

Essays on Antisocial Preferences, Altruism, and Information Transmission

Graciela Zevallos Porles

Thesis submitted for the degree of

Doctor of Philosophy in

Economics

University of East Anglia

February 2018

"This copy of the thesis has been supplied on condition that anyone who consults it is understood to recognise that its copyright rests with the author and that use of any information derived therefrom must be in accordance with current UK Copyright Law. In addition, any quotation or extract must include full attribution."

Contents

Acknowledgements	x
Abstract	xi
Introduction	1
Chapter 1: An Experimental Test of the Nastiness Hypothesis	8
1. Introduction	8
2. Related literature.....	12
3. Experimental design and procedures	16
4. Results	23
5. Post-experimental questionnaire	26
6. Discussion and conclusion	27
References	30
Appendix	36
Appendix A. Experimental instructions	36
Appendix B. Experimental instructions for the neutral treatment	39
Appendix C. Background of the participants	40
Appendix D. Background of the participants in the neutral treatment	43
Appendix E. Post-experimental questionnaire	45
Appendix F. Gender differences in antisocial behaviour.....	46
Appendix G. Cultural differences in antisocial behaviour	48
Appendix H. Data	50
Chapter 2: On the Effects of the Relative Price, Monetary Stakes and Experimental Context on the Dictator's Decision	53
1. Introduction	53
2. Related literature.....	56

3. Experimental design and procedures	60
4. Theoretical predictions.....	69
5. Results	77
6. Discussion and conclusion	90
References	92
Appendix	101
Appendix A. Experimental instructions.....	101
Appendix B. Background of the participants.....	104
Appendix C. Post-experimental questionnaire.....	108
Appendix D. Additional analysis	109
Appendix E. Data.....	110

Chapter 3: The Effect of Ex-Post Information Acquisition in Sender Receiver Games: Experimental Evidence 113

1. Introduction	113
2. Related literature.....	117
3. Experimental design and procedures	119
4. Theoretical predictions.....	126
5. Results	130
6. Free-form messages	156
7. Discussion	166
8. Conclusion	168
References	169
Appendix	178
Appendix A. Experimental instructions for fixed messages	178
Appendix B. Experimental instructions for free messages	184
Appendix C. Background of the participants in fixed messages	186
Appendix D. Background of the participants in free messages	189
Appendix E. Belief elicitation's questionnaires	192
Appendix F. Post-experimental questionnaire	193
Appendix G. Additional analysis: intentional deception.....	194
Appendix H. Regression variables	196
Appendix I. Messages	197

List of Figures

Figures to Chapter 1:

Figure 1. Decision screen.....	20
Figure 2. Questionnaire screen for bilateral destruction treatments	21
Figure 3. Questionnaire screen for unilateral destruction treatment.....	21
Figure 4. Payoff screen for 2S-Costly treatment	22
Figure 5. Destruction choices.....	25
Figure 6. Post experimental questionnaire's screens	45
Figure 7. Destruction choices across gender and treatment	47
Figure 8. Destruction choices across culture and treatment	49

Figures to Chapter 2:

Figure 1. Payoffs available to the Dictator for each Dictator giving.....	62
Figure 2. Payoffs available to the Dictator for each Dictator taking.....	63
Figure 3. Payoffs available to the Dictator for each Dictator burning.....	65
Figure 4. Control questions' screens.....	67
Figure 5. Dictator task's screen for game 2.....	68
Figure 6. Histogram of the amount transferred (n=414)	78
Figure 7. Histogram of the amount taken (n=414)	79
Figure 8. Histogram of the amount burnt (n=414)	80
Figure 9. Histogram of the amount transferred by game (n=69 in each game).....	83
Figure 10. Histogram of the amount taken by game (n=69 in each game)	84
Figure 11. Histogram of the amount burnt by game (n=69 in each game)	85
Figure 12. Bubble plots for each Dictator game giving (n=69 in each game) ..	88
Figure 13. Bubble plots for each Dictator game taking (n=69 in each game) ..	89
Figure 14. Bubble plots for each Dictator game burning (n=69 in each game)	89
Figure 15. Post-experimental questionnaire's screens.....	108

Figures to Chapter 3:

Figure 1. Sender's decision screen	124
Figure 2. Receiver's decision screen	124
Figure 3. Receiver's screen for ex-post information decision about the payoffs	126
Figure 4. Fraction of Senders who expect the Receiver to follow the chosen message and Receivers who actually follow the Sender's message	133
Figure 5. Fraction of Receivers who expect the truth and Senders who actually send Message 1	134
Figure 6. Relative frequency of Senders who expect the Receiver to learn about the payoffs and Receivers who actually learn about the payoffs.....	136
Figure 7. Distribution of Receivers' messages	152
Figure 8. Distribution of Receivers' messages across treatments.....	152
Figure 9. Distribution of Receivers' messages across information decision..	153
Figure 10. Distribution of Receivers' messages across information decision and Sender's message.....	155
Figure 11. Distribution of Receivers' message across Sender's message	155
Figure 12. Distribution of Receivers' messages across message and chosen option.....	156
Figure 13. Sender's decision screen for free-text message treatments	158
Figure 14. Receiver's decision screen for free-text message treatments	159
Figure 15. Distribution of Senders' messages by category (n=74)	162
Figure 16. Distribution of Senders' messages for FreeMess-NoInfo (n=41) and FreeMess-EndoInfo-Costly (n=33).....	164
Figure 17. Distribution of Senders' messages across treatments in Option A (n=45) and option B (n=29)	164
Figure 18. Questionnaire screen for elicitation of Sender's beliefs for EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Gain.....	192
Figure 19. Questionnaire screen for elicitation of Receiver's beliefs about the Sender's message for NoInfo, ExoInfo, EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Gain.....	192

Figure 20. Post-experimental questionnaire's screens 193
Figure 21. Sender's expectations depending upon the chosen message 195

List of Tables

Tables to Chapter 1:

Table 1. Experimental treatments	18
Table 2. Experimental results	25
Table 3. Percentage of male and female subjects by treatment	40
Table 4. Percentage of subjects by country	40
Table 5. Percentage of subjects by field of study	41
Table 6. Descriptive statistics for age by treatment.....	42
Table 7. Descriptive statistics for experimental earnings by treatment.....	42
Table 8. Percentage of subjects by gender	43
Table 9. Percentage of subjects by country	43
Table 10. Descriptive statistics for age and experimental earnings	43
Table 11. Percentage of subjects by field of study	44
Table 12. Fraction of subjects who chose to reduce by treatment and gender	47
Table 13. Fraction of subjects who chose to reduce by treatment and nationality	48
Table 14. Data for 2S-Costly treatment	50
Table 15. Data for 2S-Costless treatment	50
Table 16. Data for 2S-Benefit treatment	51
Table 17. Data for 1S-Benefit treatment	51
Table 18. Data for Neutral treatment.....	52

Tables to Chapter 2:

