eliminate any possible expectation effects about the second part in the decision made in the first part of the experiment.

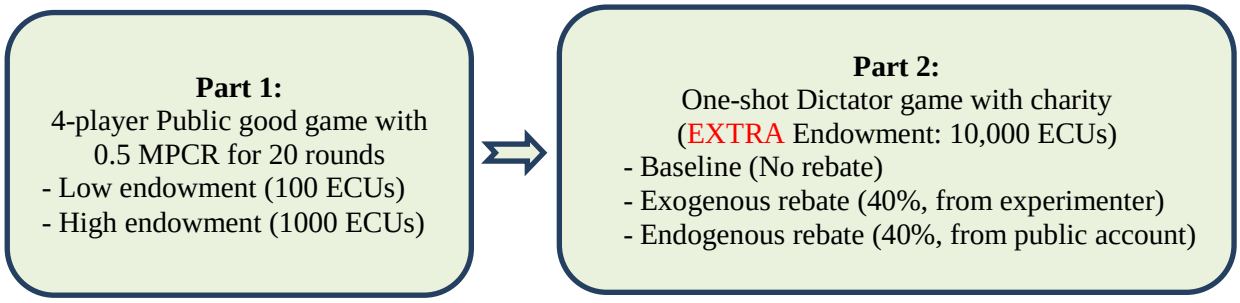
In the first part of the experiment, depending on the treatment in each round of the PGG subjects are endowed either with 100 Experimental Currency Units (ECUs) or with 1,000 ECUs and have to decide how much they will contribute to a public account. The ECUs contributed to the public account is returned to each group member as a payoff of 0.5 ECU (Marginal Per Capita Return (MPCR) = 0.5). At the end of each round, subjects receive information about the current and the past payoffs from the public and the private accounts. However, they are able to infer only the average play of other group members, but not specific decisions.

In the second part of the experiment, subjects play a variation of the dictator game. 10,000 additional ECUs are given to each subject as an endowment and they have the opportunity to divide the additional endowment between themselves and the charity as the recipient. Dictators are not allowed to use their incomes from the PGG for the donation purposes, thus the donation amount is between 0 and 10,000 ECUs. Furthermore, we introduce three rebate scheme treatments for the Dictator to compensate the cost of donation.

The schemes are no rebate (Baseline), exogenous rebate and endogenous rebate. In the case of a 'no rebate', the second part of the experiment is identical to a standard dictator game with charity. In the exogenous rebate scheme, a subject receives a rebate of 40% of his donation from the experimenter. The endogenous rebate scheme is similar to the exogenous one, except for the source of the rebate. Under this scheme, a subject receives the same 40% rebate for his donation, but the amount comes from the public good account of the PGG from the first part of the experiment. Hence, group members of the PGG share the cost of their rebates and the PGG and the DG payoff become correlated. Figure 4.1 summarizes the experimental procedure.

13 The Charity is 'Aldeas Infantiles SOS'(SOS Children's Villages). It is an international charity supporting needy children, and their families and communities.

Figure 4.1. Experimental procedure



The total payoff of a subject is the sum of the payoff from the PGG and the endowment that he keeps for himself in the DG. Formally, the payoff of player *i* under the scheme of no rebate is:

$$\pi_i^{NR} = \pi_i^{PGG} + \pi_i^{DG} = \sum_{t=1}^{20}\left\{e_P - c_{it} + \frac{b}{n}\sum_{j=1}^{n} c_{jt}\right\} + e_d - d_i \quad (1)$$

Here, as well as in the below, we denote e_p = endowment in the PGG, e_d = endowment in the DG, c_{it} = Player *i*'s contribution to the public account at time t, d_i = Player *i*'s donation to the charity, n = group size = 4, b = 2, MPCR = 0.5.

With the exogenous rebate scheme, the payoff of player *i* is the sum of the benefits from the PGG, the endowment kept in the DG and the rebate given exogenously.

$$\pi_i^{EXO} = \pi_i^{PGG} + \pi_i^{DG} = \sum_{t=1}^{20}\left\{e_P - c_{it} + \frac{b}{n}\sum_{j=1}^{n} c_{jt}\right\} + e_d - (1-r)d_i \quad (2)$$

where r = rebate rate = 0.4.

The payoff of player i under the endogenous rebate scheme is similar to (2), except the rebate comes from the PGG. It is given as:

$$\pi_i^{END} = \pi_i^{PGG} + \pi_i^{DG} = \sum_{t=1}^{20}\left\{e_P - c_{it} + \frac{b}{n}\sum_{j=1}^{n} c_{jt}\right\} + e_d - (1-r)d_i - \frac{r}{n}\sum_{j=1}^{n} d_j$$

$$\therefore\ \pi_i^{END} = \sum_{t=1}^{20}\{e_P - c_{it}\} + e_d - (1-r)d_i + \frac{1}{n}\sum_{j=1}^{n}\left\{b\sum_{t=1}^{20} c_{jt} - r d_j\right\} \quad (3)$$

where r = rebate rate = 0.4.

We recruited 212 economics and business undergraduate students at the University of Valencia, registered for experiments in the laboratory webpage, by e-mail. Six computerized sessions, each for a treatment, were conducted at the Laboratory for Research in Experimental Economic (LINEEX) at the University of Valencia, using z-tree (Fischbacher 2007). Each session had 36 subjects except one (BSL-LOW) that had 32 subjects.

Subjects were randomly allocated to private cubicles and the experimenter read the instruction of the first part aloud. Then subjects answered a pre-experimental questionnaire and played the PGG. Before starting the second part, the experimenter distributed the

instruction for the DG and read it aloud. Moreover she read the charity's main goal extracted from the charity's webpage. Once everybody finished making decisions, the experimenter announced the total donation made in the room and transformed the total ECUs to be passed to the Charity in Euros. The exchange rate (2000 ECUs = 1 Euro) was a common knowledge. We used the internet to make the donation to the charity. It was done in real time and participants could follow the process through a projector. A randomly selected participant was chosen to supervise the process through which the money was transferred to the charity. The average earnings were 8.5 Euros in the low endowment treatments and 19.5 Euros in the high endowment treatments. The sessions lasted for around 90 minutes.

4.3 Results

In this section we first report the relevant descriptive statistics and then analyze the effects of the rebate mechanisms and the PGG endowments on DG giving. To capture the intrinsic motivation in social behavior, we further investigate the relationship between the contribution in the first round of the PGG and the charity donation. Finally, we investigate how gender affects the decisions on charity donation.

4.3.1 Descriptive statistics

Table 4.2 reports average earnings in the PGG by treatment. Since the PGG is adopted to replicate the rebate source in the real life and the main question of this study is to investigate how the source of rebate influences individual's giving behavior, we describe briefly average earnings over treatment from the PGG. Note, however, that since the contents of the second part of the experiment were unknown to the subjects, there should not be any systematic difference between treatments with same endowments.

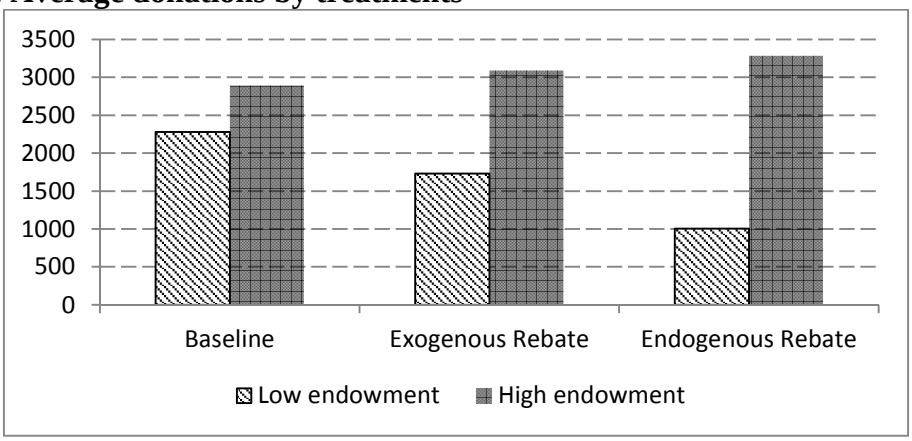
Table 4.2. Average (standard deviations) earnings in the public good game

	Baseline (No Rebate)	Exogenous Rebate	Endogenous Rebate	All
Low Endowment	2583.88 (352.81)	2625.53 (290.90)	2751.33 (456.67)	2656.26 (377.09)
High Endowment	23473.97 (2322.81)	26645.28 (4578.06)	26773.03 (3627.02)	25630.76 (3908.19)

For the case of low endowment, subjects overall earned around 2656 ECUs, and the differences in average earnings across rebate schemes are not statistically significant. Overall average earnings for high endowment is around 25630 ECUs, and average earnings between two rebate schemes, END-HIGH and EXO-HIGH are not significantly different. However one for the BSL-HIGH is significantly less than in the other two rebate schemes (BSL-HIGH vs EXO-HIGH: p-value = 0.0015, and BSL-HIGH vs END-HIGH: p-value = 0.0002) when we run Mann-Whitney tests. It might be caused by the fact that somehow there are more free riders in the BSL-HIGH treatment than in others. Furthermore, in order to compare the high and the low endowment groups directly, we scaled the earnings of the high endowment group by 1/10 and found that average earnings across all the treatments except BSL-HIGH are not statistically different. The difference in average earnings between the BSL-HIGH and other treatments, however, did not have any significant effect on any of our further analyses.

Turning to the second part of the experiment, Figure 4.2 depicts the average donations to the charity by treatment in the DG (the corresponding table is provided in the Appendix). Recall that an additional endowment was given to the dictators separately, and the earnings from the PGG could not be used in the DG. Comparing average donations between the low and the high endowment groups, we find that the donation in the high endowment group is on average higher. Furthermore, this difference becomes more prominent when rebate schemes are introduced. In the baseline treatment, the difference is 610 ECUs but it becomes 1361 ECUs and 2279 EUCs in the endogenous and exogenous rebate treatments, respectively. However, the difference is significant in a Mann-Whitney test only under the endogenous rebate treatment. (BSL-LOW vs. BSL-HIGH: p-value = 0.25, EXO-LOW vs. EXO-HIGH: p-value = 0.19 and END-LOW vs. END-HIGH: p-value = 0.0009).

Figure 4.2. Average donations by treatments



Even within the same endowment, rebate treatments play very different roles. In the low endowment group, an introduction of rebate decreases the average donations, with a higher degree of reduction of donation for the endogenous rebate scheme. The average donation in the baseline treatment is 2,278 ECUs. But it drops down to 1,729 ECUs and 1,002 ECUs with exogenous and endogenous treatments, respectively. On the other hand, rebate treatments have positive effects on the giving behaviors within high endowment. With no rebate, the average donation is 2,888 ECUs; whereas average donations go up to 3,088 ECUs and further to 3,281 ECUs with exogenous and endogenous rebates, respectively. These differences, however, are not statistically significant.[14]

From the distribution of average donations across treatments we observe that the level of endowment in the PGG and the rebate scheme in the DG can have heterogeneous effects on giving behavior. An introduction of exogenous to endogenous rebate monotonically decreases the average donation under the low endowment case; while the average giving increases with any rebate scheme under high endowment. It may be possible that the amount given to the charity is affected differently for different treatments. Next we analyze this issue.

4.3.2 Treatment effects

In this section, we estimate the effects of the rebate schemes on the giving behavior under a specific endowment level using a Tobit regression model. We employ the Tobit model to investigate how much a subject actually donates to the charity if he indeed makes a positive donation. The dependent variable in the Tobit model is the amount given to the charity. EXO and END are treatment dummies for the exogenous rebate and the endogenous rebate schemes respectively (with no-rebate treatment used as the baseline). PROFIT IN PGG is the total amount of earning from the PGG. We include this to control for any possible income effect that may arise from the first part of the experiment. FEMALE is a dummy variable depicting a female dictator, and NGO is a dummy variable indicating whether the subject self-reported to be a member of NGOs.

[14] Mann-Whitney Test summary: BSL-LOW vs. EXO-LOW: p-value = 0.84, BSL-LOW vs. END-LOW: p-value = 0.20, EXO-LOW vs. END-LOW: p-value = 0.20, BSL-HIGH vs. EXO-HIGH: p-value = 0.92, BSL-HIGH vs. END-HIGH: p-value = 0.56 and EXO-HIGH vs. END-HIGH: p-value = 0.62.

Table 4.3. Treatment effect: Tobit model

	Low Endowment		High Endowment	
	(1)	(2)	(3)	(4)
EXO	-521.47	-952.87	-394.56	-420.37
	(822.22)	(826.13)	(1061.25)	(1053.40)
END	-1778.18**	-1983.02**	39.71	-57.46
	(851.25)	(837.22)	(1051.07)	(1051.82)
PROFIT IN PGG	0.32	0.47	0.14	0.16
	(0.93)	(0.96)	(0.11)	(0.11)
FEMALE		754.98		308.28
		(709.69)		(852.59)
NGO		1726.23**		1654.91**
		(696.66)		(804.39)
Constant	730.62	-747.69	-952.40	-2656.91
	(2485.62)	(2564.65)	(2746.25)	(2915.59)
# of Obs.	104	103	108	108

Standard errors are in parentheses; ***, ** and * indicate significance at the 1%, 5%, and 10% level.

Table 4.3 includes the results of Tobit regressions for the two endowment levels with and without controlling for the demographic variables. The rebate schemes play very different roles in determining the giving behavior. For exogenous rebate, the price of giving 1 ECU becomes 0.6 from 1, hence in line with existing studies showing that giving is price elastic, one would expect the donation to increase.[15] We, however, find that exogenous rebate scheme does not have a significant effect on the charity amount. Although uncommon, this phenomenon is not unheard of. Marcuello and Sala (2001) among others also found no crowding-out effect (among Spanish subjects), which in spirit is similar to our result.

More interestingly, the endogenous rebate scheme has significant effects, but only under the low endowment treatments. This scheme decreases the amount of charity by 1,778 and 1,983 ECUS (17.8% and 19.8% of the endowment given). This may come from three channels; the preference for the rebate system, warm glow, and money perception. First, the donors realize that an endogenous rebate scheme will essentially be cross subsidized from

15 There is a stream of literature (Feldstein and Clotfelter, 1976; Feldstein and Taylor, 1976; Clotfelter, 1985; Feenberg, 1987; Randolph, 1995; O'Neil et al., 1996; Joulfaian, 2000; Tiehen, 2001; Auten et al., 2002; Eckel and Grossman, 2003, 2006, 2008; List 2011) that investigate similar issues.

another service served in the society, and they might not be favorable to this idea. They may also anticipate that some of the other dictators will donate some amount that will reduce the total PGG payoff, and as a result their own total payoff. To compensate for the same they reduce their own giving. Second, part of the warm glow effects gets reduced when the rebate scheme is introduced and the donors donate less due to the same. Third, although the monetary income from the PGG is controlled in regressions, we expect that the level of income from the PGG has a negative effect on individuals' money perception and the amount of donation becomes sensitive to the income level. Wiepking and Breeze (2012) summarize the effects of money perception on giving behavior, asserting a negative relationship between financial income and money perception (in terms of retention and inadequacy). They show that those who feel more financially insecure are willing to donate less. In our setting of the PGG a subject is given either a low or a high endowment and, as a result, a subject with a low endowment earns significantly less. Moreover the average earning under low endowment is significantly lower than the endowment given for the dictator game. This may cause the subjects in the low endowment to become cautious and more worried about their financial situation. In END-LOW, all of these three effects work in the negative direction and as a result the charity donation declines. However, in END-HIGH, although the first two sources impose negative effects, money perception brings in a positive effect on giving. Consequently the variable END turns out to be not significant in the regressions under high endowment.

These results may provide directions regarding employment of subsidy schemes for charity donation across income levels. If an endogenous rebate scheme is implemented to encourage charity giving regardless of income level, it might not only fail to increase the charity amount, but may also face a reduction in donation from the lower income donors.

Focusing on the demographic variables, there was no gender difference on the overall amount given. However subjects who reported to be members of NGOs are more likely to donate a larger amount compared to others. This may happen due to two reasons. First, the membership might indicate an intrinsic altruistic nature of those subjects. Second, it might be that subjects try to justify their behavior and report the NGO membership accordingly. Hence, it will be important to investigate the relationship between revealed social preference and corresponding amount given for the charity.

4.3.3 Social preference type and charity donation

The PGG in the first part of the experiment also allows us to further analyze the relative social preferences of the subjects. If a subject is relatively more other-regarding compared to his group member(s) in the PGG, he might contribute more than others. This, in turn, may reflect his behavior in the DG. To capture this mechanism, we rank the subjects in ascending order of their public good contribution in the very first round (in case of a tie, we compare their contributions in the next round and decide upon their ranks). Then we categorize the subjects as two types: social and selfish. Social type includes subjects who contributed the highest or the second highest amount in their group. People who are not social type are defined as selfish type.

We choose the contribution in the first round for our analysis, because we believe that it is a robust indicator of other-regarding preferences. Possible other indices such as the total contribution or the average contribution in the PGG have two opposite effects on charity donation. A subject contributing most in the PGG may be the most other-regarding person. This should naturally translate into higher donation in the DG. However, high contribution in the PGG may result in very low earnings in the PGG part for him. As a result, he might not be able to afford to donate a large amount in the DG. Since these two factors work in the opposite directions, the net effect of the total PGG contribution will be unclear.

Figure 4.3 and Figure 4.4 show the average donations of social and selfish types by rebate schemes for the two endowment levels. In Figure 4.3, regardless of the type, the introduction of any rebate scheme tends to decrease the average donations by group. This is in line with the pattern of the low endowment case we discussed in the previous section. Also, the average donations by social and selfish types are not significantly different within the treatment. However in Figure 4.4, where subjects have a high level of endowment in the PGG, the average donations by type vary extensively. A movement from exogenous rebate to no rebate to endogenous rebate scheme has opposite effects on the social and on the selfish types. Under the exogenous scheme, the average donation by the social type is lower than the one by the selfish type, but the difference is not significant. On the other hand, the social type donates significantly higher amounts than the selfish ones in the baseline and in the endogenous scheme. (Mann-Whitney Test BSL-HIGH Social vs. Selfish: p-value = 0.057 and END-HIGH Social vs. Selfish: p-value = 0.033).