Table 1. Dictator games giving.....	61
Table 2. Dictator games taking.....	63
Table 3. Dictator games burning.....	64
Table 4. Descriptive statistics: amount transferred to the Recipient (£).....	81
Table 5. Descriptive statistics: amount taken from the Recipient (£).....	81

Table 6. Descriptive statistics: amount burnt from the Recipient (£).....	82
Table 7. OLS regression on the amount transferred.....	86
Table 8. OLS regression on the amount taken	86
Table 9. OLS regression on the amount burnt	87
Table 10. Percentage of male and female subjects by session.....	104
Table 11. Descriptive statistics for age by session	104
Table 12. Percentage of subjects by field of study	105
Table 13. Percentage of subjects by country	106
Table 14. Descriptive statistics for experimental earnings by session.....	107
Table 15. Summary of the results.....	109

Tables to Chapter 3:

Table 1. The payoff matrix	119
Table 2. Experimental treatments	121
Table 3. Descriptive statistics (relative frequencies)	137
Table 4. Determinants of sending Message 2.....	140
Table 5. Determinants of following the Sender's message.....	142
Table 6. Determinants of learning about the payoffs.....	144
Table 7. Determinants of Receiver's beliefs about the Sender's message	145
Table 8. Determinants of Sender's belief about the Receiver's information decision.....	146
Table 9. Bivariate probit model	148
Table 10. Messages classification	150
Table 11. Message classification.....	161
Table 12. Comparison of fixed and free message treatments	166
Table 13. Percentage of male and female subjects by treatment	186
Table 14. Percentage of subjects per country	186
Table 15. Percentage of subjects by field of study	187
Table 16. Percentage of subjects by language	188
Table 17. Descriptive statistics for age by treatment.....	188
Table 18. Descriptive statistics for experimental earnings by treatment.....	188
Table 19. Percentage of subjects by gender	189

Table 20. Percentage of subjects by country	189
Table 21. Percentage of subjects per language	189
Table 22. Percentage of subjects by field of study	190
Table 23. Descriptive statistics for age by treatment.....	191
Table 24. Descriptive statistics for experimental earnings by treatment.....	191
Table 25. Definition of variables	196
Table 26. Receivers' messages for ExoInfo treatment (only session 1).....	197
Table 27. Senders' messages for FreeMess-NoInfo treatment (only session 1).....	198

Acknowledgements

I am grateful for the support I received from many people during the time when I worked on this thesis. First, I want to thank Anders Poulsen for his guidance and valuable advice from the beginning of this thesis. I also want to thank Ben Mcquillin for his inspiring thoughts that helped to improve my work.

I also extend my thanks to my examiners, Dr. Stefania Sitzia and Professor Pablo Brañas-Garza, for agreeing to read the manuscript, and to make helpful comments on it.

I thank the Centre for Behavioural and Social Science (CBESS) at the University of East Anglia for providing generous financial support to conduct all my research. I am also grateful to seminar participants at different universities in the United Kingdom and abroad for their useful suggestions.

I am extremely grateful to all my colleagues and friends, who believed in my work. I am also grateful to the staff from the Post Graduate Research Office and the Dean of Student Office for their support. All errors and persistent views and thoughts are my total responsibility.

I dedicate this thesis to my parents.

Graciela Zevallos Porles
University of East Anglia
February 2018

Abstract

This doctoral thesis consists of a collection of three independent chapters. The first chapter reports the results of a laboratory experiment designed to investigate whether antisocial behaviour is driven by nastiness, defined as an intrinsic pleasure derived from lowering the well-being of others. As we shall see, people are not nasty for the sake of being nasty. Rather, people become nastier when lowering somebody's welfare is monetarily beneficial. The second chapter explores the effects of the relative price and monetary stakes on the Dictators' decisions in three different contexts. The data shows the experimental context matter: the effect of the monetary stakes is strong and significant on giving, but is weak and insignificant on taking. The third chapter presents an experiment designed to investigate the role of ex-post acquisition information in Sender-Receiver games. The analysis reveals that deception decreases when the Receiver can learn about the structure of the payoffs. Furthermore, a significant minority of Receivers choose to get ex post information about the payoffs.

Introduction

This doctoral thesis is a collection of three independent chapters and as such, each chapter focuses on a different topic. Methodologically, however, these three chapters share an important common feature: they all use controlled laboratory experiments. In this sense, this thesis is a contribution to the experimental method and to economic science.

The first chapter was motivated by the relatively limited experimental studies that examine antisocial behaviour in the laboratory. Antisocial behaviour is now widely recognized as a major social and economic problem in the real world.¹ People litter the streets, damage private properties, steal information, harass other people, and send malicious computer viruses.² According to the British Crime Survey (2016), the police recorded around 1.8 million incidents of antisocial behaviour in 2012/2013.³ These actions are, for sure, costly and have a negative effect on people's lives. For instance, Governments spend resources on patrolling problem areas and creating new laws aiming to tackle the problem, while individuals spend money to secure their properties.⁴ It is clearly important to investigate the motivations underlying this behaviour.

In recent years, significant progress has been made by empirical studies to gather information about antisocial behaviour. Despite these efforts, there is a problem of defining and measuring the behaviour itself. Common understandings of antisocial behaviour are still characterised by vagueness and subjectivity. There are as many

¹ The growing alertness that this problem generated is reflected in an increasing number of local initiatives to combat this behaviour. For instance. There is an increasingly high emphasis being placed by the Government on antisocial behaviour (ASB) and methods to tackle it. They range from the Antisocial Behaviour Order (ASBO) to lighting on streets and opening playing fields with football and rugby facilities.

² Other examples of antisocial behaviour include vandalism, graffiti, street drinking, and using residential premises for illegal purposes

³ See <https://www.ons.gov.uk/peoplepopulationandcommunity/crimeandjustice/bulletins>

⁴ Grossman and Komai (2012) measured the cost of antisocial behaviour in the laboratory and found that antisocial behaviour reduces total economic welfare by around 20%.

definitions of antisocial behaviour as there are studies of this behaviour. For instance, according to The Metropolitan Police in the UK, anti-social behaviour can be described as 'behaviour by a person which causes, or is likely to cause, harassment, alarm or distress to one or more persons not of the same household as the person.'⁵ In addition, data is restricted to those incidents that have been reported. Hence, it becomes extremely difficult if we wish to compare data and study antisocial behaviour seriously.

Laboratory experiments have helped to overcome these problems by collecting data in a controlled manner. Behavioural economists who are concerned with the dark side of human behaviour have developed a new class of experimental games where they can elicit antisocial preferences. Many experimental studies have discovered that subjects are willing to reduce their participant's earnings even though this is costly to themselves. This result is undoubtedly a puzzle for these economists, given the abundant evidence on pro-social behaviour (Andreoni and Miller, 2002; Fehr and Schmidt, 1999; Charness and Rabin, 2002)

In Chapter 1, I report the results of a laboratory experiment designed to determine whether antisocial behaviour is driven by nastiness, defined as an intrinsic pleasure derived from inflicting harm on others (Abbink and Sadrieh 2009).⁶ Whether the nastiness hypothesis is robust is an important economic question. If nastiness explains antisocial behaviour, it may not then be possible to control it. This is important, not only in order to understand antisocial behaviour, but also to have a clear picture of the behaviour of individuals. While the nastiness hypothesis is intuitively appealing, this hypothesis has not yet tested experimentally. In the first chapter, I aim to fill this gap.

The experimental set up is a destruction game (Abbink and Sadrieh, 2009, Abbink and Herrmann 2011). Two subjects were randomly matched and assigned the role of either Player 1 or Player 2. Each player received an initial endowment of £6. They simultaneously chose whether to reduce the other player's endowment by £3. The

⁵ See https://www.met.police.uk/anti_social_behaviour

⁶ Experimental studies have different explanations for this behaviour (Zizzo 2003, Zizzo and Oswald 2001, Abbink and Herrmann et al 2010).

features of the game ruled out inequality aversion as the motivation for destructive behaviour in this experiment.

The experiment had four treatments. The cost of destruction varies across treatments. In treatment 1, subjects simultaneously decide whether or not to reduce their co-participant's earnings. If they choose to reduce the other player's earnings, they reduce their own endowment by 60 pence. This treatment worked as a baseline. Treatment 2 is similar to treatment 1, but destruction is free. Treatment 3 is similar to treatment 2, but if the subjects chose to reduce the other player's earnings, they increase their payoff by 60 pence. By comparing these three games, I tested the nastiness hypothesis. A treatment with one-sided destruction was also conducted.

I found that nastiness is not a common trait on human behaviour. The results showed that nobody chose to reduce their co-participant's earnings when destruction was costly. Only 10% of the subjects chose to reduce their co-participant's earnings when destruction was costless. However, differences were not significant. By contrast, when destruction was monetarily beneficial, almost half of the subjects chose to reduce their co-participant's earnings. This happened despite the fact that subjects were initially equal endowed. This finding suggests that people are not nasty for the sake of being nasty. Therefore, antisocial behaviour is contingent on the contextual environment. Nastiness alone cannot explain the data. The data is also consistent with Homo oeconomicus.⁷

This chapter addresses a number of open research questions that can be studied experimentally. For instance, how do individuals behave when they can decrease and increase their co-participant's earnings? Do we find framing effects in antisocial preferences?

The second chapter was motivated by the more recent evidence, which showed that giving in Dictator games can be strongly affected by the experimental choice set. The

⁷ The Homo oeconomicus is an abstract individual, whose actions are guided by selfishness and greedy motivations (Smith, 1776; Walras, 2014).

most common assumption in Economics is that people only care about maximizing their own income. Moreover, standard economic theory assumes that individuals are homogeneous in every respect, but in their economic assets (Smith, 1776).

Nevertheless, a large body of research has shown that subjects are altruistic (Andreoni and Miller, 2002), inequality averse (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), and reciprocal (Falk and Fischbacher, 2006). In particular, the Dictator game has been used as a vehicle of research to study altruism.

In a standard Dictator game, the Dictator has some money and decides upon how much of that money to give to the Recipient. The Recipient has to accept the Dictator's decision (Forsythe et al., 1994). The sub-game Nash equilibrium is to transfer zero. However, in many Dictator games, the Dictator transfers a positive amount of money (Kahneman et al., 1986; Forsythe et al., 1994). As the assumption of self-interest has been proved to be too rigid, experiments have introduced the concept of other-regarding preferences.

In a seminal paper, Andreoni and Miller (2002) used a within-subject design to examine whether altruism is rational and consistent with the Generalized Axiom of Revealed Preference (GARP). The authors find that giving varied with the price of giving and the subjects' endowment. Moreover, the authors find a substantial heterogeneity across subjects: some dictators do leave nothing, but others give away as much as 50% of the endowment.⁸

Recent evidence, however, showed that a change in the experimental context affects Dictators' preferences. For instance, List (2007) and Bardsley (2008) showed that generosity can be reversed by allowing the Dictators to take money from the Recipient. This evidence has implications for the external validity of the experimental results and for modelling behaviour with social preferences.

⁸ See also <https://www.thirdsector.co.uk/uk-ranked-fourth-world-giving-percentage-gdp-says-cafreport/fundraising/article/1381921>

The second chapter explores the effects of the relative price and monetary stakes on the Dictators' decision in three different contexts. The experimental set up employed variants of the Dictator game. Subjects were randomly assigned to the role of Dictator and Recipient. Both were given an initial endowment. According to the game, Dictators can transfer money to the Recipient, take money from the Recipient, and burn money from the Recipient, even though it is costly to themselves. The relative price or the trade-off between own payoff and Recipient's payoff, as well as the Dictator's endowment, varied across games. Dictators engaged in up to eighteen allocation decisions. After Dictators made their decisions, one of the games was selected randomly for final payoffs.

I found that Dictators' decisions are affected by the relative price in the direction predicted by standard economic theory. The effect of the monetary stakes, however, differs by whether Dictators transfer or take money from the Recipient. The effect of the Dictator's endowment is strong and significant on giving, but is weak and insignificant on taking. These findings contribute to the experimental literature which suggest that the experimental context matters. Chapter 2 concludes with a summary of results and discussion of open questions to be explored in future experimental work.

The third chapter is concerned with information transmission in asymmetric situations. Asymmetric information is a characteristic in many social and economic situations, where one party to an exchange possesses important information, but the other party does not. These situations often provide incentives to deceive the less informed party. For instance, in 2013, the International Council for Clean Transportation (ICCT) revealed that some of the major carmakers inserted special software to their cars to manipulate emissions tests.⁹

Following the theoretical work of Crawford and Sobel (1982), behavioural economists have introduced Sender-Receiver games to investigate the factors affecting deception. The typical finding from many laboratory experiments is that subjects do not frequently lie (Gneezy, 2005; Charness and Dufwenberg, 2005; Sutter, 2009). The results from these experiments have been interpreted as showing that people have lie aversion. The

⁹ See <http://www.bbc.co.uk/news/business-21759258>

literature, however, has little to say about how deception is affected by the possibility that the Receiver learns about the payoffs. In the above studies, deception is totally disclosed to the experimenter, but the Receiver never learns about the payoffs.

In the third chapter, I investigate the role of ex-post acquisition information on deceptive behaviour in Sender-Receiver games. The Receiver can ex-post, and at a cost, find out if the Sender's message was true or not. I ask whether attitudes toward lying are affected by the possibility that the Receiver can find out, ex post, if they were lie to. To the best of my knowledge, one study Behnk et al (2014), previous studies have not experimentally examined the role of ex-post information on deceptive behaviour. I aim to fill this gap. Behnk et al (2014). examines how costless ex post information about the payoffs affect deception. However, the authors are not able to study the demand for ex post information about the payoffs. In this experiment, I do.