Figure 4.3. Average donation by player type and rebate scheme: Low endowment

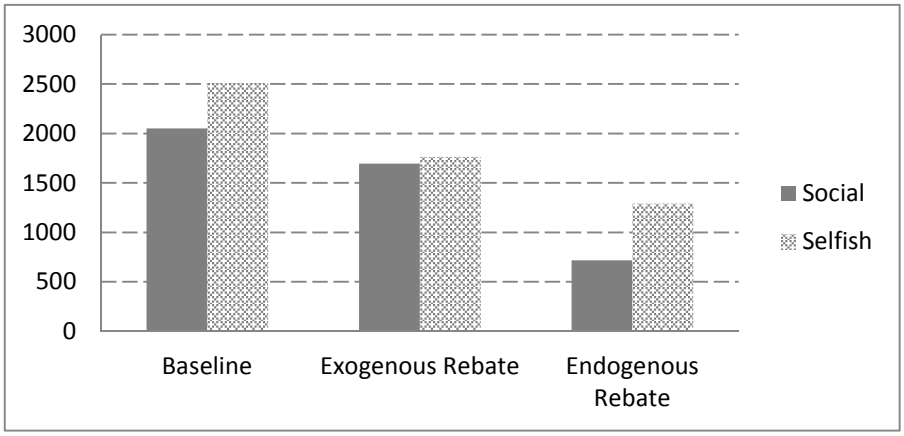
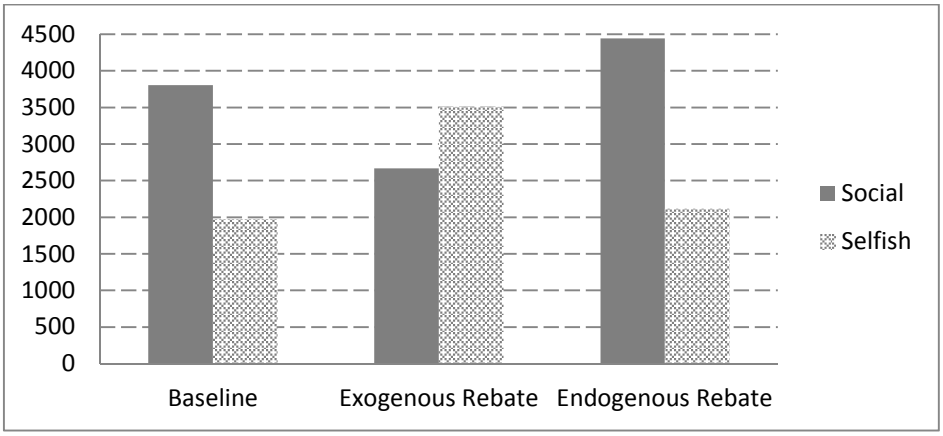


Figure 4.4. Average donation by player type and rebate scheme: High endowment



We further investigate the effects of the other-regarding preference defined by social types on the actual amount donated for each rebate treatment using a Tobit model. The results are summarized in Table 4.4. In the analyses reported, the total PGG earnings and demographic variables are employed as controls. The results remain qualitatively same even otherwise.

We introduce a dummy variable SOCIAL TYPE taking value 1 if the subject is of social type. This variable turns out to be highly significant and positive while regressing on the amount of donation in the BSL-HIGH and END-HIGH treatments. Social type subjects in the BSL-HIGH treatment are willing to donate around 2,413 ECUs more than the selfish type subjects. Similarly in the END-HIGH treatment, the donation by the social type is on average 3,043 ECUs higher than the one by the selfish types. However, social type does not have any significant effect on the charity donation in other treatments. Hence, this result reaffirms that other-regarding preference in the PGG does not matter in determining the donation level in

the case of low endowments. In the case of high endowment, it matters except when the rebate scheme is exogenous.

We believe that these results are mainly driven by a combination of money perception effect and other-regarding preference. As discussed in the previous section, the low income level may be prone to the feeling of preservation and worry about financial status. When the endowment is low in the PGG, the effect of money perception dominates the other-regarding preference. Thus the coefficient of social group is insignificant across all the treatment with the low endowment. However if the endowment in (and as a result, income earned from) the PGG is high enough, then the other-regarding preference becomes prominent and has significantly positive effects on the giving.[16]

Non-parametric results discussed below and regression results in Table 4.4 confirm that when there is no rebate or the rebate is endogenous in the high endowment, the social type is more generous than the selfish one. Interestingly the EXO-HIGH treatment shows a different pattern from BSL-HIGH and END-HIGH treatment. In the EXO-HIGH treatment, the total earnings in the PGG become significant rather than the other-regarding preference. It is possible to explain this in terms of the price elasticity of giving. Since the price of altruism is the lowest with the exogenous rebate scheme, it increases the average donation even for the selfish subjects. Hence the variable social turns out to be not significant in the regression.

The coefficient of FEMALE is significant and positive in both END treatments, i.e., females are more generous under the endogenous rebate scheme. Since the price of giving is lower under the endogenous scheme (compared to baseline), at a first glance, this result is in contradiction with the ones by Andreoni and Vesterlund (2001) who find females to be more generous when the price of giving is higher. But, since in the endogenous scheme the rebate comes from the PGG account, the real price of giving depends on the perception about how much everybody is giving in the DG. Hence, this structure is not appropriate to compare to the Andreoni and Vesterlund (2001) analysis. This result, however, is comparable to the ones in Rigdon and Levin (2011) who show that females tend to make higher donations than males when they have to perceive about the possible giving by others.

16 This may also be viewed through the lenses of Impure Altruism. See Andreoni (1990) for the theory, or Chowdhury and Jeon (2014) who discuss the mechanisms through which this effect may take place.

Table 4.4. Effects of social preference: Tobit regressions

	BSL-LOW	EXO-LOW	END-LOW	BSL-HIGH	EXO-HIGH	END-HIGH
SOCIAL TYPE	54.09	-57.47	-682.14	2,412.89***	58.03	3,042.62***
	(1,816.87)	(1,408.63)	(762.36)	(665.87)	(2,005.26)	(799.36)
PROFIT IN PGG	3.14	-2.21	-0.39	-0.03	0.35*	0.07
	(1.70)	(2.19)	(0.54)	(0.16)	(0.16)	(0.12)
FEMALE	823.72	-265.74	1,793.64*	-2,197.70	-1,497.85	2,741.61**
	(1,474.55)	(998.54)	(822.81)	(2,137.05)	(1,841.96)	(781.47)
NGO	4,375.05**	19.56	548.42	1,190.40	3,730.45*	-754.82
	(1,428.97)	(1,442.19)	(538.92)	(1,585.82)	(1,369.59)	(699.04)
Constant	-8,864.03	6,979.44	379.17	3,030.21	-8,062.47	-1,567.13
	(5,589.01)	(6,449.13)	(1,742.68)	(5,969.90)	(5,541.27)	(3,412.46)
# of Obs.	32	35	36	36	36	36

Standard errors are in parentheses; ***, ** and * indicate significance at the 0.1%, 1%, and 5% level.

4.4 Discussion

The existing experimental literature on charity rebate rarely considers the source of the rebate budget, and assumes it to be exogenous. In this study we investigate the effects of the sources of charity rebate on donor behavior. We adopt a two part game consisting of a public good game in the first part and a dictator game with real charity in the second. Different rebate schemes that are funded either by the experimenter or from the public account in the public good game are introduce in the dictator game to distinguish between the exogenous versus endogenous source of the rebate. To incorporate possible interaction with income effect, we employ either a high or a low endowment level in the public good game.

The results show that giving behavior crucially depends both on the type of rebate scheme and the endowment amount in the public good game. Subjects having a low level of the endowment decrease the amount given to the charity when the rebate is budgeted endogenously. This result may be driven by three factors; preference for the rebate system, warm glow and money perception. The structure of endogenous rebate scheme reduces both the willingness of donation and warm glow, and the low level of the endowment brings up the response of retention and inadequacy and as a result the donation is reduced. On the other hand, in the high endowment treatment under endogenous scheme, other-regarding preference becomes salient and has significantly positive effects on giving. Furthermore, the exogenous rebate scheme (the standard treatment in experimental literature) does not have

any significant effect on charity donation across endowment levels. Our results also indicate gender difference when the endogenous rebate was implemented. Since dictator's decision is endogenous with the expectation for group members' donations, female subjects are more generous than males when the rebate scheme is endogenous.

It is important to note that this experiment was run in Spain, which has a specific donation culture different from the United State or the United Kingdom where most of experiments were run earlier. Spain is ranked twelfth in terms of 2010 GDP, whereas the United State and the United Kingdom are first and sixth respectively. However, according to the World Giving Index 2010, Spain is ranked ninety first in terms of the percentage of donating money to charity, volunteering time, and helping a stranger, whereas the United Kingdom and the United States are the fifth and the eighth. If we consider only the percentage of people who make a charity donation, twenty five percentage of the population in Spain had given to a charity but sixty percentage and seventy three percentage of people in the US and the UK had given. Since our experiment was run in 2010, before Spain was officially in the recession it is hard to believe that our results face any direct consequence of the recent economic crisis in Spain. Thus the donation culture may be a factor in explaining some of the current results that are not in line with the existing literature.[17]

The fact that subjects with different endowment react differently with endogenous rebate scheme is highly relevant in designing policies. These results show that policy makers should be careful in implementing a blanket rebate scheme to encourage charity donation. Current rebate scheme (equivalent to the endogenous rebate) may not have any effect to increase donations among the high income people, but it might cause a decrease in the donation from the lower income people. Moreover if an individual is self-regarding, a tax deduction for charity donation may have positive influence on the giving and might result in an increase in donation. However the effects of the endogenous rebate scheme may be different across the countries due to the donation culture, the distribution of income and altruistic types. Hence, a policy maker needs to take account of these as well.

There are many ways this research can be extended. A real effort wage scheme can replace the public good game. It is also possible to introduce tax to bring this frame closer to field observations. A mix of exogenous and endogenous rebate scheme or a mix of high and low endowment (which are prevalent in some cases in real life) can also be considered. We hope to build upon the current study and consider these issues in the future.

[17] See Marcuello and Salas (2001) for further discussions in related issues.

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Chapter 5

Identity and Group Conflict[18]

5.1 Introduction

Conflicts among groups are ubiquitous in daily life. At any level of society, groups engage in violent or non-violent conflicts and group members expend costly resources for their group in order to gain material benefits (e.g., spoils of a war), or to achieve social recognition, or to avoid a loss. Examples include racial conflict, conflict relating to language, religion or culture, political competition, team sports and collective rent-seeking, to name a few. Such group conflicts are costly for the individual group members and often also costly for society. The earliest documented human conflict resulting in fatalities was between 14,000 BC and 12,000 BC in Nubia (present-day Sudan) in which at least 59 people died (Kelly, 2005). Almost 15,000 years later, the conflict between the Hutu and the Tutsi populations in Rwanda, Burundi and Uganda in the last century claimed more than a million lives. Numerous conflicts are recorded in between across time, place and culture. Even when fatalities are not involved, group conflicts often create costs such as long run tension and hatred, the expense of resources on unproductive defense activities etc.

It is, hence, not surprising that researchers across disciplines have spent a considerable amount of effort to explore the reasons leading to a conflict, and possible ways to eradicate them. Hirshleifer (1995), while discussing the possible reasons for a war, inquires:

> *"Is war mainly due to hatred and ingrained pugnacity (preferences)? Or to the prospects for gain at the expense of weaker victims (opportunities)? Or, is war due to mistakes in evaluating others' motives and capacities (perceptions)?"*

While the latter two points are studied in detail in Economics, the first issue remains relatively unexplored. In this study we consider a very specific dimension of preference - identity - which may contribute to the initiation and escalation of conflict.

The Oxford Dictionary defines Identity as "the characteristics determining who or what a person or thing is". It is plausible that identity can be a key ingredient in defining a group,

18 This Chapter is a joint work with S. M. Chowdhury and A. Ramalingam.

and, also a basis for conflict. In his seminal work, Sen (2007, Ch. 2) introduces the relationship between the salience of a particular dimension of identity and conflict. He hypothesizes that certain situations when a particular dimension of identity, say religion or race, becomes salient can engender conflict and even lead to its escalation. He further differentiates between 'identity' and 'classification'. Sen (2007, pp. 26) argues that a person's race can be his identity, but his shoe size can only be interpreted as a classification. A classification can certainly be cheap, but one's identity is not. As a result, when the identity or classification determines the concept of a group, we would observe two phenomena. First, the salience of identity increases conflict. Second, the effect of an identity in conflict is more than the effect of a classification. To the best of our knowledge, there is no existing applied study, using field or laboratory data to investigate the issues of the effects of identity on conflict as described by Sen (2007). In this study we run a laboratory experiment to explore the issue of the relationship between identity and conflict. We ask the following questions: Is there empirical support for Sen's hypotheses about the effects of identity on conflict? Does real identity initiate and instigate conflict more than a classification (a minimal identity)? If so, then is the effect of identity symmetric across agents?

The relationship between identity and several other behavioral outcomes, however, is well examined. Sherif et al. (1961), Turner (1978), and Tajfel & Turner (1979) among others investigate this broad topic in Psychology. Sen (1985) himself discusses the effects of identity on coordination, and Fearon & Laitin (1996) and Hardin (1997) explore similar issues in Political Science. In economics, however, the exploration of the effects on identity starts with the seminal study by Akerlof & Kranton (2000). Since then, a series of follow up studies have emerged. Robinson (2001), Akerlof & Kranton (2002, 2005, 2008, 2010), and Basu (2005, 2006) explore the theoretical background to the issues related to identity; whereas Fershtman & Gneezy (2001), Goette et al. (2006), Deck et al. (2009), Chen & Li (2009), Hargreaves-Heap & Zizzo (2009), Chen & Chen (2011), Chen et al. (2014), Kranton et al. (2013) pursue field and experimental studies in related areas. Chen & Chen (2011) employ a minimal identity paradigm that can be compared with the idea of 'classification' as explained in Sen (2007, pp. 26). Although hypotheses relating to identity and conflict as in Sen (2007) are often discussed in various studies, evidence related to the same is yet to be analyzed in the literature.

Existing field studies or laboratory experiments in social psychology and in economics have shown that an introduction of real or minimal identity elicits in-group out-group discrimination (see, for example, a majority of the early studies in social psychology as

covered in Tajfel & Turner, 1979; and Ahmed, 2007, 2010; Benjamin et al., 2009, 2010; Deck et al., 2009; Hargreaves Heap & Zizzo, 2009; Klor & Shayo, 2010 from economics). It has further been shown that group identity can be formed in terms of inequality (Esteban et al., 2012a, b), and that a group identity can be exploited to manage diversity (Eckel & Grossman, 2005) or invoke stereotypes (Shih et al., 1999). Although a few recent theoretical studies explore the relationship between conflict and identity (see Smith, 2012; Kolmar & Wagener, 2012) using conflict models, the existing experimental literature employs mixed-motive games such as prisoner's dilemma (Bornstein, 1992) or social dilemma situations such as public good games (Hugh-Jones & Leroch, 2014) or allocation games (Kranton et al., 2013) for the same. These games are appropriate to examine *conflict of interests* in agents, but are not able to replicate conflict situations in which groups compete with each other and the group members expend costly resources in order to gain something or to avoid losing something. Hence, it still remains to be seen if, in a controlled setting, the salience of a particular identity increases 'Conflict'.

While discussing this issue, Basu (2005) famously mentions that:

> *"Our sense of self, or identity, can influence our social, economic and political behavior. We hear of religious wars, ethnic tensions and the coming clash of civilizations but we do not hear of friction between short and tall people, or between the bald and the hirsute"*.

This, in spirit, is in the same line with the differentiation Sen (2007) makes between the effects of identity and classification. Rightfully, since there are various dimensions of identity that can be considered as the focus of conflict and not all identities may have the same effect, it is important to narrow down our focus on some specific dimension of identity. In this respect Young (1982) argues that:

> *"(r)ecent history suggests that the major pattern of conflict cohere around two organizing principles: class and ethnicity"*.

Scholars have found that race or ethnicity remains one of the most important factors in various type of social conflict across the globe (see Esteban & Ray, 2012a, b; Horowitz, 2000; Humphreys et al., 2002; Fearon & Laitin, 2000; Reynal-Querol, 2002). Hence, we consider race as the focal real identity in our study. To control for the effects of any arbitrary identity on conflict against the real (racial) identity, and to test Sen's idea about the effects of 'classification', we also include a minimal identity in our study.

We study a group contest with a group specific public good prize. In the game group members may expend effort for their group and the 'group effort' influences which group

wins the prize. Irrespective of the outcome of the contest, all agents lose their efforts. A very preliminary version of a similar game was included in Rapoport and Bornstein (1987). However, the theoretical background of this type of games is provided first by Katz et al. (1990), and experimental evidence by Abbink et al. (2010). We use the theoretical structure of Katz et al. (1990) and the experimental procedures as in Abbink et al. (2010), but introduce the concept of identity and classification in the game.

We employ three treatments. In the baseline treatment two three-player groups – one consisting of East Asians, and the other consisting of Whites (Caucasians) – engage in a group contest. But no information about the group composition is revealed. In the classification treatment or minimal identity treatment the same experiment is run, but each group is arbitrarily given a different color code. In the real identity treatment the racial compositions of the groups are revealed.

We find that in all three treatments subjects expend significantly more effort than the Nash equilibrium prediction. However, efforts are significantly higher in the real identity treatment than in the other two treatments. Further, this comes both from a reduction in free-riding (expending zero effort), and an increase in positive efforts. No particular racial group behaves differently from each other. But, regardless of race, females expend significantly higher effort than males, and the difference widens when real identity is introduced.

We find support for Sen (2007)'s argument that making a real identity salient initiates (in terms of the reduction of free-riding) and increases (in terms of higher positive effort) conflict. Moreover we find that a minimal identity (classification) may increase conflict but the increase is not as large as the effects of a real identity – further supporting Sen's hypothesis. Finally, exploring gender differences, we conclude that the effect of identity, at least in this induced effort set-up, is asymmetric across demographic groups.