I used a simple Sender-Receiver game and conducted five different treatments. In treatment 1, the Receiver never learns about the payoffs. In treatment 2, the Receiver, after choosing between option A and B, automatically learns about the payoffs. By comparing this treatment to the baseline, I study the role of ex-post information in reducing deception. In treatment 3, the Receiver, after choosing between option A and B, decides whether she wants to learn about the payoffs. If the Receiver chooses to learn about the payoff, he/she decreases his/her payoffs by £1. Treatment 4 is identical to treatment 3, but now information is free.

Finally, treatment 5 is identical to treatment 3, except that if the Receiver chooses to learn about the payoff, he/she increases his/her payoff by £0.10. Across treatments 3, 4, and 5, I examined whether and to what extent the Receiver is willing to acquire ex-post information about the payoffs. To examine whether the Receiver wishes to punish or reward the Sender's behaviour, I allowed the Receiver, after learning or not about the payoffs, to send back a message to the Sender.

The main results emerging from this study can be summarised as follows. First, the evidence show that providing ex post information, compared to non-information, can significantly reduce the probability to lie. This result is hard to reconcile with Senders

being exclusively motivated by lying aversion. The effect of ex post information about the payoffs on deception is consistent with models of guilt and shame aversion. Second, a significant minority of Receivers get ex post information about the payoffs even though it is costly to themselves. In the current design, there is not strategic reason to get information about the payoffs. Hence, the evidence contradicts the standard assumption that Receivers view information as merely a means for making better decisions, in which case no one should get information about the payoffs.

I also move beyond the fixed messages and allow Senders to send a free text message to the Receiver. I found that many Senders instead of telling a direct lie, they prefer to send a partial truth or an ambiguous message. Moreover, Senders' messages are effective even in the absence of strategic concerns. These results suggest that fixed message treatments fail to capture the large range of preferences for truth telling.

Chapter 1

An Experimental Test of the Nastiness Hypothesis

1. Introduction

Behavioural economists who are concerned with understanding antisocial behaviour have created a new variety of experimental games that allow people to make directly antisocial choices.¹ This broad class of games includes money-burning games (Zizzo and Oswald, 2001; Zizzo, 2003), joy-of-destruction games (Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011) as well as first-strike games (Abbink and de Haan, 2014) and destructor games (Kessler, 2012). Results from these games have consistently revealed a significant proportion of subjects willing to reduce their co-participant's earnings, even though such behaviour is costly to themselves.²

The results, however, are in strong contrast to the vast evidence from laboratory experiments that have reported a substantial proportion of subjects behaving pro-socially. Subjects are altruistic (Andreoni and Miller, 2002; Andreoni et al., 2010), inequality averse (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), reciprocal (Falk and Fischbacher, 2006); or maximize social welfare (Charness and Rabin, 2002; Engelmann and Strobel, 2004).

¹ Real-life examples for antisocial behaviour include violence against others, vandalism, malicious computer virus, and vagrancy.

² In a money-burning game (Zizzo and Oswald, 2001), four players receive an unequal endowment and simultaneously choose whether to reduce the other player's earnings. In a joy-of-destruction game (Abbink and Sadrieh, 2009), two players simultaneously choose whether to reduce the other player's earnings over many rounds. In the first-strike game (Abbink and de Haan, 2014), two players accumulate earnings across rounds and choose whether to reduce the other player's earnings in each round. In the destructor game (Kessler, 2012), only one player makes the decision whether to reduce the other player's earnings.

In the joy-of-destruction game, Abbink and Sadrieh (2009) proposed nastiness, an intrinsic motivation to cause harm to others, as the main motivation for the observed antisocial behaviour. I will refer to this idea as the nastiness hypothesis. As Abbink and Sadrieh (2009) clearly state: 'We interpret the high destruction rates in the hidden treatment as an indication of the pleasure of being nasty' (p.308).

This study reports the results of an experiment specifically designed to test the empirical plausibility of the nastiness hypothesis. To the best of my knowledge, previous studies have not experimentally tested the nastiness hypothesis.³ I aim to fill this gap. I ask two important questions: Does nastiness drive destruction choices? If so, to what extent does nastiness influence antisocial behaviour?

To elicit nastiness, I conducted a laboratory experiment using a destruction game. The destruction game is a variant of the joy-of-destruction game introduced in the experimental literature by Abbink and Sadrieh (2009). In the current experimental game, subjects were randomly matched in pairs and were initially endowed with the same amount of money. This feature of the game removed inequality aversion as a motivation for destructive behaviour. The subjects, who interact only once, can simultaneously reduce the other player's endowment by half.

I ran three treatments. The cost of destruction, that is, the amount of money that subjects pay to reduce their co-participant's earnings, varied across treatments. In the first treatment (called 2S-Costly), destruction was costly, that is, if a subject chose to reduce the co-participant's earnings, he/she had to reduce her/his own earnings by 60 pence. This treatment served as a baseline and provides evidence on the number of subjects who are intrinsically motivated by nastiness.

In the second treatment (2S-Costless), destruction was costless: if a subject chose to reduce the co-participant's earnings, he/she did not reduce her/his own earnings. In the

³ Straatman (2012) reviewed seven relevant experimental studies to examine evidence in support of the nastiness hypothesis. Based on these experimental studies, he argues that it would be premature to accept that nastiness plays an important role in the antisocial behaviour in the laboratory.

third treatment (2S-Benefit), subjects had the opportunity to increase their earnings by causing harm to others. If a subject chose to reduce the other player's earnings, he/she earned 60 pence. This treatment is a Prisoner's Dilemma game, a stylized representation of situations in which an economic agent has the opportunity to increase his earnings by causing harm to others.⁴ By comparing the results across the three treatments, I examined the extent of nastiness in antisocial behaviour. If subjects were motivated by nastiness, the fraction of subjects willing to reduce the co-participant's earnings should be higher in 2S-Costless than in 2S-Costly, and higher in 2S-Benefit than in 2S-Costless.

Because the pairs play simultaneously, negative reciprocity (also called pre-emptive retaliation) may play a role in the decision to reduce the other player's earnings (Fehr and Gächter, 2000; Falk and Fischbacher, 2006).⁵ In other words, subjects may have pessimistic expectations about how their co-participant will behave and can reduce the co-participant's earnings based on the expectation that the co-participant will reduce her/his earnings (Abbink and de Haan, 2014; Zizzo and Oswald, 2001).⁶

To investigate the effect of negative reciprocity on the behaviour of individuals who choose to reduce the co-participant's earnings, I also ran a fourth treatment, which I called 1S-Benefit. This treatment was similar to 2S-Benefit; but where only one subject, randomly chosen, decided whether to reduce the co-participant's earnings. The other player did not make any decision. By comparing 1S-Benefit with 2S-Benefit, I examined the effect of negative reciprocity when destruction entailed a small monetary gain.

⁴ This may resemble circumstances such as criminal damage, assaults, theft, and credit card fraud. Some experimental studies on crime employed a taking game (Eichenberger and Oberholzer-Gee, 1998; Falk and Fischbacher, 2002)

⁵ In addition, Cooper and Kagel (2009) review the economic models on reciprocity, and Meir (2006) review the experimental evidence from the field on reciprocity.