The rest of the paper is organized as follows. The second section provides a theoretical background of the model of conflict we use and the third section explains the design of the experiment. The fourth section presents the results and the final one concludes.

5.2 Theoretical background

We study behavior in a group contest in which multiple groups compete for a prize. Group members can expend costly effort, and the sum of all group members' effort constitutes the group effort. The group effort determines the group's likelihood of winning the prize. The probability of winning is determined through the well-known Tullock (1980) contest success

function (CSF).[19] In particular, a group's probability of winning the prize is equal to the group's effort divided by the total group effort of all competing groups. Here we present the theoretical model of such a group contest.

Let the number of groups competing for the prize be n (≥ 2) and the number of (risk-neutral) group members in each group be m (≥ 2). Each player i in each group g ($= 1, 2, \ldots n$) has the same endowment, $e > 0$, from which he/she can expend effort $x_{gi} \in [0, e]$. Any effort expended by a member of group g increases the likelihood that group g will win the prize. Any endowment not expended remains with player *i*.

The group effort of group g, X_g, is the sum of the effort expended by all members of group g, i.e., $X_g = \sum_i x_{gi}$. Let the total group effort by all groups competing for the prize, i.e., by all nm players, be $X = \sum_g X_g$. The probability with which group g wins the prize, p_g, is determined by a lottery contest success function (Tullock, 1980) and is given by

$$p_g = \begin{cases} X_g/X & \text{if } X \neq 0 \\ 1/n & \text{otherwise} \end{cases}$$

The prize is a group-specific public good prize in the sense that each member of the winning group earns the prize regardless of the amount of his/her effort. Let the prize evaluated commonly by each individual player be denoted by $V > 0$. The losing groups receive a prize of 0. The expected payoff of player *i* in group *g*, is given by

$$\pi_{gi} = p_g V + (e - x_{gi})$$

where the first term is the expected value of the prize and the second term is the part of the endowment that player *i* kept with him/her. From Katz et al. (1990) it can be shown that there exist multiple equilibria and individual equilibrium efforts cannot be characterized. However, in any equilibrium, the group effort for each group is

$$X^* = V(n-1)/n^2$$

In a finitely repeated game, the equilibrium prediction is that each group will expend X^* in each repetition of the stage game.

5.3 Experimental Design

The experiment consists of a finitely repeated contest between two groups of three subjects each, and the core design is very similar to the design implemented in Abbink et al. (2010). In

19 We employ a Tullock (1980) type CSF for several reasons. First, it is shown by Munster (2009) that this CSF is derived under a set of reasonable axioms. Second, it is widely used in modelling various situations such as rent seeking, war, innovation tournament, legal battles etc. Finally, since a pure strategy equilibrium exists, this CSF allows us a greater flexibility in designing an experiment focused on issues other than testing theory.

the experiment, each member of each group is endowed with 60 Experimental Currency units (ECUs) which he/she can allocate to a group account or to an individual account. Once all individuals make a decision, the Tullock contest success function is used to determine the winner. Each member of the winning group is awarded 40 ECUs. Subjects are then informed of the total ECUs in their group account, the total ECUs in the other group's group account, which group has won the prize and their individual earnings in ECUs from that round.

This contest is repeated for 20 rounds. Subjects cannot use past earnings in future rounds and receive a fresh endowment of 60 ECUs in every round. At the end of the session, each subject is shown his/her individual earnings in ECUs in each of the 20 rounds. Subjects are then paid for the same 5 rounds chosen randomly at the rate of 25 ECUs to 1 GBP. In addition, each subject receives a 2 GBP show-up fee. In terms of the theoretical model presented in the previous section, the parameters of the contest in our experiment are $n = 2, m = 3, e = 60$ and $V = 40$. The equilibrium prediction in our experiment is thus that group effort for each group is 10 ECUs, i.e., $X^* = 10$, in each of the 20 rounds.[20]

Each session includes 9 subjects from each of two racial cohorts: East Asians and Whites (Caucasians). Within each cohort, subjects are randomly and anonymously assigned to groups of three. Two groups – one from each racial cohort – are then randomly and anonymously paired with each other. Thus all three members of a group are from the same racial cohort and the two competing groups are composed of subjects from the two different racial cohorts. The matching within and between groups remains fixed throughout a session. A subject can participate in only one session.

The experiment has three between-subject treatments and four sessions in each of the treatments. In the *Baseline* (no identity) treatment two three-player groups – one consisting of East Asian subjects, and the other consisting of White subjects – are matched to play the group contest, but no information about the group composition is revealed. In the instructions, phrases such as 'your group' and the 'group you are matched with' are used. In the *Color* ('classification' or 'minimal identity') treatment the same experiment is run, but each group is arbitrarily given a different color code – either Green or Blue. In two sessions, the East Asians are called the Green groups while Whites are called the Blue groups. To check if the artificial color labels have an impact on behavior, the assigned color labels are reversed in two further sessions (we call them the *Green-Blue* and the *Blue-Green* sessions).

20 Note that the endowment given to the subjects are higher than the Nash equilibrium amount, and the prize value. As shown by Baik et al. (2014), endowment may have an effect on bid level. But, since the endowment is the same across treatments and we are interested in treatment effects, this does not affect our analysis.

At the beginning, the instructions in this treatments mention which color group the subject belongs to, but all the remaining parts remain the same as in the *Baseline* treatment. In the *Race* (real racial identity) treatment the racial compositions of the groups are revealed. They are informed at the start of the instruction that everyone in their group is of the same race and that everyone in the other group is of the other race. The remaining parts of the instruction stay the same.

The experiment involved a total of 216 student subjects from the University of East Anglia, UK recruited through the online recruiting system ORSEE (Greiner, 2004) that enabled the selection of participants according to their reported race.[21] No subject had any prior experience in participating in a contest experiment or in an identity experiment. All sessions were computerized using z-Tree (Fischbacher, 2007). Each session consisted of 9 East Asian and 9 White, i.e., a total of 18 subjects. At the beginning of each session, instructions were handed out and were read aloud by an experimenter. Subjects were required to answer a questionnaire before the experiment began. Each session lasted approximately 60 minutes and average earning per subject was 16 GBP including a show-up fee of 2 GBP. The instructions can be found in the Appendix.

5.4 Results

Note that every treatment has 72 subjects. However, since players receive feedback on group efforts of their group and of the competing group, each competing pair of groups forms an independent observation. As each set of group pair consists of (3+3=) 6 subjects, we have 12 independent observations for each treatment.

To test if a particular color has an effect on behavior in the Color treatment, we run a random-effect regression of individual efforts on a constant, one-period lagged own effort, one-period lagged effort of the rival group, a time trend and a treatment dummy. The results indicate that there are no significant differences in behavior between these two color labels under Color treatment (p-value for the treatment dummy = 0.372). Hence, in all our analyses, we pool data from the *Green-Blue* and the *Blue-Green* sessions under the Color treatment.

21 The East Asian subject pool consists of individuals from China, Hong Kong, Macau, Taiwan etc. whereas the White subject pool consists of individuals from Canada, France, Germany, UK, USA etc. We also ensured that ethnically the subjects are indeed White or East Asian (e.g., British students of Indian origin are excluded).

5.4.1 Group-Level Analysis

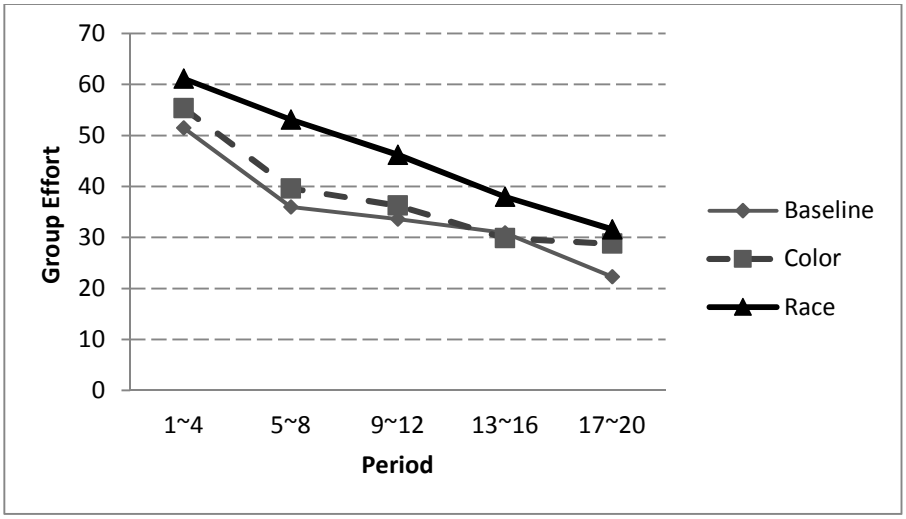
We first investigate if there are differences between the treatments at the group level before moving to an individual level analysis of the reasons for any treatment differences. Table 5.1 presents summary statistics of the mean (averaged over all 20 rounds) per-round group effort by competing pairs of groups. One can immediately observe that the average effort in any treatment is higher than the effort predicted by the Nash equilibrium. Wilcoxon signed-rank tests confirm this result (p-value < 0.000 for all treatments). This, however, only reiterates the well-known robust phenomenon that overdissipation, i.e., expending more effort than the Nash prediction, occurs in this type of contest experiments (Dechenaux et al., 2012). A more interesting observation arises when we compare the group efforts across treatments. Mean group efforts (and standard deviations) over all 20 rounds shows a monotonic increase from Baseline to Color to Race.

Table 5.1. Mean (St. Dev.) of Competing group pairs' Efforts

Treatment	Baseline	Color	Race
Average	34.869	38.006	46.008
Standard Dev.	(13.161)	(9.607)	(17.401)

To understand whether the observations from Table 5.1 are general or due to a concentration in some particular periods or particular effort range, we plot the mean group efforts over periods (Figure 5.1) and over the effort range (Figure 5.2) in each treatment. For all treatments, overall efforts decrease over time, but still stay over the equilibrium effort. Efforts are also distributed over the whole effort range.

Figure 5.1. Mean group effort over time by treatment



The levels of mean group efforts and their trajectory as shown in Figure 5.1 show that group efforts are higher than the Nash prediction (=10) in all treatments. However, while effort is decreasing over time in all treatments, efforts in the Race treatment are *always* higher than efforts in the Baseline or Color treatments. The comparison between the Color and the Baseline treatments, however, is not that obvious. Efforts in the Color treatment remain higher than, although very close to, the efforts of the Baseline treatment.

Figure 5.2. Distribution of group effort by treatment

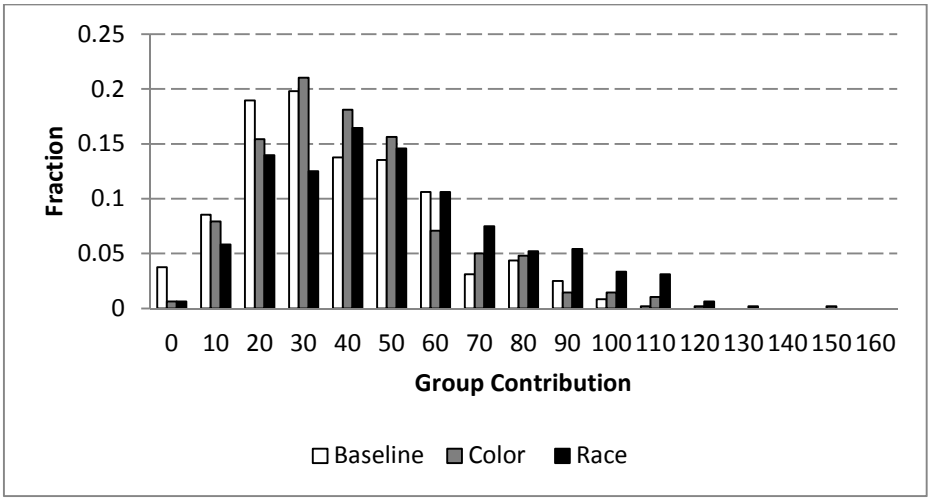


Figure 5.2 provides further indication that the effort levels are higher in the Race treatment. The figure shows that, while the efforts are distributed over the whole range, the distribution of group efforts is shifted to the right in the Race treatment relative to the Baseline or the Color treatment; the observed fraction of low efforts is greater in the Baseline treatments while the observed fraction of higher efforts is greater in the Race treatment. We next test whether the differences noted above are statistically significant with a Kruskal-Wallis test. It confirms that the efforts distributions are different in different treatments (p-value < 0.001).

To further examine treatment differences and direction of the difference, we run a panel random effects regression that uses multiple observations for each group, one for each period. The dependent variable is group *i*'s effort in period *t*, and the independent variables are two treatment dummies for Race and Color. We also control for the group's own effort in the previous period, the other group's effort in the previous period and a time trend captured

by period (1 to 20). We estimate robust standard errors clustered on independent competing pairs. The equation below presents the regression estimates.[22]

$$\text{Group effort}_{it} = \underset{(2.340)}{10.811^{***}} + \underset{(1.646)}{3.088^{*}}\ \text{Race} + \underset{(1.311)}{0.747}\ \text{Color}$$
$$+ \underset{(0.058)}{0.591^{***}}\ \text{Group effort}_{it-1} + \underset{(0.051)}{0.156^{***}}\ \text{Rival's Effort}_{t-1} - \underset{(0.085)}{0.332^{***}}\ \text{Period}$$

No. of obs. = 1368. No. of groups = 72. No. of competing group-pairs (clusters) = 36.

The regression confirms the observations made above. The dummy for the Race treatment is positive and significant (p-value = 0.061) but the dummy for the Color treatment is not; groups expend about 3.1 ECUs more per period in the Race treatment than in the Baseline treatment. This gives our first result as follows.

Result 1: *Group* efforts *are higher in the Race treatment than in the Baseline treatment. But there is no difference between group efforts in the Color and in the Baseline treatment.*

Since efforts are contributed in the contest in order to overcome the opponent's efforts and to win the prize, the efforts can be used as a measure of the level of conflict. Result 1 thus essentially confirms both the hypotheses put forth by Sen (2007) at a group level. First, introducing a real identity increased the level of conflict in a group. Second, introducing a classification did not affect the level of conflict significantly.

It can also be observed that group efforts are positively correlated to lagged own and opponent group efforts, and are negatively correlated to time trend. These confirm earlier findings (Dechenaux et al., 2012) that group efforts decline over time and that a group's efforts are increasing in their own past efforts and in those of the competing group.

5.4.2 Individual-Level Analysis

We next explore the reasons for observing higher efforts in the Race treatment. This can happen due to two reasons: either the subjects are free-riding less (deviating from expending zero efforts) under the Race treatment, or they are expending more effort in general. To investigate this, we need to analyze the effort behavior at an individual level. We are also interested in understanding whether the results, outlined in Result 1, are true across demographic groups. To examine this, we explore individual behavior separately for each of the two racial groups, and for male and female subjects.

22 In this equation, as in following equations and tables, ***, **, and * respectively indicate significance at the 1%, 5% and 10% level. Figures in parentheses are robust standard errors.

We first explore the extent of free-riding (zero efforts) by individual players in each treatment. Note that, each subject can free ride between 0 and 20 times in the whole experiment, i.e., once in each round. Table A.2 (in the Appendix) and Figure 5.3 present the distribution of the number of instances of free-riding by subjects in each treatment. Basically, the X-axis in Figure 5.3 shows the number of times a subject can possibly free ride, and the Y-axis shows the corresponding frequency. See from Figure 5.3 that black and grey bars are higher than white bars for the zero free-riding category, i.e., inducing either 'identity' or 'classification' reduces the number of absolute free riding. To put it another way, introducing either a real or a minimal identity increases engagement in conflict.

Figure 5.3. Incidence of free-riding by treatment

However, it also seems that severe free-riding, defined as free-riding for over half of the possible 20 periods, is greatly reduced in the Race treatment. To test if this decline in severe free-riding is significant, we estimate an *individual-level* ordered probit regression where subjects are categorized into 4 types in terms of the number of incidences of free-riding; category 0 (zero free-riding), category 1-5, category 6-10 and category 11-20 (severe free-riding). The dependent variable is an ordered categorical variable capturing this feature. The independent variables are treatment dummies, and controls for race and gender. The regression is shown below and the marginal effects are provided in Table 5.2.

$$\text{Free riding}_i = \underset{(0.195)}{-0.348^{*}\ \text{Race}_i} \ \underset{(0.162)}{-0.126\ \text{Color}_i} \ \underset{(0.157)}{-0.041\ \text{East Asian}_i} \ \underset{(0.185)}{-0.201\ \text{Female}_i}$$

No. of obs. = 216. Pseudo R-Sq = 0.0104

Table 5.2. Marginal effects of identity treatments per category

# Free-ride	0	1-5	6-10	11-20
Color	0.040	0.004	-0.014	-0.031
	(0.051)	(0.007)	(0.018)	(0.040)
Race	0.119*	-0.004	-0.039*	-0.076*
	(0.068)	(0.014)	(0.023)	(0.043)
E. Asian	0.014	-0.000	-0.005	-0.009
	(0.053)	(0.001)	(0.018)	(0.035)
Female	0.068	-0.001	-0.022	-0.045
	(0.062)	(0.006)	(0.019)	(0.043)
# of Obs.	216	216	216	216

Note in the table above that the coefficient of Race for category 0 is positive and significant whereas for category 11-20 it is negative and significant. It implies that the introduction of a real identity significantly increases conflict participation (increases the likelihood of being in the 0 category), as well as reduces severe free-riding (decreases the likelihood of being in the 11-20 category). This is not the case for the Color treatment. It also confirms that the decline in severe free-riding in the Race treatment is not driven by any particular racial or gender group; as the East Asian and Female dummies are not significant.

The above results suggest that participation in the conflict, in terms of reduction of free-riding, increases with real identity – across racial or gender groups. This is our second main finding and is summarized in Result 2.

Result 2: *The incidence of severe free-riding is lower in the Race treatment than in the Baseline treatment. Color treatment does not show such difference.*

The analysis presented above has not yet considered effort levels across subject demography. In Table 5.3 we present means and standard deviations of individual efforts per period for male, female, East Asian and White subjects.

Table 5.3 shows that individual efforts are higher in the Race treatment than in the Baseline (15.336 ECUs vs. 11.669 ECUs). Overall, the effort in the Color treatment is not much higher (12.669 ECUs) than in the Baseline. The table also suggests that higher efforts in the Race treatment are driven mainly by higher efforts by female subjects. Efforts of female subjects increase from 11.718 ECUs in the Baseline treatment to 18.265 ECUs in the Race. Females still expend more effort in the Color treatment (14.184 ECUs) than the Baseline, but the increment is not as vivid. Males do not show such behavior. Furthermore,

the increase in efforts in the Race treatment is not very different between the two racial groups. Whereas East Asians increase efforts marginally for both types of identities, Whites increase efforts only in the real identity treatment but not in the minimal identity.

Table 5.3. Mean (St. Dev.) Individual Efforts per period separated by gender and race

	Baseline	**Color**	**Race**
Male	11.523 (11.599)	11.313 (11.655)	12.407 (11.307)
Female	11.718 (11.229)	14.184 (13.312)	18.265 (13.814)
White	11.539 (11.454)	11.510 (11.466)	14.788 (12.269)
East Asian	11.707 (11.367)	13.828 (13.443)	15.885 (13.592)
All	11.623 (11.407)	12.669 (12.543)	15.336 (12.954)

Pairwise Mann-Whitney tests at the group-pair level show a significant difference between the Race and the Baseline treatments (p-value = 0.083), but no difference between the Color and the Baseline treatments (p-value = 0.326). Since individual effort decisions are not independent, it is not possible to run non-parametric tests for gender or race categories. Hence, we once again use panel regressions to test if the differences seen in Table 5.3 are statistically significant. As a first step, Table 5.4 presents the estimates of four individual random effects regressions of efforts on treatment dummies and controls. Whereas the first column includes the data from all the treatments, the next three columns consider treatments pairwise. The additional independent variables are lagged effort of the rival group, the individual's one-period lagged effort, a time trend (period), and race and gender dummies.

The first column in Table 5.4 reiterates that individual efforts are indeed higher in the Race treatment than in the Baseline; the treatment dummy is positive and significant at the 5% level. Subjects expend 1.16 ECUs more effort more per round in the Race treatment than in the Baseline treatment. However, as observed in the descriptive statistics and in the non-parametric test, although the efforts in the Color treatment are higher than the Baseline, it is not statistically significant. In the next two columns we further run the same regression but with pair-wise treatment data – between the Baseline and Race, and between Baseline and

Color treatments. As can be seen from the table, the outcomes remain the same. These findings are summarized in the following result.

Table 5.4. Individual efforts

Dep variable: $Effort_{i,t}$	**All**	**Baseline + Color**	**Baseline + Race**	**Color + Race**
Color	0.321	0.343		
	(0.473)	(0.548)		
Race	1.156**		1.030**	0.813
	(0.569)		(0.508)	(0.517)
Lag rival effort	0.056**	0.042*	0.082***	0.046***
	(0.016)	(0.024)	(0.012)	(0.018)
Lag own effort	0.547***	0.546***	0.502***	0.579***
	(0.040)	(0.046)	(0.047)	(0.047)
Period	-0.128***	-0.124***	-0.128***	-0.135***
	(0.027)	(0.039)	(0.027)	(0.033)
E. Asian	0.485	0.454	0.325	0.657
	(0.476)	(0.581)	(0.508)	(0.637)
Female	0.998**	0.450	0.879*	1.558***
	(0.468)	(0.605)	(0.532)	(0.553)
Constant	3.397***	4.176***	3.143***	3.415***
	(0.759)	(0.791)	(0.823)	(0.962)
# of Obs.	4,104	2,736	2,736	2,736
# of subjects	216	144	144	144

Standard errors are in parentheses; ***,** and * indicates significance at the 1%, 5%, and 10% level

Result 3: *Individual* effort*s are higher in the Race treatment than in the Baseline treatment, but they are not higher in the Color treatment compared to the Baseline treatment.*

Note, however, from the last column of Table 5.4 that the overall effort level is not significantly different at conventional level (p-value = 0.119) when we compare the Race and the Color treatments. A casual observation indicates that this might be due to the fact that females expend significantly more effort in Race treatment but males do not, and that dilutes the overall effects of the Race treatment. The first column of Table 5.4 also shows that the female subjects expend about 1 ECU more effort than their male counterparts overall. However, this difference is not as significant in the pairwise comparisons (except in the last column). Note that the regressions in Table 5.4 do not test for differences in effort levels between males and females in the different treatments. We investigate this by estimating

individual-level regressions with interaction between treatment and gender dummies. The dependent variable is once again individual efforts and the dependent variables include those in Table 5.4, along with the interaction terms. Table 5.5 summarizes the result.

Table 5.5. Effects of Gender

Dependent variable: $Effort_{i,t}$	**All**	**Baseline vs. Color**	**Baseline vs. Race**	**Color vs. Race**
Baseline×female	-0.440	-0.371	-0.491	
	(0.485)	(0.499)	(0.554)	
Color×male	-0.559	-0.480		
	(0.693)	(0.721)		
Color×female	0.745	0.813		1.121
	(0.816)	(0.844)		(0.975)
Race×male	-0.140		-0.343	0.400
	(0.710)		(0.711)	(0.762)
Race×female	1.995**		1.921**	2.389***
	(0.803)		(0.769)	(0.743)
Lag rival effort	0.057***	0.044*	0.083***	0.046***
	(0.016)	(0.024)	(0.012)	(0.018)
Lag own effort	0.543***	0.544***	0.495***	0.578***
	(0.038)	(0.045)	(0.043)	(0.047)
Period	-0.130***	-0.123***	-0.132***	-0.137***
	(0.027)	(0.039)	(0.027)	(0.033)
E. Asian	0.556	0.426	0.463	0.716
	(0.508)	(0.605)	(0.566)	(0.655)
Constant	4.156***	4.557***	3.893***	3.653***
	(0.664)	(0.806)	(0.653)	(0.887)
N	4,104	2,736	2,736	2,736

Standard errors are in parentheses; ***,** and * indicates significance at the 1%, 5%, and 10% level

The first and third columns in Table 5.5 show that the higher individual efforts in the Race treatment are really driven by higher efforts by females in that treatment. Relative to males in the baseline, females in the Race treatment expend significantly higher amount of effort. The race dummy remains insignificant in this regression. The second column shows that females do not expend significantly higher effort in the Color treatment compared to the Baseline. Finally, it is clear from the last column that females expend significantly higher

effort in the Race treatment compared to the Color treatment. None of the other controls show any different results from the previous analyses. These findings are summarized below.

Result 4: *The higher efforts in the Race treatment relative to those in the Baseline treatment are driven by the higher efforts expended by female subjects. Females also expend significantly higher effort in the Race treatment compared to the Color treatment. However, there is no significant difference in efforts expended by racial groups.*

Although seemingly counterintuitive, these results conform to the general observation of higher effort exertion by females in contests (Dechenaux et al., 2012) or being prone to the winner's curse (Casari et al., 2007). These also match with the existing observation that there are differences between the decisions of men and those of women (Eckel and Grossman, 2008) and that being in a group has stronger effects on female decisions (Croson et al., 2003). These results also add to the existing literature on the competitiveness of females (Apesteguia et al., 2012; Cadsby et al., 2013; Niederle and Vesterlund, 2007, 2011), that has thus far shown mixed results.

We further investigated whether the higher effort of females in the race treatment, is due to an effect of real identity or if it is only context-driven behavior as discussed by Croson and Gneezy (2009). We included an interaction of female dummy with race treatment in the first regression of Table 5.4, and it turns out to be significant *along with* the race treatment dummy itself. Hence, we conclude that identity itself does induce higher efforts by females.

5.5 Discussion

We, for the first time in the literature, investigate the effects of identity and classification in group conflicts in a controlled experiment. We employ a group contest with no identity, real racial identity and a minimal identity. We find that conflict is significantly higher in the real identity treatment than in the baseline but it is not the case for the minimal identity treatment. This is due to both initiation (less free riding) and escalation (expending more positive effort) of conflict in the real identity case. Hence, our result supports the hypotheses of Sen (2007) that the salience of an identity can initiate and escalate conflict. Furthermore, whereas a real identity indeed increases conflict, an induced identity ('classification') might not do so. We further find that the increase in conflict comes mainly from the increase in efforts by females across racial groups. However, race itself is not significant in increasing effort level.

The results, however, cannot tease out the effects of identity on in-group cooperation versus out-group hate. Hence, the most we can conclude from these results is that a real

identity has a direct effect on parochial altruism (Choi & Bowles, 2007; Halevy et al., 2008; Abbink et al., 2012). That is, the incremental effect of identity in conflict efforts can come either through an increase in group cooperation for the love of own group's identity, or through an increase in hate for the other group's identity. While the current design is enough to answer our specific research questions as hypothesized by Sen (2007), it will still be interesting to explore which effect out of parochial altruism turns out to be stronger.

The issue discussed above, hence, can be an area of extension for the current study. Many other extensions are also possible. The results seem to be robust since there is no pre-existing conflict between Whites and East Asians in the UK. But it will be interesting to see the effects when the same experiment is run between groups that have pre-existing conflicts. A lab-in-the-field experiment will also help. It would be useful to run individual contest that does not have the issues of in-group cooperation. Finally, searching for mechanisms through which the conflict intensity can be reduced will be an important area of extension.

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Chapter 6

Eye-image in Altruism Experiments: Social Cue or Experimenter Demand Effect?[23]

6.1 Introduction

A fundamental question explored in economics and psychology alike is 'why do people behave pro-socially?' Researchers till date suggested various explanations for the same. These include social preference theories such as pure and impure altruism, reciprocity, inequality aversion etc. (Andreoni, 1989; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002), economic effects such as property rights, income, price of giving etc. (Ruffle, 2000; Chowdhury and Jeon, 2014; Andreoni and Vesterlund, 2001), demographic effects such as gender, age, race, etc. (Eckel & Grossman, 1998; Croson and Gneezy, 2003), and social control effects such as social distance, information, and social cues (Burnham, 2003; Leider et al., 2009; Alevy et al., 2014).[24] A recent stream of literature has shown that one of the most interesting component to guide pro-social behavior is social cue (Rigdon et al., 2009). A social cue (SC) is a verbal or visual hint that guides conversation, transaction and other social interactions. SCs can be of various types, but visual cues are the most prolific type that are employed in the laboratory and the field experiments investigating social preference. It is argued that the introduction of an image of a pair of eyes in the experimental environment is such a visual SC.

It is observed in biology, psychology and economics experiments that such eye-image often increases the level of pro-social behavior in an experiment. The examples of such pro-social behavior includes making proper payment for something bought (Bateson et al, 2006), the amount of giving in a dictator game (Haley and Fessler, 2005), amount of contribution in a public good game (Burnham and Hare, 2007), higher donation for library (Croson and Krupka, 2008), lower theft (Nettle et al., 2012), keeping an area clean (Ernst Jones et al., 2011) etc. A weaker version of the eye-image, such as three dots, in a dictator game causes similar results on giving behavior (Rigdon et al., 2009). The effect of the eye-image, however, is not symmetric across games and agents. Ernes-Jones et al. (2011) and Ekstrom

23 This Chapter is a joint work with S. M. Chowdhury and B. Saha.
24 See Engel (2011) for a meta-analysis and discussion of each issue in detail.

(2012) find that eye-image has more effect when there are fewer people around. Rigdon et al. (2009) find that the male subjects are affected more by the image than the females. A very few studies such as Fehr and Schneider (2010), Carbon and Hesslinger (2011) and Raihani and Bshary (2012) cannot find significant effects of eye-image. They argue that eye-image might only promote cooperative behavior in relatively public settings, and that the existing results might be caused by uncontrolled implicit SCs. Sparks and Barclay (2013) find a robust, positive, effect of eye-image on pro-social behavior in their meta-analysis of 25 studies. Hence, the related area of literature considers that the eye-images constitute a 'social cue', i.e., a signal that cues the subjects to be pro-social.

We observe that all the existing analyses (except Nettle et al., 2012) on the effects of eye-image employ a 'positive frame' – in the sense that the subjects can choose between being pro-social or not. It is argued that several games such as dictator game or public good game may be prone to an experimenter demand effect (Zizzo, 2010). Experimenter demand effect (EDE) is defined as the "*changes in behavior by experimental subjects due to cues about what constitutes appropriate behavior (behavior 'demanded' from them)*". Zizzo (2010) further notes that EDE can be "*a potential problem only when they are positively correlated with the true experimental objectives' predictions*". Since the employment of eye-image may cue the subjects about 'the behavior demanded from them' in the game, and that behavior is perfectly positively correlated with a pro-social behavior, these experiments cannot disentangle these two effects. Ekstrom (2012) acknowledges this issue and employs a field setting arguing that since "*subjects are unaware of participating in an experiment, demand effects are unlikely*". The issue of the cue regarding 'appropriate behavior', however, remains in the experiment. In this paper we aim to overcome this issue by implementing a 'negative frame', in which the effects of the SC and the EDE are opposite. In particular, we use a specific version of the dictator game – the taking game.

In a standard dictator game a *dictator* has some money, and decides upon how much of that money to give to a *recipient*. The recipient needs to accept the dictator's decision. We call this a 'giving' frame. This game is introduced by Forsythe et al. (1994) and it is well known that a non-trivial number of subjects as dictators give a non-trivial amount of money to the recipient (Kahneman et al., 1986; Forsythe et al., 1994; Camerer, 2003; Engel, 2011). In a 'taking' frame, however, the recipient starts with some money and the dictator decides upon how much to take from the recipient. Again, the recipient cannot counter the dictator's decision. Suvoy (2003) was the first to introduce such a frame in dictator game and allowed

dictators to make decisions under both 'give' and 'take' frames.[25] Different variations of this framing is implemented by List (2007), Bardsley (2008), Dreber et al. (2012), Korenok et al. (2013) and Chowdhury et al. (2014), among others. List (2007) and Bardsley (2008) allow dictators not only to give an amount to a recipient, but also to take from recipient's endowment. They show that this causes a reversed generosity for a dictator and that previous social preference theories do not work anymore. The other studies conclude that when only one option (giving or taking) is available, then giving is equivalent to not taking.

We note that if the eye-image indeed acts as an SC, then introducing the image should give the dictator cues to take less from the recipient, as that would be the pro-social behavior. However, if the eye-image acts as an EDE, then the image should give cues about the behavior demanded by the experimenter in a taking frame, i.e., to take more. Since the two possible effects act in opposite directions, unlike the existing studies, this frame will allow one to find out whether the existing results are obtained due to SC or due to EDE.

In this study we implement a 'taking' game in two treatments with and without eye-image. Since it is found in existing studies that the images may have asymmetric effects on gender (Rigdon et al., 2009) we keep a gender balance among subjects in each treatment for our analysis. We aim to make a two-fold contribution in this study. First, we run the very first laboratory experiment with proper control with eye-image in a negative frame. Second, we for the first time raise the issue of EDE vs. SC effects of eye-image, and employ a controlled setting to test the same.

Supporting the majority of the literature, we find that the eye-image indeed acts as an SC, even in a negative frame. But the effect is not symmetric across gender. Similar to Rigdon et al. (2009), we find that eye-image makes male dictators more pro-social by becoming less selfish and more egalitarian. The effect on the females, although apparently in the direction of the EDE, is not significant. We conclude that, at least for males, eye-image indeed acts as a social cue. The rest of the paper is organized as follows. Section 2 proposes a theoretical framework to state the logic in a comprehensive manner. Section 3 discusses the experimental design and Section 4 presents the results. We conclude in the next section.

6.2 A Theoretical Framework

The utility function of the dictator can be written as -

$$u_i = u_i\left(x_i, x_j | SC_i(I_E), EDE_i(I_E)\right) \quad (1)$$

25 Note, however, that a negative framing was implemented earlier in public good game by Andreoni (1995).

Where x_i is own payoff, x_j is recipient payoff, I_E is an indicator variable that takes value 1 when eye-images are included. $SC_i(I_E)$ is subject-specific social cue effect and $EDE_i(I_E)$ is subject specific experimental demand effect with $|SC_i(1)| \geq |SC_i(0)|$, $|EDE_i(1)| \geq |EDE_i(0)|$, and for expositional purposes assume $SC_i(0) = EDE_i(0) = 0$.
For dictator game, the control variable is giving and the utility function is written as

$$u_i = u_i\left(F - x_j, x_j | SC_i(I_E), EDE_i(I_E)\right) \quad (2)$$

For Taking game the control variable is taking and the utility function is written as

$$u_i = u_i\left(x_i, F - x_i | SC_i(I_E), EDE_i(I_E)\right) \quad (3)$$

$SC_i(I_E)$ affects the utility in the following way: when $SC_i(I_E)$ takes high value, it increases social preferences in the subject and he allocates more weight to the payoff of the recipient; which is x_j for dictator game, and $F - x_i$ for taking game.

$EDE_i(I_E)$ affects the utility in the following way: an experimental demand effect makes him to follow the instruction and give higher weight to the control variable. Hence, when $SC_i(I_E)$ takes high value, he allocates more weight to x_j for dictator game, and x_i for taking game.

Understandably, the effects of $SC_i(I_E)$ and $EDE_i(I_E)$, when eye-images are visible, go in the same direction for dictator game. Hence, if any of the two effects is strictly positive, then the dictator will give more to the recipient when eye-image is visible. If both effects are zero, then he will give the same amount as no eye-image.

However, the effects of $SC_i(I_E)$ and $EDE_i(I_E)$, when eye-images are visible, go in the opposite direction for taking game. Taking more reduces mental cost of calculation and EDE affects a subject to take more whereas SC affects the subject to take less. Hence, the relative effect of the two determines whether a dictator will take more, less or the same amount in eye-image treatments compared to the no eye-image treatment.

Given the results from the existing literature, one would expect the effect of SC to be stronger for males (as seen in Rigdon et al., 2009), and the effect of EDE to be stronger for females (as discussed in Croson and Gneezy, 2003). But whether that is actually the case is an empirical question.

6.3 Experimental Design and Procedure

We employed a computerized one-shot Taking Game with 2 treatments. In the treatment with eyes (the 'Eye' treatment), the computer screen and the paper instructions had a rectangle

with a pair of eyes imprinted on it, whereas in the 'Baseline' treatment there was a grey colored rectangle (of the same size) instead of the eye picture.