⁶ Abbink and de Haan (2014) gave a good demonstration of how pre-emptive retaliation or fear of destruction may lead to destruction, even though the subject does not want to destroy: 'but even if I do not believe that the opponent is malevolent, I may fear that he fears that I am, and thus strike against him.' (p.191)

The data from this experiment showed that when destruction was costly (2S-Costly treatment), nobody was willing to reduce other player's earnings; when destruction was costless (2S-Costless treatment), only 10% of all subjects chose to reduce the other player's earnings. This increase, however, was not statistically significant. The data also showed that the fraction of subject willing to reduce the other's earnings increased drastically when destruction entailed a small monetary gain. Around 45% of subjects chose to reduce their player's earnings. Therefore, subjects become nastier when they benefit monetarily from it. I also found that negative reciprocity does not play a role on willingness to reduce the other player's earnings, when the destruction entails a small monetary gain.

The results suggest that people are not nasty for the sake of being nasty. Rather, destructive decisions seemed to be consistent with *Homo oeconomicus*.⁷ Nastiness is not the primary motivation for behavioural differences observed across treatments. The results from this study challenge the predictive power of the nastiness hypothesis. This hypothesis would be supported if, for example, subjects chose to reduce the other player's earnings, whether such destruction was costly or costless. The results from this study do not support this proposition. Rather it suggests that it is necessary to reconsider the role of nastiness played in destruction games.

This study makes two important contributions to the literature examining antisocial preferences. First, to the best of my knowledge, this is the first experiment to explore the extent of nastiness in the antisocial behaviour. Second, previous research has either examined costly destruction or costless destruction, but researchers have not used the same experimental setting. For instance, Abbink and Herrmann (2011) used a joy-of-destruction game where destruction was costly, whereas Abbink and Sadrieh (2009) used a joy-of-destruction game where destruction was costless. Zizzo and Oswald (2001) and Zizzo (2003) varied the cost of burning in a money-burning game, which differed from the experimental setting used in this study. None of these experiments was able to test for nastiness. I use the same experimental game to systematically elicit nastiness.

⁷ The *Homo oeconomicus* is an abstract individual, whose actions are guided by selfishness and greedy motivations (Smith, 1776; Walras, 2014).

Third, I show that nastiness alone cannot explain the high prevalence of antisocial behaviour in the laboratory.

The remainder of this chapter is organised as follows. Section 2 reviews the key experimental studies on antisocial behaviour. Section 3 presents the experimental design and describe the procedures. Section 4 summarizes the experimental results. Section 5 presents the answers to the post-experimental questionnaire. Section 6 presents a discussion of the results and concludes.

2. Related literature

This section reviews the literature that look for the occurrence of antisocial preferences.⁸ I divide the related literature into two main groups. The first group included antisocial experiments involving bilateral destruction: two subjects simultaneously can reduce the co-participant's earnings. The second included antisocial experiments involving unilateral destruction: only one of the subjects can reduce the co-participant's earnings.⁹

2.1. Bilateral destruction

Zizzo and Oswald (2001) introduced a money-burning game. Four players were randomly matched and received an unequal number of tokens. They simultaneously choose how many tokens to reduce from the other player's earnings. The price of burning, the number of tokens that a player must give up in order to reduce the other player's endowment by one unit, varied across treatments. Zizzo and Oswald (2001) found that approximately 62.5% of players chose to reduce some part of other player's

⁸ I use the term 'antisocial preferences' to refer to motives that induce people to hurt others even when it is personally costly to do so. This can include envy (Mui, 1995; Zizzo, 2003) and interdependent preferences (Levine 1998).

⁹ For the purpose of this study, I focus only on experiments where subjects make individual antisocial choices. Other experimental studies allow subjects to make prosocial and antisocial choices. For instance, Sadrieh and Schröder, (2017) showed that pro-social and antisocial behaviour can be explained by the desire to influence others, while Zhang and Ortmann (2016) showed that prosocial and antisocial behaviour are correlated.

money. Each subject had on average 48.7% of their earnings burnt. Most of the money burning was aimed at subjects with higher number of tokens. Those who had less tokens burnt more money than those who had more tokens. The amount burnt was not sensitive to the price of burning. The authors suggest that unfair inequality lead to money burning. The current study differed from Zizzo and Oswald's (2001) study in the experimental setting. I removed any destruction motivated by inequality aversion preferences.

Abbink and Sadrieh (2009) introduced the joy-of-destruction game. Two subjects performed a task to earn money. They simultaneously decided how much money to reduce from the other player in multiple rounds. In this game, subjects did not have to reduce their own money to destroy the co-participant's earnings (destruction was costless). In the open treatment, subjects knew how much destruction their co-participant has caused them, whereas in the hidden treatment, they do not. Abbink and Sadrieh (2009) found that 8.5% of all decisions in the open treatment and 39.5% of all decisions in the hidden treatment were destructive. The researchers interpreted the high destruction rates in the hidden treatment as evidence of nastiness. In Abbink and Sadrieh (2009), players were not informed about income differences. In the current study, subjects know their co-participant's initial endowment.

Abbink and Herrmann (2011) modified the joy-of-destruction game (Abbink and Sadrieh, 2009). Players were equally endowed and interacted only once. Moreover, subjects simultaneously decided whether to reduce the other player's endowment by half. Subjects paid to destroy the other player's endowment. Similar to Abbink and Sadrieh (2009), Abbink and Herrmann (2011) found higher destruction rates in the hidden treatment than in the open treatment, but these percentages were lower than in the joy-of-destruction with multiple rounds. Specifically, 26% of subjects chose to reduce their co-participant's earnings in the hidden treatment and 10.8% of subjects in the open treatment. However, Abbink and Herrmann (2011) were not able to make the

comparisons across treatments that I make in this study, so they did not test the nastiness hypothesis.¹⁰

Karakostas and Zizzo (2016) extended the joy-of-destruction game used in Abbink and Herrmann (2011) to examine experimenter demand effects. Karakostas and Zizzo (2016) found experimenter demand effects: 60% of the players chose to reduce their co-participant's earnings when a 'cue' to destroy was given to them. The authors suggested that social image towards the authority appears to be the reason why people choose to behave antisocial.

2.2. Unilateral destruction

The experimental design in this study also bears some resemblance to other settings studied in the experimental literature where destruction is unilateral. Zizzo (2003) extends the experimental design in Zizzo and Oswald (2001) by introducing unilateral destruction. All subjects in a group make their decisions on how much to reduce from the other player's earnings, but only the decision of one of the subjects selected randomly is implemented. He found that unilateral destruction leads to less destruction compared to multilateral burning. He also found that burning is also more sensitive to the price of burning compared to the simultaneous game. This study differs from Zizzo 2003 in that I randomly chose who made the decision first, so I eliminate negative reciprocity.