There were 160 subjects spread across 4 sessions. In each session, subjects sat in cubicles and were randomly and anonymously placed into pairs with roles as either a 'Dictator' or a 'Recipient'.[26] Hence, in each treatment, there were 40 dictators providing us with 40 independent observations. Furthermore, we ensured a gender balance, i.e., there were 20 male dictators and 20 female dictators; and 20 male recipients and 20 female recipients in each treatment. However, the dictator or the recipient did not know the gender of the partner. Each subject received a £3 show-up fee, but the recipients had access to an additional £10. The dictators had the choice to take away any amount between £0 and £10 from the recipient, and the recipients were left with the residual amount. All of these were common knowledge.

Each session consisted of two parts. In the first part, dictators were informed that they can take any amount, in denominations of 1 penny, from the £10 the recipients have and the recipients will have to accept the decision. In the second part, recipients had to guess the amount the dictator had taken. If the absolute difference between the actual amount taken and the guess was within 50 pence, then the recipient received an extra £1.[27] The instructions for the second part was given only after the decisions in the first part were made. It was mentioned in the instruction of the first part that the Recipient's decision was payoff irrelevant to the Dictator. This was done to avoid any possible strategic interaction between dictator giving and recipient anticipation. However, we do not find any treatment or gender effect in the guesses and hence it is not discussed in the continuation.

Each subject participated in only one session. The subjects were students at the University of East Anglia, without any prior experience with giving or taking game or a social cue experiment, and were recruited through the online recruitment system ORSEE (Greiner, 2004). The experiment was computerized with z-TREE (Fischbacher, 2007). The sessions were run by a research assistant who read the instructions aloud and answered any question in private. Each session took around 30 minutes and the average payment was about £8. Demographic information such as gender, age, nationality, and study area of each subject was collected in an anonymous computerized survey after the experiment.

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26 The subjects did not know about these tags and they were referred as 'you' and 'the person you are paired with'. Please see the instructions in the Appendix. A better terminology for this game could have been 'Taker' and 'Owner'. But, following the literature, we continue to refer them as 'Dictator' and 'Recipient'.

27 This linear incentive mechanism for guess is similar to the ones in Brañas-Garza and Rodriguez-Lara (2014) or Chowdhury and Jeon (2013) who apply it for standard dictator games.

6.4 Results

We start by comparing the amount taken across treatments. Table 6.1 shows the average and standard deviation of amount taken by treatment, and by gender. Complying with the results obtained in the standard dictator game by Carbon and Hesslinger (2011) and Raihani and Bshary (2012), we observe not much overall treatment effect in amount taken. Whereas in the Baseline treatment on average £7.77 is taken, in the Eye treatment the average amount taken is £7.47. When we observe the same statistics by gender, the result, however, is surprising. It seems that the eye-images decreases taking for the males (from 8.80 to 7.48), but increases the taking for the females (6.74 to 7.48). Hence, it can be hypothesize that the eye-image has an SC effect on the males but an EDE effect on the females.

Table 6.1. Average amount (Std. Dev) taken

Data	Baseline	Eye
All	7.773 (2.156)	7.477 (2.348)
Male	8.805 (1.70)	7.475 (2.370)
Female	6.740 (2.097)	7.480 (2.389)

To test whether the differences are significant, we run six OLS regressions. Table 6.2 reports the results of OLS regression in which the dependent variable is the amount taken and the independent variables are treatment and gender dummies, their interaction, and age. The first and fourth columns show results for the whole dataset, whereas the second and third columns show results by gender, and the two remaining columns show results by treatment.

The first three columns of Table 6.2 reiterate the results from Table 6.1. Whereas for overall and for females the Eye-image does not have any treatment effect, it is negative and significant for males (p-value = 0.033). We further explore the effects of gender in the next three columns. It shows that females overall and without eyes take less than males. But that is not the case when there is an eye-image. This happens, as we have seen earlier, due to an increase in female taking and a decrease in male taking with eye-image. This is reflected in the coefficient of the interaction variable Male×Eye, which is negative and significant. As robustness checks, we also ran Mann-Whitney tests. Complying with the regression results,

we find no difference in the total and in the female data. But the amount taken by male subjects in the two treatments turn out to be different at 10% significance level. Hence, it seems that eye-image has an SC effect for males; but no overall effect for females as well as in the total data.

Table 6.2. Regression of amount taken on treatment and controls

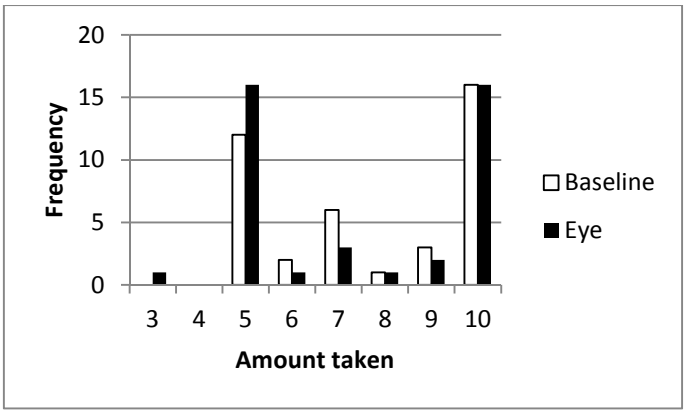
Dep var: amount taken	Total	Female	Male	Total	Baseline	Eye
Intercept	9.064***	7.086***	10.739***	9.499***	9.952***	7.307***
	(0.988)	(1.175)	(1.790)	(0.988)	(1.025)	(1.682)
Eye	-0.335	0.715	-1.386**	0.688		
	(0.496)	(0.723)	(0.652)	(0.687)		
Female	-1.026**			-2.045***	-2.032***	0.007
	(0.494)			(0.684)	(0.601)	(0.763)
Male×Eye				-2.038**		
				(0.968)		
Age	-0.032	-0.014	-0.080	-0.029	-0.047	0.007
	(0.036)	(0.042)	(0.071)	(0.036)	(0.038)	(0.068)
# of Obs.	80	40	40	80	40	40
Adjusted R^2	0.030	-0.022	0.081	0.072	0.226	-0.054

Standard errors in parentheses. ***,** and * indicates significance at the 1%, 5%, and 10% level.

Before investigating further, we note that our result matches qualitatively with that of Rigdon et al. (2009), who find SC effect on males but not on females. Their significance level, however, was much stronger (at 1% level). We conjecture that this can be due to two reasons. First, their experiment was run manually whereas we ran ours on computer. The lower level of social contact may have an effect on the level of pro-social behavior and as a result on the effect of the eye-images. Secondly, as mentioned in the theory section, both the SC and EDE effect might be prevalent for both male and females. Whereas for male the SC effect is stronger, for female they cancel out each other. Since both the effects work in the same direction in Rigdon et al. (2009), they find a stronger effect of the eye-image; but the effects are in the opposite direction in this study and we find a weaker effect.

Since the results show such a difference in the effects of eye-image across gender, we decide to explore this issue further by classifying the dictators in terms taking behavior. Figure 6.1 reports the amount taken distribution for all the subjects separated by treatments. The X-axis is the amount taken by a dictator, and the Y-axis shows the number of dictators.

Figure 6.1. Distribution of amount taken: Overall data



Taking follows a bimodal distribution with most of the dictators being 'complete selfish' (taking all the amount, i.e., £10), or very much 'egalitarian' (taking half or almost half of the amount, ~£5).[28] This matches with the observation from a standard dictator game (Engel, 2011). However, there is not much difference in the distribution by treatment. We then plot the same figure for male and for female in Figure 6.2 and Figure 6.3.

Figure 6.2. Distribution of amount taken: Female

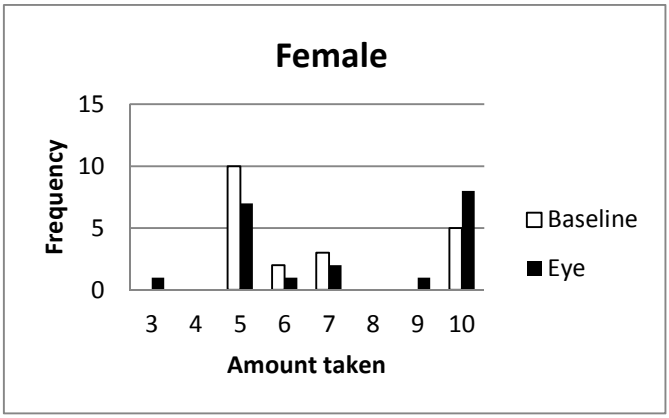
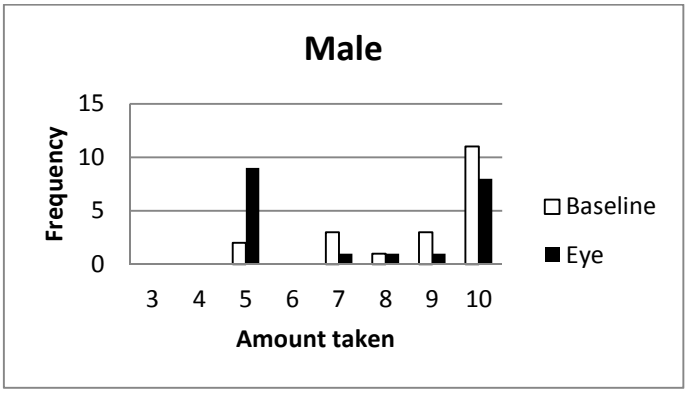


Figure 6.3. Distribution of amount taken: Male



28 We consider three subjects taking £5.5 or £5.8 as egalitarians. Also one subject who took less than £5 is excluded here. Variation of these do not change results either in the descriptive statistics or in the regressions.

These figures, again, show a stark difference in the treatment effect by gender. Whereas the overall bimodal feature still prevails, the treatment effect is now different. When there is no eye–image available, most of the females act like egalitarian, but most of the females act like complete selfish when eye-image is available. It is just the opposite and even more extreme in terms of frequency for males. This, again, brings back the idea that the effects of eye-image as SC might be stronger for the males whereas the effects of eye-image as EDE might be stronger for the females. To test this we run an ordered probit model where the dependent variable is whether the dictator is a complete selfish, or egalitarian or in between (i.e., the dictator has taken £10, or £5 or in between). The dependent variables are the treatment dummy and the demographic variable. We run this for the whole data and by gender. The regression results are reported in Table 6.3 whereas the marginal effects are included in Table 6.4. In both the tables, standard errors are included in parentheses and ***,** and * indicates significance at the 1%, 5%, and 10% level.

Table 6.3. Ordered probit results

All:	$Category_i$ = – 0.156 Eye_i – 0.337 $Female_i$ – 0.021 Age_i (0.259) (0.258) (0.020) No. of obs. = 79. Pseudo R-Sq = 0.018
Female:	$Category_i$ = 0.400 Eye_i – 0.009 Age_i (0.367) (0.023) No. of obs. = 39. Pseudo R-Sq = 0.017
Male:	$Category_i$ = – 0.770** Eye_i – 0.061 Age_i (0.381) (0.042) No. of obs. = 40. Pseudo R-Sq = 0.065

The results in Table 6.3 reaffirms that there is a significant effect of the eye-image on males. It further confirms that when the eye-image is available, males move out of the 'compete selfish' type, and move into the 'egalitarian' type. This is statistically significant. Although the opposite movement may be observed for females, it is not significant.

Table 6.4. Marginal effects of ordered probit regressions

Dep var: Ordered categories		Egalitarian	Mixed	Selfish
All	Eye	0.055	0.004	-0.059
		(0.091)	(0.008)	(0.097)
	Female	0.119	0.009	-0.128
		(0.089)	(0.013)	(0.095)
	Age	0.007	0.001	-0.008
		(0.007)	(0.001)	(0.008)
	# of Obs.	79	79	79
Female	Eye	-0.149	0.007	0.142
		(0.132)	(0.020)	(0.126)
	Age	0.004	-0.000	-0.003
		(0.009)	(0.000)	(0.008)
	# of Obs.	39	39	39
Male	Eye	0.235**	0.043	-0.277**
		(0.107)	(0.032)	(0.118)
	Age	0.019	0.003	-0.022
		(0.012)	(0.003)	(0.014)
	# of Obs.	40	40	40

6.5 Discussion

We study, for the first time in the literature, the effects of eye-images on social-preference in a negative dictator game frame. We hypothesize that if the dictators take more in a taking game under eye-image, then the result would interpret the net effect of eye-image as an experimenter demand effect. But if they take less, then the net effect of eye-image will be interpreted as a social cue effect. We found no significant effect of the eye-image in the overall data, but males take significantly less and females take insignificantly more under eye-image. Furthermore, this decrease in taking by male comes from an increase in egalitarian and decrease in complete selfish type of subject under eye-image.

The aggregate result matches with that of Rigdon et al. (2009), but it is in contradiction with the results of Alevy et al. (2014), who find females to take less and males to be unaffected with their decisions while being observed by others (but not due to eye-images) in a frame where giving and taking are simultaneously possible. This raises the importance of further analysis on the effects of strategy space in dictator type games (with or without the effects of social cues) as suggested in Bardsley (2008) and List (2007).

We conclude that for males, the net effect of the eye-image definitely act as a social cue. Whereas, for the females, the net effect is either diluted or there is no effect of the eye-image. This, as well, asks for more research in this area including field studies.

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Chapter 7

Giving, Taking, and Gender in Dictator Games[29]

7.1 Introduction

Researchers in economics and in other disciplines of social science have shown a considerable interest in understanding social preference. Understanding the issues of why one individual behaves altruistically with others, or how to measure altruism have fascinated researchers. In this context, since its inception in the experimental literature, the 'Dictator game' has been considered as one of the most popular workhorse methods to understand and measure altruism. In the most standard form of a dictator game (Kahneman et al., 1986; Forsythe et al., 1994) a subject, called the dictator, is given an amount and decides upon how much money from that mount to allocate between himself and another passive subject, called the recipient.

Since the dictator does not otherwise have any incentive to share the money with the recipient, the amount transferred is often used as a measure of altruism. Indeed, several experiments showed consistently that dictators, on average, allocate a non-trivial amount of money (Camerer, 2003; Engel, 2011). Various social preference theories such as pure altruism (Becker, 1974), inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), impure altruism (Andreoni, 1989), conditional altruism (Konow, 2010) etc. are proposed to explain this behavior. Due to its extreme easy and interpretable structure, this game is employed in various forms and frames to investigate specific questions related to altruism. A simple Google scholar search to date (late 2014) returns almost 800 published and unpublished papers in economics, management, psychology, and other research areas implementing a dictator game.

The interpretation of giving in the game, however, is not free of criticism. It is observed that this game can be prone to the house money effect (Ruffle, 1998), be affected by experimenter demand effect (Zizzo, 2010), become sensitive to contexts (Eckel and Grossman, 1998) and cues (Bateson et al., 2006). Finally, it is also argued that the outcomes can be sensitive to the framing of the experiment. Understandably, in case the results depend on framing, they may not be interpreted in a certain general way.

29 This Chapter is a joint work with S. M. Chowdhury and B. Saha.

In investigating the effects of framing for the first time, Suvoy (2003) introduces a 'taking game'. Whereas in a standard dictator game (the 'giving game', or GG) the dictator owns some money and decides to 'give' a part to the recipient, in the taking game (TG) the recipient owns some money and the dictator decides upon how much to take from him. Suvoy (2003) allows the dictators to play the two games simultaneously but separately and did not find a difference in allocation (i.e., the money given in a GG or the money not taken in a TG) to the recipient. Bardsely (2008) and List (2007) extend this to the case when the dictator can either give some amount to the recipient or take some amount from the recipient. Hence, these studies also alters the strategy space. Results by both of Bardsely (2008) and List (2007) show that the availability of a taking option in a giving game decreases the amount given to the recipient – thus confirming the sensitivity of the framing in dictator allocation.[30]

Since then, there is a flurry of studies that employs this framing in investigating various questions and when both GG and TG framing are employed, the result is – at most – mixed. Eckel and Grossman (2012) use the GG and TG where the recipient is a real charity. They find no difference in allocation. Same result appears in Rubinstein (2014), who run virtual GG and TG games online with no money. Alevy et al. (2014) employ a 2x2 design in which GG and TG are played anonymously vs. when the decisions are observed. They found no difference in allocation when the play was anonymous. But females take less and males are unaffected with their decisions while being observed. In the same line, Kettner and Ceccato (2014) investigate the effects of framing while interacting with revelation of dictator and recipient gender. They find no overall framing effect on allocation when gender is not revealed. But dictators significantly take less when the recipient is of opposite gender. Korenok et al. (2013) employ a frame similar to List (2007), and found that not taking is not equivalent to giving. Cappelen et al. (2013) also employ a 2x2 design where GG and TG are interacted with whether the amount is earned or it is windfall. They found that introducing a take option always decreases allocation to recipient, but origin of the amount is not significant.[31] Finally, Engel (2011) in his meta-analysis finds no effect of framing in dictator allocation. But Zhang and Ortman (2014), using the same data but different analysis, found result similar to Bardsley (2008) and List (2007).

30 Applying similar framing Brosig et al. (2007) investigate within subject behavior in repeated GG and TG games, and found dictators to take almost all the amount with experience. Keysar et al. (2008) applies deception and found that the act of giving is objectively considered more generous than the act of not taking.

31 There are two studies, to the best of our knowledge, which employ only a TG frame. Heinrich and Weimann (2013) find that dictators are sensitive to taking price. Chowdhury et al. (2014) find gender difference in the effect of social cue in a taking game.

Investigating whether giving in a GG frame is equivalent to not taking in a TG frame is important, since this allows one to understand the robustness of the game, as well as robustness of the behavioral theories that are constructed and tested using this game. Although the effects of strategy space is another important question to consider, it is however not an apple to apple comparison of the framing, when the strategy space is also altered in one frame to another. Hence, the studies of Bardsley (2008), List (2007) and others that use similar frame, are not appropriate to test pure framing effect on altruism. A specific test of the framing would be to compare GG and TG, while keeping the strategy space the same. In this this study we do exactly the same. We employ between-subject GG and TG frames in which the dictator has the option to allocate £10. Our study is very close to the paper by Dreber et al. (2013) who run laboratory and online dictator game experiments with GG ad TG frames to investigate if preferences are affected by the framing. They do not find any difference in the amount allocated to the recipient by framing.

We further observe that the results in the dictator game, even under anonymity, are known to be affected by dictator gender (Eckel and Grossman, 1998; Croson and Gneezy, 2003). Female dictators are on average more generous than males (Engel, 2011). Females are also more prone to the context of the experiment (Croson and Gneezy 2009) than males. Moreover, the change in frame from GG to TG has implication such as a property right of the pie to divide (Oxby and Spraggon, 2008), and possible cognitive biases such as endowment effect (Thaler, 1980) and status-quo bias (Samuelson and Zeckhauser, 1988). Since these biases may not be gender independent, it is important also to understand the effects of gender while investigating the framing effects. As a result, we replicate the study by Dreber et al. (2013), but in design and analysis we keep a focus on the effects of dictator gender.

Our results support the overall result by Dreber et al. (2013) that giving in GG is indeed equivalent to not taking in TG. Investigating further, the framing show opposite effects on male and female. Females are significantly generous than males in the TG but not in the GG. They are also significantly more generous in TG than in GG, but the males only insignificantly less generous in TG than in GG. We hence extend the result of Dreber et al. (2013) showing that framing does not affect overall allocation, but it does across gender.

The rest of the paper proceeds as follows. Section 2 describes the experimental design and procedures. Section 3 includes the main results and Section 4 concludes.

7.2 Experimental Design

We employed a between-subject Dictator game with 2 treatments and 40 dictators in each treatment. To keep it gender balanced, in each treatment we recruited 20 male and 20 female dictators. Only one treatment was run in a particular session. In each session, subjects were randomly and anonymously placed into pairs and were asked to sit in cubicles. They were then assigned the role as either a dictator or a recipient. Each subject played only one role and the role remained the same until the end of the session.

Subjects received information about a £3 show-up fee, which was the same for all subjects in this experiment. In the 'Giving' treatment the dictator was given access to an additional £10 and could transfer any amount between £0 and £10 to the recipient. In the 'Taking' treatment the recipient, instead, was given access to an additional £10 and the dictators could transfer any amount between £0 and £10 to himself. We intentionally used neutral command 'transfer' instead of 'give' and 'take' to make the effects of instructions minimal. The roles of the recipients were passive and they had to accept the dictators' decision (please see the instructions in the Appendix).

Each session consisted of two parts. In the first part, Dictators made decisions. In the second part, Recipients had to guess the amount the dictator had given or taken. If the absolute difference between the actual amount and the guess was within 50 pence, then the recipient received an extra £1.[32] However, we did not find any treatment or gender effect in the guesses and hence it is not discussed in the continuation.

The subjects were students at the University of East Anglia and did not have any prior experience with dictator or taking game. They were recruited through the online recruitment system ORSEE (Greiner, 2004). The sessions were computerized with z-TREE (Fischbacher, 2007). Each subject could participate in only one session. Each session took around 30 minutes and the average payment was £8. Demographic information such as gender, age, nationality, subject area of each participating subject was collected through a computerized survey after the experiment.

32 This linear incentive mechanism for guess is similar to the ones in Brañas-Garza and Rodriguez-Lara (2014) or Chowdhury and Jeon (2013) who apply it for standard giving games. The instructions for the second part was given only after the decisions in the first part were made. It was mentioned in the instruction of the first part that the recipient's decision was payoff irrelevant to the dictator. This restricts any possible strategic interaction between dictator decision and recipient anticipation.

7.3 Results

Table 7.1 shows the average amount allocated (amount given in the GG or amount left in the TG) towards recipients for overall data and also for male and female by treatment. In the GG, an average of £1.93 is given to the recipients. In the TG, the dictators take on average £7.77 and as a result, the amount left to the recipient is £2.23. A Mann-Whitney test shows no significant difference in final amount allocated to recipients between the two treatments (p-value = 0.78). This result is consistent with the results of Dreber et al. (2013) who also find no framing effects in all three experiments they run.

We now investigate the same issue across gender. Male dictators in GG give on average £1.83 and in TG leave an average of £1.20 for the recipient and this difference is not significant at conventional level (p-value = 0.11). However, the average allocation to recipient by female dictators are £2.03 in GG and £3.26 in TG respectively and the difference is significant (p-value = 0.08). Within treatments, the results also show gender difference. In the GG, there is no gender difference on average amounts given. The TG, however, shows gender difference in the average amount left to recipient. The average amount left by male dictators is only £1.20 whereas for female dictators it is £3.26. A Mann-Whitney test confirms that this difference is significant at 1% level.

Table 7.1. Average (Standard Dev) allocation to recipient

Data	Giving game	Taking game	Mann-Whitney test (Giving vs. Taking)
All (40 obs/treatment)	1.928 (1.739)	2.228 (2.156)	No difference (p=0.778)
Male (20 obs/treatment)	1.830 (1.611)	1.195 (1.702)	No difference (p = 0.112)
Female (20 obs/treatment)	2.025 (1.895)	3.260 (2.097)	Difference at 10% (p = 0.079)
Mann-Whitney test (Male vs. Female)	No difference (p=0.879)	Difference at 1% (p= 0.005)	--

To test the robustness of the findings, we further run OLS regressions. The dependent variable is the amount transferred to the recipient and the independent variables are treatment

dummy, gender dummy, their interactions, and age. We run the analysis for total data and for males and females separately. The results are reported in table 7.2.

Table 7.2. Regression of amount transferred to recipient

OLS	Total	Total	Male	Female
Intercept	0.442	0.119	-0.121	2.378*
	(0.823)	(0.817)	(0.993)	(1.199)
Giving game	-0.173		0.716	-1.106*
	(0.424)		(0.520)	(0.656)
Female	1.148***			
	(0.417)			
TG × Female		2.034***		
		(0.576)		
GG × Male		0.702		
		(0.577)		
GG × Female		0.961		
		(0.582)		
Age	0.049	0.04	0.054	0.035
	(0.030)		(0.038)	(0.045)
# of Obs.	80	80	40	40
Adjusted R^2	0.088	0.130	0.038	0.058

Standard errors in parentheses. ***, ** and * indicates significance at the 1%, 5%, and 10% level.

In the first column of Table 7.2, we use the dummy variable of GG to test for the framing effect, while controlling for gender and age. Complying with the result in Table 1, the coefficient for GG is insignificant but the coefficient of Female is positive and significant at 1% level. That is, overall there is no treatment effect on transfer but female dictators are more generous than males. This result also matches exactly with that of Dreber et al. (2013), who report the same result from the lab experiment in their Table 7.1.

These, however, do not reveal any effect of gender through treatments and Dreber et al. (2013) do not shed light in this issue either. Hence, to examine this result further, we run the same regression for males and females separately. We find that males do not change their altruistic behavior due to framing (p-value for the coefficient of GG = 0.161). But females transfer less amount to the recipients in the GG compared to the TG (p-value for the coefficient of GG = 0.092). We further use interaction of gender and treatment in the whole data and result in column 2 it shows that females transfer significantly higher amount in the TG frame compared to the GG frame.

7.4 Discussion

We investigate whether a pure framing effect exists on a dictator game when the strategy space remains the same across treatments. We run a giving and a taking game and compare the amount given in the giving game with the amount left for a recipient in the taking game. Both non-parametric test and regressions reaffirm the observation of Eckel and Grossman (2012) and Dreber et al. (2013) that overall there is no framing effect on dictator game and giving is indeed equivalent to not taking. However we extend the observation by further investigating the results by gender. We find that the framing has opposite effects for female and for male dictators, although the effect is significant only for females. We find this result even though we have implemented an extremely neutral instruction with words such as 'access to' instead of 'belong to' and 'transfer' instead of 'take'. We believe that the results will be stronger if a more frame specific instruction is implemented, or the experiment was run manually instead of in a computer. Nevertheless, our result shows that gender effect exists even in the weakest possible case.

We suppose that this result arises due to two effects. First, the framing of GG and TG introduces (at least) a weak salience of property right about the pie to be divided. Whereas in the GG frame the property right belongs to the dictator, in the TG frame it belongs to the recipient. This brings in the issues of cognitive biases such as endowment effect (Thaler, 1980) and status-quo bias (Samuelson and Zeckhauser, 1988). Second, it is known from the study of Croson and Gneezy (2003) that females are more affected with the context of an experiment. Hence, once we introduce the biases through the framing, it significantly affects the females and they respond to the endowment effect or the status-quo bias by taking less in the TG.

This explanation, however, cannot be tested within this experiment. A further and concrete investigation of the reason of this gender difference is warranted and we leave it as a topic of future research.

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Appendix A (appendix Chapter 2)

A.1 Proof of Proposition

A.1.1 Linear form inequality aversion: Proof of Proposition 1

Equation (1) can be rewritten as:

$$u_i = (y_i + F) - \alpha_i max\big[(y_j + F) - (y_i + F), 0\big] - \beta_i max\big[(y_i + F) - (y_j + F), 0\big]$$
$$= (Y - y_j + F) - \alpha_i max\big[2y_j - Y, 0\big] - \beta_i max\big[Y - 2y_j, 0\big]$$

The dictator would try to maximize utility with respect to the giving decision. There can be 2 cases: $y_j > Y/2$ and $y_j \leq Y/2$. It is easy to show that the first case does not arise. Hence the dictator's optimization problem boils down to:

$\underset{y_j}{max}\ u_i = (Y - y_j + F) - \beta_i(Y - 2y_j)$ subject to $Y/2 \geq y_j \geq 0$

Let λ_1 and λ_2 be Lagrangian multipliers. The Lagrangian equation and the corresponding first order conditions are given below.

$$L_i = (Y - y_j + F) - \beta_i\big(Y - 2y_j\big) + \lambda_1 y_j + \lambda_2\big(Y/2 - y_j\big)$$

$$\frac{\partial L_i}{\partial y_j} = -1 + 2\beta_i + \lambda_1 - \lambda_2 = 0 \qquad (2.5)$$

$$\frac{\partial L_i}{\partial \lambda_1}: y_j \geq 0; \lambda_1 \geq 0;\ \lambda_1 y_j = 0$$

$$\frac{\partial L_i}{\partial \lambda_2}: \big(Y/2 - y_j\big) \geq 0; \lambda_2 \geq 0; \lambda_2\big(Y/2 - y_j\big) = 0$$

Case a: $\lambda_1 = 0, \lambda_2 = 0$. This implies $\beta_i = 1/2$, i.e., the dictator is indifferent between giving any amount between 0 and $Y/2$. But the second order condition does not hold.

Case b. $\lambda_1 > 0$, $\lambda_2 = 0$ and hence $Y/2 > y_j = 0$. In this case dictator keeps the whole amount. The required condition from (4) is $\beta_i < 1/2$.

Case c. $\lambda_1 = 0$, $\lambda_2 > 0$ and hence $y_j > 0$. Here the dictator gives $Y/2$. The required condition for this is $\beta_i > 1/2$.

Consequently, the equilibrium y_j is independent of F. Therefore under Fehr and Schmidt (1999) structure: $\frac{dy_j^*}{dF} = 0$; an increase in the common show-up fee does not have any effect on the giving behavior.

□

A.1.2 Ratio form inequality aversion: Proof of Proposition 2

Equation (2) can be rewritten as

$$u_i = a_i\big(F + Y - y_j\big) - b_i\big[\big(F + Y - y_j\big)/\big[\big(F + y_j\big) + \big(F + Y - y_j\big)\big] - 1/2\,\big]^2$$

The dictator would try to maximize u_i with respect to the giving decision (y_j) subject to $Y \geq y_j \geq 0$. Denote μ_1 and μ_2 as Lagrangian multipliers. The Lagrangian equation and the corresponding first order conditions are given below.

$$£_i = a_i\big(F + Y - y_j\big) - b_i\left[\frac{\big(F + Y - y_j\big)}{\{\big(F + y_j\big) + \big(F + Y - y_j\big)\}} - 1/2\right]^2 + \mu_1 y_j + \mu_2\big(Y - y_j\big)$$

$$\frac{\partial £_i}{\partial \mathrm{y_j}} = -\frac{2b_i}{(2\mathrm{F}+\mathrm{Y})^2}\mathrm{y_j} + \left[b_i\left\{\frac{\mathrm{Y}}{(2\mathrm{F}+\mathrm{Y})^2}\right\} - a_i\right] + \mu_1 - \mu_2 = 0 \qquad (2.6)$$

$$\frac{\partial £_i}{\partial \mu_1}: y_j \geq 0; \mu_1 \geq 0;\ \mu_1 y_j = 0$$

$$\frac{\partial £_i}{\partial \mu_2}: \big(\mathrm{Y} - y_j\big) \geq 0; \mu_2 \geq 0; \mu_2\big(\mathrm{Y} - y_j\big) = 0$$

Case a. $\mu_2 = 0, \mu_1 > 0$ and hence $y_j = 0$, i.e., the dictator gives nothing. From (6) observe that $\mu_1 > 0$ implies $a_i(2F + Y)^2 > b_i Y$. Hence, the required restriction becomes $F \geq \big[(b_i Y/a_i)^{1/2}/2 - Y\big]$.

Case b. $\mu_1 = 0, \mu_2 > 0$ and hence $y_j = Y$, i.e., the dictator gives the whole pie. From (2.6) observe that $\mu_2 > 0$ implies $0 > Y + a_i\big(F_i + F_j + Y\big)^2/b_i$. This is not possible.

Case c. $\mu_1 = \mu_2 = 0$, i.e., an interior solution. Solving we get $y_j = \frac{1}{2b_i}[b_i Y - a_i(2F + Y)^2]$ $= \frac{1}{2}Y - \frac{a_i}{2b_i}(2F + Y)^2$. Hence, this boils down to $y_j = \frac{1}{2}Y - \frac{a_i}{2b_i}(2F + Y)^2$, with required restrictions $F < \big(\sqrt{(b_i/a_i)Y} - Y\big)/2$ and $(a_i/b_i) > Y$. It is easy to check that the SOC holds. The equilibrium giving implies $\frac{dy_j^*}{dF} < 0$; i.e., an increase in the common show-up fee will result in a lower giving in the interior. □

A.1.3 The theory of Impure Altruism: Proof of Proposition 3

Following Andreoni (1989), define total payoff of the recipient as $x_j = F_j + y_j$. Then equation (3) can be rewritten as:

$$u_i = u_i\big(F_i + F_j + Y - x_j, x_j, x_j - F_j\big)$$

Assuming interior solution, the optimum level of x_j can be solved by differentiating the above equation with respect to x_j and setting it equal to zero. Hence, the solution can be written as the following implicit function:

$$x_j = f_i\big(F_i + F_j + Y, F_j\big)$$

Where the first argument reflects the altruism component and the second argument reflects the warm-glow component of the utility function. Subtracting F_j from both sides we get

$$y_j^* = f_i\left(F_i + F_j + Y, F_j\right) - F_j \qquad (2.4)$$

When both own consumption and charity are normal goods, then one can argue following Andreoni (1989, pp. 1451) that for the case of non-neutral transfers: $1 \geq \frac{dy_j^*}{dF_i} \geq -\frac{dy_j^*}{dF_j} \geq 0$.

Now differentiating (4) with respect to F we find:

$$\frac{dy_j^*}{dF} = f_{i1}\frac{dF_i}{dF} + f_{i1}\frac{dF_j}{dF} + f_{i2}\frac{dF_j}{dF} - \frac{dF_j}{dF}$$

Where f_{i1} is the partial derivative of the function f_i with its first argument, and f_{i2} is the partial derivative for the second argument. So if both $F_i = F_j = F$, then $\frac{dF_i}{dF} = \frac{dF_j}{dF} = 1$. Given this and imposing the condition $1 \geq \frac{dy_j^*}{dF_i} \geq -\frac{dy_j^*}{dF_j} \geq 0$, we get:

$$\frac{dy_j^*}{dF} = 2f_{i1} + f_{i2} - 1 \geq 0$$

Hence, y_j^* is increasing in F; i.e., an increase in the common show-up fee will result in a higher giving in the interior. □

A.2 Instructions for the experiment

(Baseline case: £10 participation fee)

General Instruction

This is an experiment in the area of economic decision making. Various research agencies have provided funds for this research. The instructions are simple. If you follow them closely, then depending on your decision and the decision of the others, you can earn an appreciable amount of money. The experiment has two parts. At the end of today's experiment, you will be paid in private and in cash. Your identity and your decisions will also remain private. 16 participants are in today's experiment.

It is very important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your cooperation.

Your Decisions

You have already received a £10.00 participation fee. This experiment contains the decision problem that requires you to make economic choices that determine your earnings over and above your participation fee.

At the beginning of the experiment, you will be randomly and anonymously placed into one of 8 groups (groups 1, 2, 3, 4, 5, 6, 7, and 8). Each group consists of 2 types of participants '**Participant A**' and '**Participant B**'. Again you will be randomly assigned either as a '**Participant A**' or a '**Participant B**' in your group. Both the group name and your type will be written in a card given to you at the start of the experiment. Other participants will not know your group number or your type (A or B).

Both '**Participant A**' and '**Participant B**' are paid £10 each as their respective participation fee. Every Participant A will receive an additional amount of £10.

Part I. Participant A

Participant A will make the decision to allocate this additional £10 between himself / herself and the Participant B in his/her group. Participant A can decide to give any amount in British Pounds, between 0.00 and 10.00 (up to two decimal points), to Participant B. Suppose Participant A gives X to Participant B. Then Participant A will have the remaining Y= £10.00 - X. The total earnings of Participant A will be the participation fee plus the share of the additional £10. Hence, earnings of Participant A = £10 + Y. Earnings of Participant B = £10 + X. See the following examples for clarification. All the numbers are in British Pounds:

Example 1. Suppose Participant A decides to give 7.29 to Participant B. Then the total earnings of Participant B is (participation fee + share of the additional amount) = 10 + 7.29 = 17.29. And the total earnings of the Participant A is = 10 + (10 - 7.29) = 10 + 2.71 = 12.71.

Example 2. Suppose Participant A decides to give 3.37 to Participant B. Then the total earnings of Participant B is (participation fee + share of the additional amount) = 10 + 3.37 = 13.37. And the total earnings of the Participant A is = 10 + (10 - 3.37) = 10 + 6.63 = 16.63.