Abbink et al (2011) used a unilateral burning game to study framing effects in antisocial preferences. Subjects were randomly assigned to the role of decider and victim. There are two frames. In the negative frame, the decider can reduce the victim's payoff and destruction is costly. In the positive frame, the decider can increase the victim's payoff and increasing the victim payoff increases the decider payoff. The antisocial choice in this frame is to not increase the other payoff. In both frames, the decider's task is equivalent in terms of payoffs. Abbink et al (2011) found no framing effects on the total amount of money destroyed in both frames (25% of the subjects choose to burn).

¹⁰ Blackwell and Diamond (2016) examined the impact of hugs upon behaviour in the joy-of-destruction game. They found that hugging reduced spiteful behaviour.

However, destructive behaviour is affected by whether the decider has an initial endowment lower or higher than the victim's endowment. In the negative destruction frame, deciders burn more money when inequality is advantageous than when it is disadvantageous. Equal distributions of money are also prone to destruction. However, this effect is reversed in the positive frame. This current study differs from Abbink et al (2011) in that I use a between-subject design. I also give each decider an equal endowment as the victim. In addition, I do not study framing effects.

Kessler et al (2012) introduced the destructor game. Two subjects randomly matched are assigned to the roles of destructor and passive player. Subjects completed a task to earn money. The money earned is the same for both subjects. The destructor chose whether to reduce a determined share of the passive's earnings. Destruction was costless for the destructor. In addition, nature could also destroy a share of the passive's earnings. Kessler (2012) found that 15% of destructors chose to reduce their co-participant's earnings. This current study differs from Kessler et al (2012) in that subjects cannot hide their destruction decision behind nature.

Abbink and de Haan (2014) introduced the first-strike game. Two players randomly matched perform an individual task over several rounds to accumulate earnings. In any round, subjects could simultaneously reduce the total earnings of her partner. The subject who chose to destroy other player's earnings, reduced their own earnings. Abbink and de Haan (2014) found that fear of destruction considerably increased destruction: 77.8% of subjects inflicted significant damage on the other subject, but mutual trust decreased destructive behaviour (33.9% of the subjects choose to reduce). This current study differs from Abbink and de Haan (2014) in that players made a single decision instead of playing many rounds. In addition, subjects did not perform a task to earn their endowments.

Fehr (2015) examined whether increasing inequality increases anti-social behaviour towards others. Subjects randomly matched into groups of four performed a task to earn money. They received information about their relative performance in their group. They could burn up to half of the income of a group member. The decision to burn incurred a cost to the perpetrator. After the decision, only the decision of one randomly selected

group member was implemented. Fehr (2015) found that unfair inequality increased anti-social behaviour.

3. Experimental design and procedures

The experimental set-up implemented a variant of the one-shot joy-of-destruction game (Abbink and Herrmann, 2011). Two subjects were randomly matched and assigned the role of either Player 1 or Player 2. Each player received an initial endowment of £6. They simultaneously chose whether to reduce the other player's endowment by £3. The experiment had four treatments: the two-sided costly treatment (2S-Costly), the two-sided costless treatment (2S-Costless), the two-sided benefit treatment (2S-Benefit), and the one-sided benefit treatment (1S-Benefit). The cost of destruction varies across treatments.

3.1. The two-sided costly treatment (2S-Costly)

This was the first treatment. Subjects decide whether or not to reduce the co-participant's earnings. If a subject chose to reduce the other player's earnings, she/he had to reduce her/his own endowment by 60 pence. This treatment worked as a baseline. I examined whether subjects were intrinsically motivated by nastiness. Hence, if subjects were nasty it is likely that they will choose to reduce other player's earnings.

The fact that destruction is costly, the subject has to reduce his/her own earnings by 60 pence to reduce the co-participant's earnings might have an effect on the decision to reduce the co-participant's earnings. Destruction may be too costly, and consequently a nasty subject will not reduce the other player's earnings. Therefore, failure to observe positive destruction choices should not be seen as a definitive rejection of pure nastiness hypothesis. This motivated the second treatment.

3.2. The two-sided costless treatment (2S-Costless)

This was the second treatment. This was identical to the 2S-Costly treatment, except that if a subject chose to reduce the co-participant's earnings, he/she does not pay any money

to do it. By comparing 2S-Costly with 2S-Costless, I examined whether simply reducing the cost of destruction increases the amount of decisions motivated by nastiness.

In the 2S-Costless, subjects who were intrinsically motivated to behave nastily and did not reduce the other subject's earnings in the previous treatment, will do now because it is costless.

3.3. The two-sided benefit treatment (2S-Benefit)

This was the third treatment. This was identical to the 2S-Costless treatment; except that subjects had the opportunity to increase their own earnings by reducing the other player's earnings. If a subject chose to reduce the other player's earnings, he/she earned 60 pence. By comparing, 2S-Benefit to the other two treatments, I examined whether a small economic gain can trigger nastiness.

3.4. The one-sided benefit treatment (1S-Benefit)

Given that the subjects played simultaneously, it is also possible that negative reciprocity or pre-emptive retaliation triggered destruction in 2S-Benefit. In other words, a subject may want to reduce the other player's earnings because he/she has pessimistic expectations that the other player will reduce his/her monetary earnings (Fehr and Gächter 2000, Falk and Fischbacher 2006).

Previous experiments have already pointed out the fact that destruction can be driven by negative reciprocity or pre-emptive retaliation. The subject thinks the other subject will destroy, so she destroys (Abbink and Sadrieh 2009, Zizzo and Oswald 2001).

For this reason, I conducted a fourth treatment. I called this treatment 1S-Benefit. This treatment was identical to the 2S-Benefit treatment, except that those in the role of Player 1 decided whether to reduce the other player's endowment by £3. Player 2 was passive. If Player 1 chose to reduce Player 2's earnings, Player 1 earned 60 pence.

In this treatment, I removed the potential for negative reciprocity by removing uncertainty about Player 2's decision. The difference of the fraction of destructive choices between 2S-Benefit treatment and 1S-Benefit treatment will provide a measure of the amount of destruction that would be attributable to nastiness in a setting where destruction entails a monetary gain.

I expected Homo economicus not to destroy in 2S-Costly and to be indifferent in 2S-Costless. However, he would destroy in 2S-Benefit. If nastiness is a motivation for destruction, I expected more destruction in 2S-Costless than in 2S-Costly, and more in 2S-Benefit than in 2S-Costless. Moreover, comparison of decisions to reduce the other player's earnings across games allows permitted to investigate how willingness to reduce other player's earnings varied with the cost of destruction. Table 1 summarizes the experimental design.

Table 1. Experimental treatments

Treatment	Number of sessions	Number of subjects	Player 1's endowment (£)	Player 2's endowment (£)	Amount to reduce from the co-participant's earnings (£)	Amount to pay if the subject chose to reduce the co-participant's earnings (£)
2S-Costly	2	20	6	6	3	0.60
2S-Costless	2	20	6	6	3	0.00
2S-Benefit	2	20	6	6	3	-0.60
1S-Benefit	4	40	6	6	3	-0.60

3.5. Procedures

The experiment was conducted at the Arts Laboratory of the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia (UEA) between January and July 2013. The subjects were undergraduate students recruited from diverse disciplines using the on-line recruitment system ORSEE (Greiner, 2015). None of the subjects had previously taken part in or otherwise gained experience with a similar experiment. Subjects received a £2 participation fee, regardless of their decisions. The

experiment was programmed and conducted using the software z-Tree (Fischbacher, 2007).

I ran two sessions per treatment, except for 1S-Benefit treatment, in which I ran four sessions. In each session, I had ten subjects. Subjects participated in only one of the sessions. In total, 100 subjects participated in the experiment: 20 each in 2S-Costly; 2S-Costless; and 2S-Benefit treatment, and 40 subjects in 1S-Benefit treatment. In each session, subjects were given printed instructions, a blank sheet of paper, and a receipt form to hand to the experimenter at the end of the session for their final payments. Instructions for all the treatments can be found in this chapter's Appendix A. Experimental instructions.

The subjects were between 18 and 39 years old, with an average age of 22.32 and standard deviation of 3.64. In the sample, 56% of the subjects were female, 21% studied Economics, 17% Business and Management; 38% were born in China and 33% were born in the United Kingdom. **Appendix C. Background of the participants** of this chapter reports subjects' characteristics.

In all sessions subjects were, upon arrival, randomly seated at visually separated computer terminals in order to avoid facial or verbal communication between subjects. During a session, communication between subjects was prohibited.

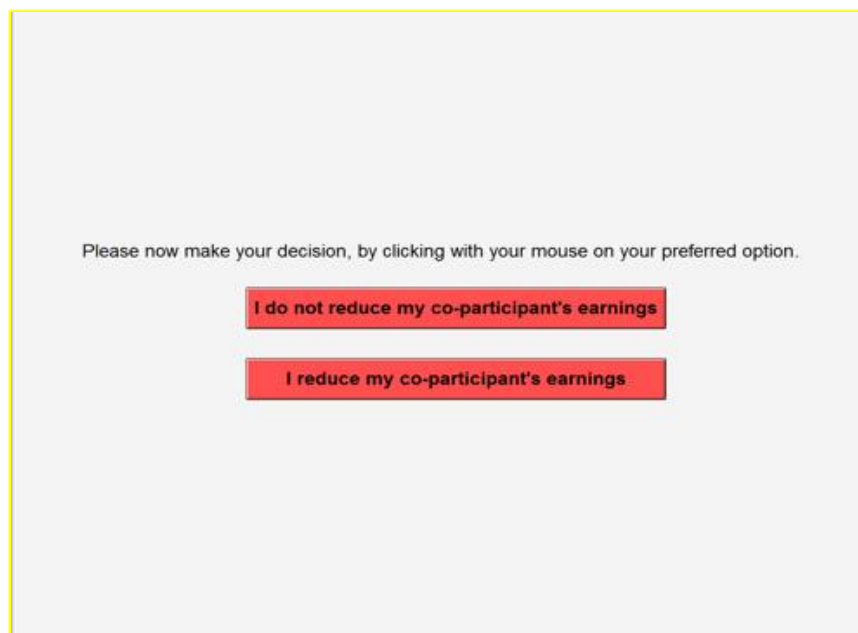
At the start of each session, the experimenter read the instructions aloud as the students followed along on their computer screens. The presentation of the experimental instructions was as neutral as possible avoiding terms such as 'destruction'. Communication between subjects was prohibited. If a subject did not understand any part of the instructions, he/she raised his/her hand for clarification. All clarification was given privately by the experimenter. The experiment started when everybody indicated that they had fully understood the instructions.¹¹

¹¹ At the end of the experiment, all subjects gave feedback about the experiment. When subjects provided feedback on the experiment, they said that 'it was easy to understand the instructions'; they also stated that 'it is interesting and simple to play'; and they also wrote that 'It was very quick and simple to follow'.

Subjects were randomly matched in pairs. Half were assigned to the role of Player 1 and the other half to the role of Player 2. The subjects did not know who their counterpart was, before and after the session. Anonymity was preserved throughout the experiment.

Figure 1 shows the subject's decision screen for all treatments. A subject made her decision by pressing one of the two buttons to indicate their choice. Each subject received a show up fee of £2, which he/she could keep regardless of his/her decision and his/her co-participant's decision made in the experiment.

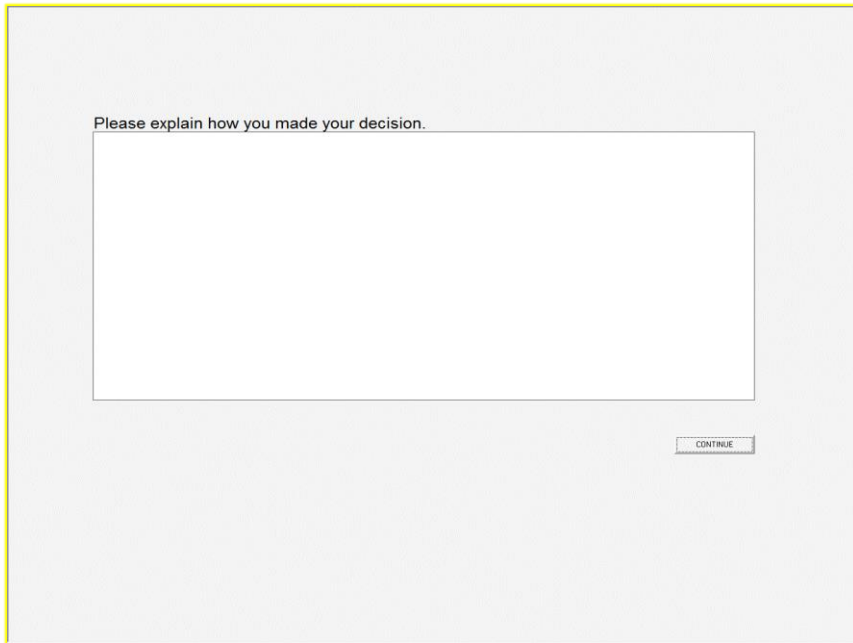
Figure 1. Decision screen



A post-experimental questionnaire was administered asking all subjects to state the reason for their actions (see Figure 2). Answers to this question provided an insight into their main motivations for their behaviour.

In 1S-Benefit treatment, subjects in the role of Player 2, who did not make any decision, were also asked to state their expectations about their co-participant's behaviour (see Figure 3). Note that these subjects had to state their expectations about Player 1's behaviour without receiving any feedback during the experiment. Answers to this question were not monetarily incentivized.