Every participant will get a card at the start of the experiment. **Line 1** of the card indicates your group number. **Line 2** indicates your role in the experiment. **Line 3** shows your participation fee. **Line 4** shows the participation fee of the other participant in your group. **Line 5** shows the additional amount (£10.00) given to Participant A to be allocated between himself/herself and the Participant B in the same group. The next lines are different for Participant A and Participant B.

Participant A's card looks like the one given below. In **Line 6**, Participant A will write a number between £0.00 and £10.00 (up to 2 decimal points) in the blank space. This is the amount given to Participant B. In **Line 7**, Participant A will calculate the amount left for him/her. To calculate this, Participant A will subtract the amount written in line 6 from £10. **Line 8** shows Participant A's total earnings. This will be the participation fee plus the share of the additional £10. Hence, Participant A will **add line 3 and line 7 and write the number in line 8**. Finally, in **line 9**, Participant A calculates the total earnings of Participant B, which is the **sum of line 4 and line 6**.

1. Your group number: **8**
2. Your role: **Participant A**
3. Your participation fee: £**10**
4. Participation fee of Participant B: £**10**
5. Additional amount to be allocated: £**10**
6. Amount given to Participant B (between 0.00 and 10.00): X =_____
7. Amount left for you: £10 - X = _____
8. Your total earnings: **£10 +** _____ **=** _____
9. Participant B total earnings: **£10 +** ____ **=** ____

Here is an **example** that draws numbers from Example 1 in page 2.

1. Your group number: **8**
2. Your role: **Participant A**
3. Your participation fee: £**10**
4. Participation fee of Participant B: £**10**
5. Additional amount to be allocated: £**10**
6. Amount given to Participant B (between 0.00 and 10.00): X = £7.29
7. Amount left for you: £10 - X = £2.71
8. Your total earnings: **£10** + £2.71 = £12.71
9. Participant B total earnings: **£10** + £7.29 = £17.29

Here is another **example** that draws numbers from Example 2 in page 2.

1. Your group number: **8**
2. Your role: **Participant A**
3. Your participation fee: £**10**
4. Participation fee of Participant B: £**10**
5. Additional amount to be allocated: £**10**
6. Amount given to Participant B (between 0.00 and 10.00): X = £3.37
7. Amount left for you: £10 - X = £6.63
8. Your total earnings: **£10** + £6.63 = £16.63
9. Participant B total earnings: **£10** + £3.37 = £13.37

Participant A will get 2 minutes to make his/her decision. After making the decision, each Participant A will put his/her card inside the envelope given and seal the envelope.

To summarize, if you are Participant A, make your decision and fill out the card. But if you are Participant B, you do not have to do anything in this part of the experiment. The total earnings of Participant A will be the sum of the participation fee, and the residual amount from the additional £10 (after giving an amount to Participant B), as calculated in **line 8**. Participant A's earnings will not be affected by the decisions of participant B in the next round. This will conclude the first part of the experiment. **Are there any questions?**

Part II. Participant B

Participant B's card looks like the one given below. **Line 6** indicates participant B's guess about the amount offered to Participant B by Participant A. Line 7 shows the total guessed earnings of Participant B, which is the sum of line 3 and line 6.

1. Your group number: **8**
2. Your role: **Participant B**
3. Your participation fee: £**10**
4. Participation fee of Participant A: £**10**
5. Total amount to be divided: £**10**
6. Your guess about the amount offered to you (between 0.00 and 10.00): ____
7. Your guess about your total earnings: **£10 +** ____ **=** ____

In the previous part of the experiment, Participant A decided to give any amount between £0.00 and £10.00 (up to two decimal points) to Participant B. In this part of the experiment, Participant B will have to guess the amount Participant A has given to him/her. If the guess is close enough to the actual amount given by Participant A, then Participant B will get an extra reward of £1.

Suppose Participant A has given X to Participant B. Participant B guesses that the amount is Z. If the difference between X and Z is less than or equal to 50 Pence, then Participant B will get the £1 reward over and above the participation fee and the amount given by Participant A.

Example 1. Suppose Participant A decides to give £7.29 to Participant B. If Participant B rightfully guesses an amount which is in between £6.79 and £7.79, then Participant B will get the reward of £1. This is because £7.29 - £0.5 = £6.79 and £7.29 + £0.5 = £7.79. If Participant B guesses numbers outside this range, then he/she will not get the reward.

Example 2. Suppose Participant A decides to give £3.37 to Participant B. If Participant B rightfully guesses an amount which is in between £2.87 and £3.87, then Participant B will get the reward of £1. This is because £3.37 - £0.5 = £2.87 and £3.37 + £0.5 = £3.87. If Participant B guesses numbers outside this range, then he/she will not get the reward.

Participant B will write the guess in **Line 6**. He/she will also need to write the total earnings in **line 7**. This will be the **sum of line 3 and line 6**. Participant B will get 2 minutes to make his/her decision. After making the decision, each Participant B will put his/her card inside the envelope given and seal the envelope. The total earnings of Participant B will be the sum of the participation fee, amount given to him/her by Participant A, and the £1 reward (if won). This will conclude the second part of the experiment. **Are there any questions?**

Appendix B (appendix Chapter 3)

B.1 Instructions for the experiment

(Baseline case: £10 participation fee)

General Instruction

This is an experiment in the area of economic decision making. Various research agencies have provided funds for this research. The instructions are simple. If you follow them closely, then depending on your decision and the decision of the others, you can earn an appreciable amount of money. The experiment has two parts. At the end of today's experiment, you will be paid in private and in cash. Your identity and your decisions will also remain private. 16 participants are in today's experiment.

It is very important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your cooperation.

Your Decisions

You have already received a £10.00 participation fee. This experiment contains the decision problem that requires you to make economic choices that determine your earnings over and above your participation fee.

At the beginning of the experiment, you will be randomly and anonymously placed into one of 8 groups (groups 1, 2, 3, 4, 5, 6, 7, and 8). Each group consists of 2 types of participants '**Participant A**' and '**Participant B**'. Again you will be randomly assigned either as a '**Participant A**' or a '**Participant B**' in your group. Both the group name and your type will be written in a card given to you at the start of the experiment. Other participants will not know your group number or your type (A or B).

Both '**Participant A**' and '**Participant B**' are paid £10 each as their respective participation fee. Every Participant A will receive an additional amount of £10.

Part I. Participant A

Participant A will make the decision to allocate this additional £10 between himself / herself and the Participant B in his/her group. Participant A can decide to give any amount in British Pounds, between 0.00 and 10.00 (up to two decimal points), to Participant B. Suppose Participant A gives X to Participant B. Then Participant A will have the remaining Y= £10.00 - X. The total earnings of Participant A will be the participation fee plus the share of the additional £10. Hence, earnings of Participant A = £10 + Y. Earnings of Participant B = £10 + X. See the following examples for clarification. All the numbers are in British Pounds:

Example 1. Suppose Participant A decides to give 7.29 to Participant B. Then the total earnings of Participant B is (participation fee + share of the additional amount) = 10 + 7.29 = 17.29. And the total earnings of the Participant A is = 10 + (10 - 7.29) = 10 + 2.71 = 12.71.

Example 2. Suppose Participant A decides to give 3.37 to Participant B. Then the total earnings of Participant B is (participation fee + share of the additional amount) = 10 + 3.37 = 13.37. And the total earnings of the Participant A is = 10 + (10 - 3.37) = 10 + 6.63 = 16.63.

Every participant will get a card at the start of the experiment. **Line 1** of the card indicates your group number. **Line 2** indicates your role in the experiment. **Line 3** shows your participation fee. **Line 4** shows the participation fee of the other participant in your group. **Line 5** shows the additional amount (£10.00) given to Participant A to be allocated between himself/herself and the Participant B in the same group. The next lines are different for Participant A and Participant B.

Participant A's card looks like the one given below. In **Line 6**, Participant A will write a number between £0.00 and £10.00 (up to 2 decimal points) in the blank space. This is the amount given to Participant B. In **Line 7**, Participant A will calculate the amount left for him/her. To calculate this, Participant A will subtract the amount written in line 6 from £10. **Line 8** shows Participant A's total earnings. This will be the participation fee plus the share of the additional £10. Hence, Participant A will **add line 3 and line 7 and write the number in line 8**. Finally, in **line 9**, Participant A calculates the total earnings of Participant B, which is the **sum of line 4 and line 6**.

10. Your group number: **8**
11. Your role: **Participant A**
12. Your participation fee: £**10**
13. Participation fee of Participant B: £**10**
14. Additional amount to be allocated: £**10**
15. Amount given to Participant B (between 0.00 and 10.00): X =_____
16. Amount left for you: £10 - X = _____
17. Your total earnings: **£10 +** _____ **=** _____
18. Participant B total earnings: **£10 +** ____ **=** ____

Here is an **example** that draws numbers from Example 1 in page 2.

10. Your group number: **8**
11. Your role: **Participant A**
12. Your participation fee: £**10**
13. Participation fee of Participant B: £**10**
14. Additional amount to be allocated: £**10**
15. Amount given to Participant B (between 0.00 and 10.00): X = £7.29
16. Amount left for you: £10 - X = £2.71
17. Your total earnings: **£10** + £2.71 = £12.71
18. Participant B total earnings: **£10** + £7.29 = £17.29

Here is another **example** that draws numbers from Example 2 in page 2.

10. Your group number: **8**
11. Your role: **Participant A**
12. Your participation fee: £**10**
13. Participation fee of Participant B: £**10**
14. Additional amount to be allocated: £**10**
15. Amount given to Participant B (between 0.00 and 10.00): X = £3.37
16. Amount left for you: £10 - X = £6.63
17. Your total earnings: **£10** + £6.63 = £16.63
18. Participant B total earnings: **£10** + £3.37 = £13.37

Participant A will get 2 minutes to make his/her decision. After making the decision, each Participant A will put his/her card inside the envelope given and seal the envelope.

To summarize, if you are Participant A, make your decision and fill out the card. But if you are Participant B, you do not have to do anything in this part of the experiment. The total earnings of Participant A will be the sum of the participation fee, and the residual amount from the additional £10 (after giving an amount to Participant B), as calculated in **line 8**. Participant A's earnings will not be affected by the decisions of participant B in the next round. This will conclude the first part of the experiment. **Are there any questions?**

Part II. Participant B

Participant B's card looks like the one given below. **Line 6** indicates participant B's guess about the amount offered to Participant B by Participant A. Line 7 shows the total guessed earnings of Participant B, which is the sum of line 3 and line 6.

8. Your group number: **8**
9. Your role: **Participant B**
10. Your participation fee: £**10**
11. Participation fee of Participant A: £**10**
12. Total amount to be divided: £**10**
13. Your guess about the amount offered to you (between 0.00 and 10.00): ____
14. Your guess about your total earnings: **£10 +** ____ **=** ____

In the previous part of the experiment, Participant A decided to give any amount between £0.00 and £10.00 (up to two decimal points) to Participant B. In this part of the experiment, Participant B will have to guess the amount Participant A has given to him/her. If the guess is close enough to the actual amount given by Participant A, then Participant B will get an extra reward of £1.

Suppose Participant A has given X to Participant B. Participant B guesses that the amount is Z. If the difference between X and Z is less than or equal to 50 Pence, then Participant B will get the £1 reward over and above the participation fee and the amount given by Participant A.

Example 1. Suppose Participant A decides to give £7.29 to Participant B. If Participant B rightfully guesses an amount which is in between £6.79 and £7.79, then Participant B will get the reward of £1. This is because £7.29 - £0.5 = £6.79 and £7.29 + £0.5 = £7.79. If Participant B guesses numbers outside this range, then he/she will not get the reward.

Example 2. Suppose Participant A decides to give £3.37 to Participant B. If Participant B rightfully guesses an amount which is in between £2.87 and £3.87, then Participant B will get the reward of £1. This is because £3.37 - £0.5 = £2.87 and £3.37 + £0.5 = £3.87. If Participant B guesses numbers outside this range, then he/she will not get the reward.

Participant B will write the guess in **Line 6**. He/she will also need to write the total earnings in **line 7**. This will be the **sum of line 3 and line 6**. Participant B will get 2 minutes to make his/her decision. After making the decision, each Participant B will put his/her card inside the envelope given and seal the envelope. The total earnings of Participant B will be the sum of the participation fee, amount given to him/her by Participant A, and the £1 reward (if won). This will conclude the second part of the experiment. **Are there any questions?**

Appendix C (appendix Chapter 4)

C.1 Table

Table A.1 Average (standard deviations) donations by preference type

		Baseline (No Rebate)	Exogenous Rebate	Endogenous Rebate
Low Endowment	Social	2050.19 (2797.47)	1694.5 (2455.56)	716.67 (1070.05)
	Selfish	2506.25 (3521.45)	1763.89 (2314.39)	1288.89 (1304.69)
High Endowment	Social	3805.56 (3339.33)	2666.67 (2869.72)	4444.44 (3395.02)
	Selfish	1972.22 (2464.29)	3509.44 (4068.03)	2118.89 (2586.13)

C.2 Instructions for the experiment

(BSL-Low treatment)

Part I. Public good game

The purpose of this experiment is to study how individuals make decisions in certain contexts. The instructions are simple and if you follow them carefully you will receive a cash amount at the end of the experiment in a confidential manner, since no one will know the payments received by the other participants. You can ask at any time that you have raised doubts first hand. Out of these questions, any communication between you is prohibited and subject to immediate exclusion of the experiment.

1. The experiment consists of 20 rounds. In each and every one of the rounds are part of the same group of 4 participants, whose composition is determined randomly at the beginning of the experiment and does not vary along the same. At no time will know the identities of other members of your group.
2. At the beginning of each round, each participant receives an endowment of 100 ECU.
3. Your only decision is to choose how you assign the Collaborative Fund. The rest will be automatically allocated to Private Fund.
4. In each round, you will receive information from the appropriations to be made to the Collaborative Fund all members of your group listed from highest to lowest, but not know the origin of each assignment.
5. In determining the profits of the Collaborative Fund is calculated based on the sum of the allocations of all members of your group to the Fund (ie the sum of the allocations of the players 1, 2, 3 and 4 to the Collaborative Fund) . That amount of your group assignments Collective Fund is doubled and divided into four equal parts among the members of the group.
6. Private Fund benefits are equal to your allocation to the fund and not depend on the decisions of others.
7. At the end of each round, you will receive information about your current and past results regarding the benefit you get from the Collaborative Fund, the benefit you get from the Private Fund, your individual benefit and the benefit accrued to date.
8. At the end of the experiment you will be paid accrued benefits over the twenty rounds at the 2000 exchange rate of ECU = 1 €.

Part II. Dictator game

The purpose of this experiment is to study how individuals make decisions in certain contexts. The instructions are simple and if you follow them carefully you will receive a cash amount at the end of the experiment in a confidential manner, since no one will know the payments received by the other participants. You can ask at any time that you have raised doubts first hand. Out of these questions, any communication between you is prohibited and subject to immediate exclusion of the experiment.

1. The experiment consists of only 1 round, where you must decide how to distribute a strictly single 10,000 ECU (any integer from 0 to 10,000) between you and the NGO Support Organization SOS Children.
2. Your earnings at the end of the experiment will come determined only by your decision: ECUs surplus after making the donation will be paid in private at the end of the experiment at a rate of 2,000 ECU = 1 €.
3. To ensure the anonymity of all participants, at any time of the experiment will provide information for decisions other participants make. Similarly, your decision will not be known by anyone, at any time.
4. At the end of the experiment will access the website of the NGO and proceed to deposit the sum of amounts that have been assigned. To ensure the procedure, randomly select a person to monitor that the process is carried out.

Appendix D (appendix Chapter 5)

D.1 Table

Table A.2 Incidence of free-riding by treatment

# of free riding	Baseline	Color Treatment	Race Treatment
0	16	24	23
1	11	8	13
2	6	7	5
3	8	3	6
4	3	4	6
5	3	4	5
6	4	3	4
7	4	1	1
8	4	1	1
9	3		1
10		1	2
11		2	1
12			
13	1	1	
14		3	
15	3	1	
16	1	1	1
17	2	2	1
18	1	1	
19		2	
20	2	3	2

D.2 Instructions for the experiment

GENERAL INSTRUCTIONS

This is an experiment in the economics of decision making. The instructions are simple. If you follow them closely and make appropriate decisions, you can earn an appreciable amount of money.

Experimental Currency is used in the experiment and your decisions and earnings will be recorded in Experimental Currency Units (ECUs). At the end of today's experiment, you will be paid in private and in cash. ECUs will be converted to Pound Sterling at a rate of **25** ECUs to **1** British Pound.

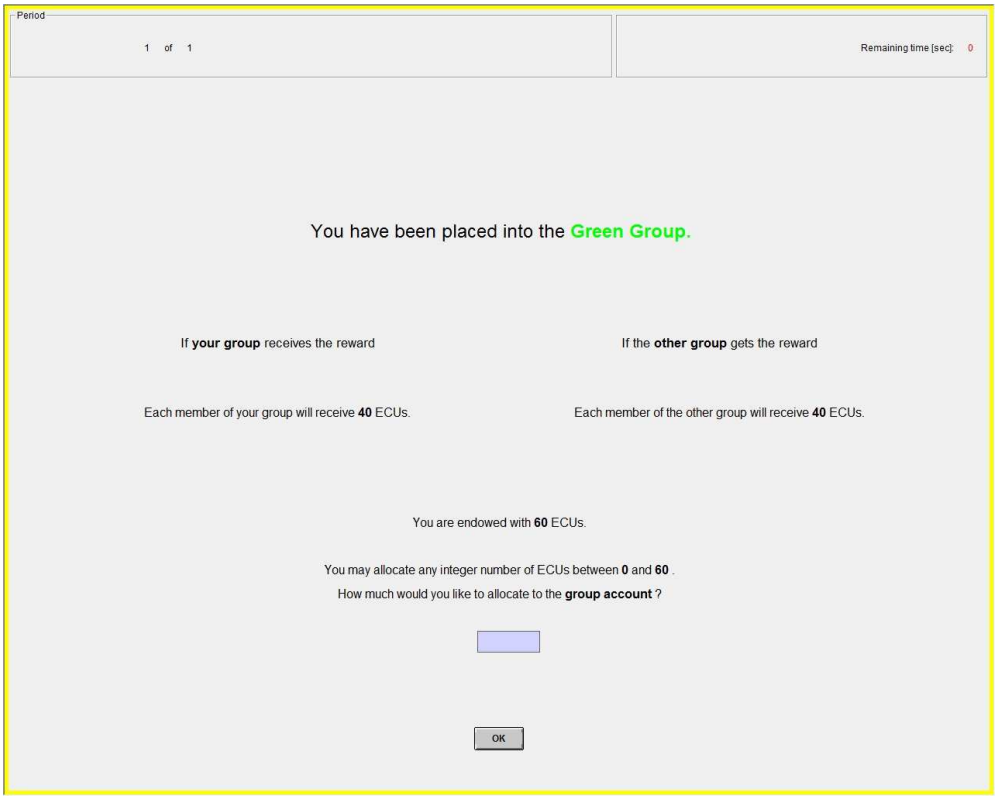
It is extremely important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your cooperation.

EXPERIMENTAL INSTRUCTIONS

YOUR DECISION

The experiment consists of 20 decision-making periods. At the beginning of the experiment, you will be anonymously placed into a group of **3 people**. You will be placed in either a 'Green' group or a 'Blue' group. Your group will then be anonymously matched with another group of 3 people. If you are placed into a Blue group, then your group will be matched with a Green group. If you are placed into a Green group, then your group will be matched with a Blue group. In each period your group ***as well as*** the group your group is matched with will remain ***the same***. However, at no point will you know who your group members are or who the members of the other group are. Also, you will not know any information about the members of your group or the members of the other group.

Each period you will be given an initial endowment of **60** ECUs. You will then decide how much to allocate to a **group account** or an **individual account**. On your screen, you will be asked to enter your allocation to the group account. You may allocate any integer number of ECUs between, and including, **0** and **60**. Any ECUs you do not allocate to the group account will automatically be allocated to your individual account. An example of your decision screen is shown below.



At the end of ***each*** period, either your group or the other group will receive a reward of **120** ECUs (40 ECUs per group member). In each period, only one of the two groups can obtain the reward. By contributing to your group account you increase the **chance** of receiving the reward for your group. If the total number of ECUs in your group account exceeds the total number of ECUs in the other group's account, your group has a **higher chance** of receiving the reward.

The computer will assign the reward either to your group or to the other group, **via a random draw** that depends on the total allocation in the group accounts by the two groups. Below is a hypothetical example used to illustrate how the computer makes a random draw to decide which group wins the reward.

Note: The following example is for illustrative purposes only.

Example 1. Random Draw

Think of the random draw in the following way. For **each** ECUs in your group's account the computer puts **1 red token** into a box and for each ECU in the other group's account the computer puts **1 black token**. Then the computer randomly draws one token out of the box. If the drawn token is red then your group receives the reward, if the drawn token is black then

the other group receives the reward. Suppose that members of both groups have allocated their ECUs in the following way (as shown in Table 1 below).

Table 1 – Allocation of ECUs by members of both groups

Your Group	Endow-ment (ECUs)	Allocation to the individual account	Allocation to the group account
Person 1	60	40	**20**
Person 2	60	45	**15**
Person 3	60	50	**10**
Total	180	135	**45**

Other Group	Endow-ment (ECUs)	Allocation to the individual account	Allocation to the group account
Person 1	60	50	**10**
Person 2	60	60	**0**
Person 3	60	55	**5**
Total	180	165	**15**

Members of your group have allocated a total of **45** ECUs to your group account while members of the other group have allocated **15** ECUs. Thus, the computer will place **45 red tokens** and **15 black tokens** into the box (**60 tokens total**). Then the computer will randomly draw one token out of the box. You can see that since your group has contributed more it has a **higher chance** of receiving the reward - your group will receive the reward **45 out of 60** times. The other group has a lower chance of receiving the reward - **15 out of 60** times.

A group can never guarantee itself the reward. However, by increasing your bid, you can increase your group's chance of receiving the reward. If your group receives the reward, 120 ECUs will be divided equally among the members of your group, i.e., you and the other 2 members of your group will receive 40 ECUs each.

YOUR EARNINGS

EARNINGS IN EACH PERIOD:

After all participants have made their decisions, your earnings for the period are calculated.

1) For each ECU in your individual account, you will earn **1** ECU in return. So, if you keep all 60 ECUs that you are endowed with in your individual account you will earn 60 ECUs.

2) You can also earn some ECUs from your group account. After all bids are made, the computer uses the random draw process described above to decide which group wins the reward. If your group wins the reward, you will earn **40** ECUs from your group account in addition to your earnings from your individual account. Each of the other 2 members of your group will **also** earn **40** ECUs from the group account. If the other group wins the reward, you and the other 2 members of your group receive nothing from your group account. In this event, your period earnings will be equal to your earnings from your individual account.

Your period earnings are the **sum of the earnings** from your **individual account** and the earnings from your **group account**. The following example illustrates the calculation of period earnings.

Note: The following example is for illustrative purposes only.

Example 2. Period Earnings

In Example 1, your group allocated a total of 45 ECUs while other group allocated a total of 15 ECUs to the group accounts. Let's say the computer made a random draw and **your group received the reward**. Thus, all the members of your group receive 40 ECUs each from **your group account** plus earnings from their **individual accounts**. All members of the other group receive earnings **only from their individual accounts**, since their group did not receive the reward. The calculation of the total earnings is shown in Table 2 below.

Table 2 – Calculation of earnings for both groups

Your group	Earnings from group account	Earnings from individual account	Total period earnings
Person 1	**40**	**40**	40+40 = **80**
Person 2	**40**	**45**	40+45 = **85**
Person 3	**40**	**50**	40+50 = **90**
Total	**120**	**135**	255

Other group	Earnings from group account	Earnings from individual account	Total period earnings
Person 1	**0**	**50**	**50**
Person 2	**0**	**60**	**60**
Person 3	**0**	**55**	**55**
Total	**0**	**165**	165

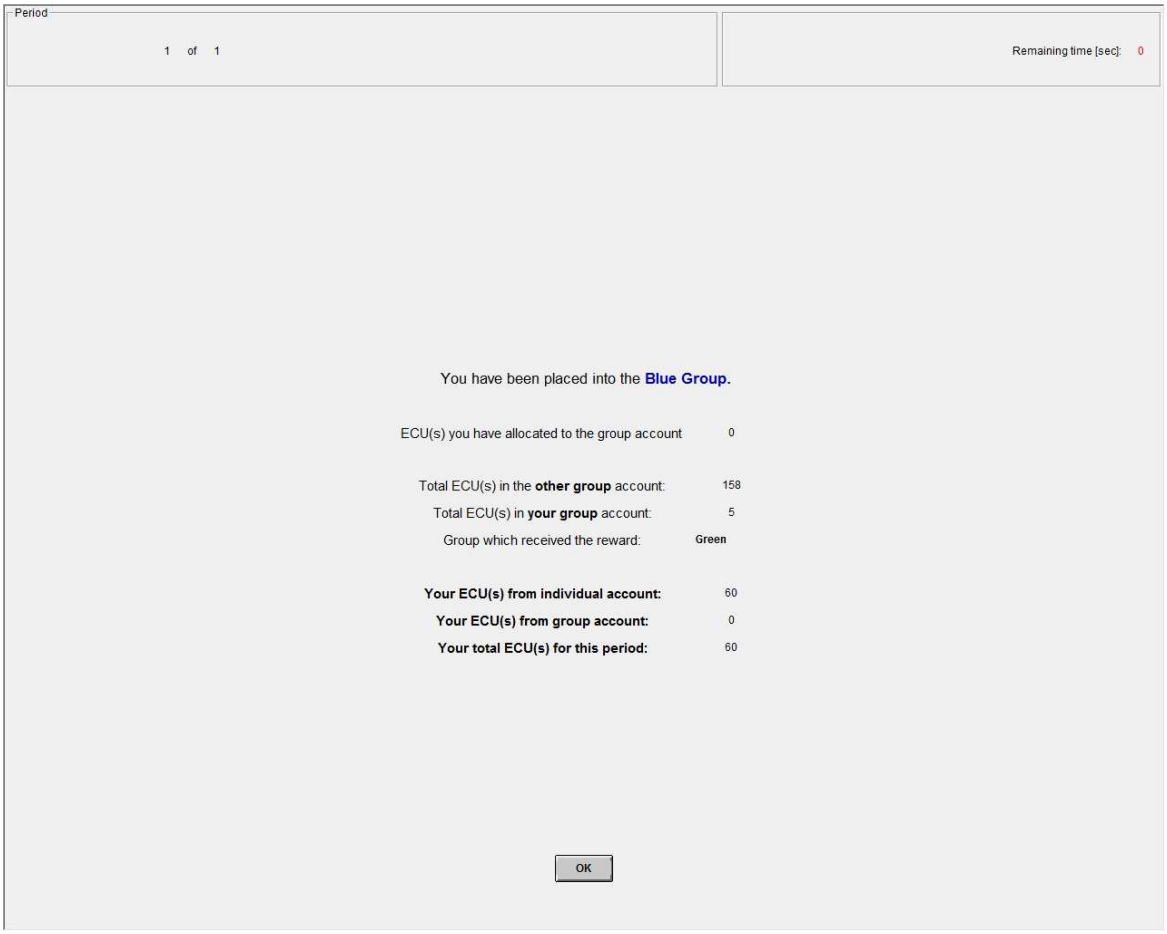
EARNINGS FROM THE EXPERIMENT:

At the end of the experiment we will randomly choose **5 of the 20** periods for actual payment using a computer program. You will be paid the sum of the earnings in each of these 5 periods. These earnings will be converted to cash at the exchange rate mentioned earlier and will be paid at the end of the experiment.

Note: All participants in this session will be paid for the same 5 periods.

OUTCOME SCREEN

At the end of each period, the total number of ECUs in the two groups' accounts, which group received the reward, your earnings from your individual and your group accounts, and your total earnings for the period are reported on the outcome screen as shown below. Please record your results for the period on your **record sheet** under the appropriate heading.



QUESTIONS TO HELP YOU BETTER UNDERSTAND THE DECISION TASKS

When everyone has finished reading the instructions, and before the experiment begins, we will ask you a few questions regarding the decisions you will make in the experiment. The questions will help you understand the calculation of your earnings and ensure that you have understood the instructions.

ARE THERE ANY QUESTIONS?

Personal Record Sheet

Period	Earnings from individual account	Earnings from your group account	Total earnings for this period
1			
2			
3			
4			
5			
6			
7			
8			
9			
10			
11			
12			
13			
14			
15			
16			
17			
18			
19			
20			

Total Earnings

Period Chosen	Total earnings for this period
Total	=

Sum earnings from table above: _____

Divide earnings by conversion rate: **÷ 25**

Earnings in British Pounds: £_____ (1)

Earnings from Showing up: **£2** **(2)**

Total payment received: (1)+(2) £_____

QUIZ

1. Does group composition change across periods in the experiment?

Ans. Yes No

Questions 2 to 6 apply to the following information.

In a given period, suppose the members of your group and the other group chose to allocate their ECUs to the group account as it is shown in the table below.

Your group	Endow-ment	Allocation to the individual account	Allocation to the group account
Person 1	60	35	25
Person 2	60	60	0
Person 3	60	55	5
Total	180	150	30

Other group	Endow-ment	Allocation to the individual account	Allocation to the group account
Person 1	60	40	20
Person 2	60	40	20
Person 3	60	50	10
Total	180	130	50

2. How many **total ECUs** will the computer place into the box?

Ans. _____

3. If the computer makes a random draw out of the box what is the **chance of your group** receiving the reward?

Ans. _____ out of _____

4. If the computer makes a random draw out of the box what is the **chance of the other group** receiving the reward?

Ans. _____ out of _____

5. If you are Person 1 in your group and your group **did not receive** the reward what are your period earnings?

Ans. _____

6. If you are Person 2 in your group and your group **received** the reward what are your period earnings?

Ans. _____

EXPLANATIONS FOR QUIZ ANSWERS

1. Does group composition change across periods in the experiment? Correct answer: No

Questions 2 to 6 apply to the following information.

In a given period, suppose the members of your group and the other group chose to allocate their ECUs to the group account as it is shown in the table below.

Your group	Endow-ment	Allocation to the individual account	Allocation to the group account
Person 1	60	35	25
Person 2	60	60	0
Person 3	60	55	5
Total	180	150	30

Other group	Endow-ment	Allocation to the individual account	Allocation to the group account
Person 1	60	40	20
Person 2	60	40	20
Person 3	60	50	10
Total	180	130	50

2. How many **total ECUs** will the computer place into the box? Correct answer: 80

Allocation to the group account by your group and by the other group, i.e., 30 from your group PLUS 50 from the other group.

3. If the computer makes a random draw out of the box what is the **chance of your group** receiving the reward? – Correct answer: 30 out of 80

Out of a total of 80 tokens, 30 belong to your group. Thus the chance of your group winning any random draw of one token from the box is 30 tokens out of 80.

4. If the computer makes a random draw out of the box what is the **chance of the other group** receiving the reward? – Correct answer: 50 out of 80

Out of a total of 80 tokens, 50 belong to the other group. Thus the chance of the other group winning any random draw of one token from the box is 50 tokens out of 80.

5. If you are Person 1 in your group and your group **did not receive** the reward what are your period earnings? Correct answer: 35

Since your group did not win the reward, your earnings from the group account for this period are zero. So, your period earnings are equal to your earnings from your individual account. From the above table, this is equal to 35 ECUs, your allocation to your individual account.

6. If you are Person 2 in your group and your group **received** the reward what are your period earnings? Correct answer: 100

Since your group did win the reward, your earnings from the group account for this period are **40** ECUs (Your group wins 120 ECUs which are split equally among all 3 of you). So, your period earnings are equal to your earnings from your individual account plus 40 ECUs (your earnings from the group account). From the above table, your allocation to your individual account is 60. Thus your total period earnings are 60 + 40 = 100 ECUs.

Appendix E (appendix Chapter 6)

E.1 Instructions for the experiment

Appendix: Instructions

1. Baseline instruction for Dictator

Welcome to this experiment! In this experiment each of you will be paired with a different person. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Your decisions will be strictly anonymous and cannot be linked to you in any way.

The experiment has two parts and is conducted as follows:

- Everyone in this room has already been allocated a show up fee of £3. You have been paired with someone else in the room.
- The other person you are paired with has access to an additional £10.
- In the first part of the experiment, you will have to make a simple decision. You have to decide what portion, if any, of the £10 to transfer to yourself. Your choice can be anywhere from £0 to £10, in 1p increments. Your take-home earnings from this experiment will be your initial £3 show up fee plus the money you transfer from the person you are paired with. The earnings of the person you are paired with will be his/her £3 show up fee plus the money left over from the £10 after you transfer to yourself.
- In the second part of the experiment, the person you are paired with will make a decision, but that decision will NOT affect your earnings.

You will have 1 minute to come to a decision about your choice. Please do not talk to the other people in this room until your session is completed. Do not be concerned if other people make their decisions before you.

2. Eye-image instruction for Dictator

Welcome to this experiment! In this experiment each of you will be paired with a different person. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Your decisions will be strictly anonymous and cannot be linked to you in any way.

The experiment has two parts and is conducted as follows:

- Everyone in this room has already been allocated a show up fee of £3. You have been paired with someone else in the room.
- The other person you are paired with has access to an additional £10.
- In the first part of the experiment, you will have to make a simple decision. You have to decide what portion, if any, of the £10 to transfer to yourself. Your choice can be anywhere from £0 to £10, in 1p increments. Your take-home earnings from this experiment will be your initial £3 show up fee plus the money you transfer from the person you are paired with. The earnings of the person you are paired with will be his/her £3 show up fee plus the money left over from the £10 after you transfer to yourself.
- In the second part of the experiment, the person you are paired with will make a decision, but that decision will NOT affect your earnings.

You will have 1 minute to come to a decision about your choice. Please do not talk to the other people in this room until your session is completed. Do not be concerned if other people make their decisions before you.

Appendix F (appendix Chapter 7)

F.1 Instructions for the experiment

1. Instruction for Dictator in Taking game

Welcome to this experiment! In this experiment each of you will be paired with a different person. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Your decisions will be strictly anonymous and cannot be linked to you in any way.

The experiment has two parts and is conducted as follows:

- Everyone in this room has already been allocated a show up fee of £3. You have been paired with someone else in the room.
- The other person you are paired with has access to an additional £10.
- In the first part of the experiment, you will have to make a simple decision. You have to decide what portion, if any, of the £10 to transfer to yourself. Your choice can be anywhere from £0 to £10, in 1p increments. Your take-home earnings from this experiment will be your initial £3 show up fee plus the money you transfer from the person you are paired with. The earnings of the person you are paired with will be his/her £3 show up fee plus the money left over from the £10 after you transfer to yourself.
- In the second part of the experiment, the person you are paired with will make a decision, but that decision will NOT affect your earnings.

You will have 1 minute to come to a decision about your choice. Please do not talk to the other people in this room until your session is completed. Do not be concerned if other people make their decisions before you.

2. Instruction for Dictator in Giving game

Welcome to this experiment! In this experiment each of you will be paired with a different person. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Your decisions will be strictly anonymous and cannot be linked to you in any way.

The experiment has two parts and is conducted as follows:

- Everyone in this room has already been allocated a show up fee of £3. You have been paired with someone else in the room.
- You have access to an additional £10. The other person you are paired with does not have access to that extra £10.
- In the first part of the experiment, you will have to make a simple decision. You have to decide what portion, if any, of the £10 to transfer to the person you are paired with. Your choice can be anywhere from £0 to £10, in 1p increments. Your take-home earnings from this experiment will be your initial £3 show up fee plus the money left over from the £10 after you transfer to the person you are paired with. The earnings of the person you are paired with will be the amount you transfer to him/her plus his/her £3 show up fee.
- In the second part of the experiment, the person you are paired with will make a decision, but that decision will NOT affect your earnings.

You will have 1 minute to come to a decision about your choice. Please do not talk to the other people in this room until your session is completed. Do not be concerned if other people make their decisions before you.