Figure 2. Questionnaire screen for bilateral destruction treatments

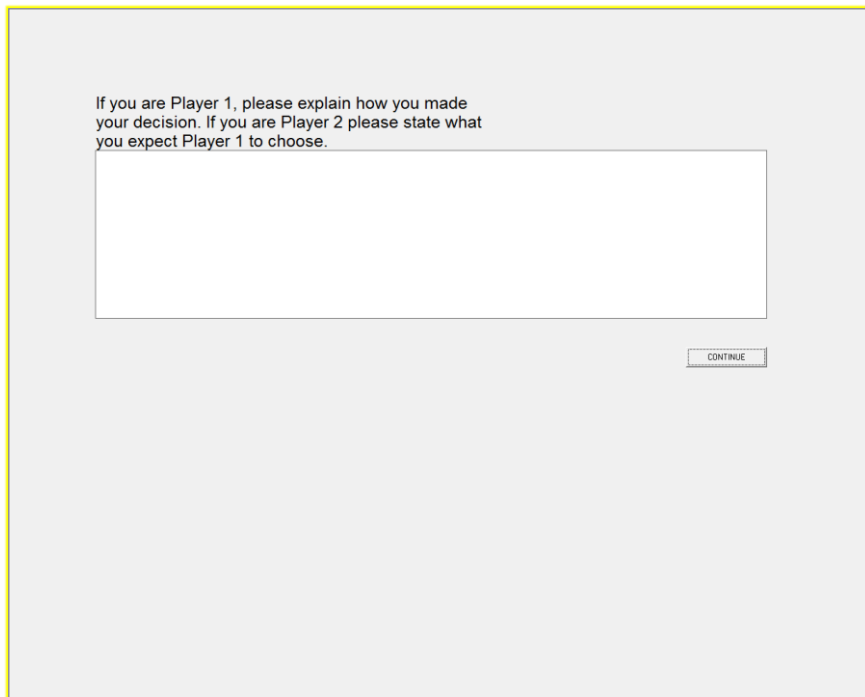


Please explain how you made your decision.

CONTINUE

This figure shows a questionnaire screen for bilateral destruction treatments. It features a light gray background with a white rectangular text input area in the center. Above the input area, the text "Please explain how you made your decision." is displayed. A small "CONTINUE" button is located at the bottom right of the screen.

Figure 3. Questionnaire screen for unilateral destruction treatment



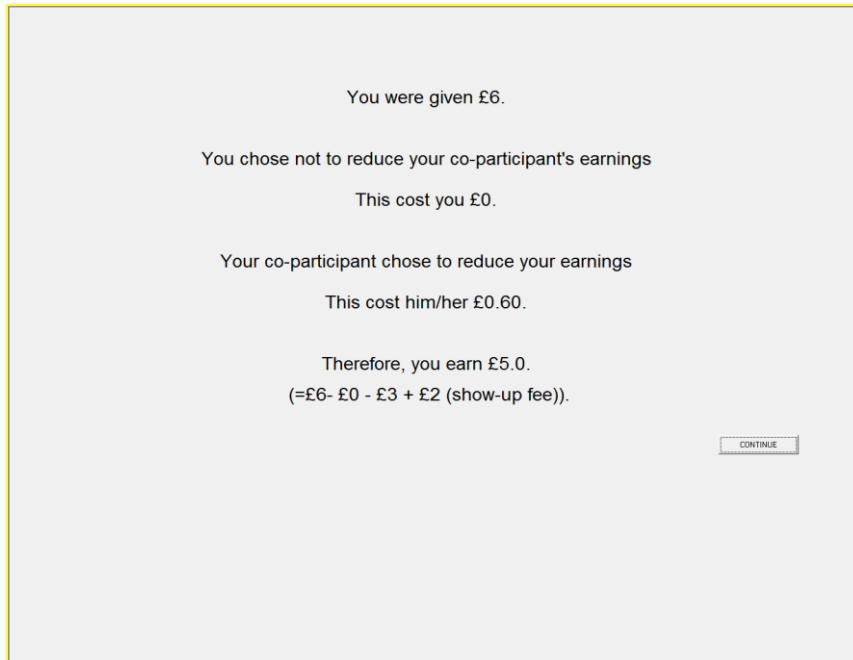
If you are Player 1, please explain how you made your decision. If you are Player 2 please state what you expect Player 1 to choose.

CONTINUE

This figure shows a questionnaire screen for unilateral destruction treatment. It features a light gray background with a white rectangular text input area in the center. Above the input area, the text "If you are Player 1, please explain how you made your decision. If you are Player 2 please state what you expect Player 1 to choose." is displayed. A small "CONTINUE" button is located at the bottom right of the screen.

After all the subjects answered the post-experimental questionnaire, they were told on their own screens what they had earned (in British pounds), but not what their co-participant had earned (for an example of the payoff screen see Figure 4).

Figure 4. Payoff screen for 2S-Costly treatment



After the experiment was over, I also asked each subject to complete a socio-demographic questionnaire designed to gather data on gender, field of study, country of origin, and language. Players also provided feedback on the experiment. Questionnaire screenshots are provided in the **Appendix E. Post-experimental questionnaire** at the end of this chapter. All the questionnaires were computerized using z-Tree software (Fischbacher, 2007).

Each session lasted around 45 minutes, including reading the instructions and filling in the questionnaires. On average, subjects earned £7.48 (including the £2 participation fee); the standard deviation was 1.04; the minimum payoff was £5; and the maximum £8.60. At the end of a session, the subjects were called individually to be paid in cash. Subjects were never told about their partner's earnings.

4. Results

Table 2 summarizes the main results across treatments. The results are also displayed in Figure 5. This figure shows the fraction of subjects who chose to reduce the co-participant's earnings for each treatment. The data from the experiment are provided in **Appendix H. Data** of this chapter.

I also examined differences by gender and country of origin across treatments. The additional statistical analysis is shown in **Appendix F. Gender differences in antisocial behaviour** and **Appendix G. Cultural differences in antisocial behaviour** of this chapter. For the statistical analysis, I pooled the data across sessions in each treatment.

4.1. Bilateral destruction

In treatment 2S-Costly, where destruction was costly, none of the participants chose to reduce their co-participant's endowment. The fact that we do not observe destruction does not mean that subjects do not have nastiness preferences. Destruction may be too costly, so that even a nasty subject is not willing to reduce his co-participant's earnings.

In 2S-Costless, where destruction was costless, 10% of subjects (2 of 20) chose to reduce the co-participant's earnings. In 2S-Benefit, where destruction had a monetary payoff, 45% of the subjects (9 of 20) chose to reduce the co-participant's earnings. There were significant differences across treatments (one-sided Fisher's exact test, $p = 0.001$).¹²

¹² Though the presentation of the experimental instructions was as neutral as possible (as discussed in Section 3.5), the antisocial features of the experimental game used in this study could have influenced the choices made by the subjects. For this reason, I ran an additional treatment and compared the results from this treatment with 2S-Costly. In this additional treatment, subjects played a single one-shot simultaneous Prisoner's Dilemma game. There were two options: Option A and Option B. There were two players randomly matched who simultaneously chose one of these options. If both players choose option A, each received £2.40. If both players choose option B, each received £6. If Player 1 chooses option A, but Player 2 chooses option B, Player 1 earns £5.40 and Player 2 earns £3. If player 1 chooses option B and Player 2 chooses option A, Player 1 earns £3 and Player 2 earns £5.40. Note that the payoff structure remained essentially the same as for 2S-Costly. In this sense, the present experimental treatment also provides a test of the robustness of the results presented in 2S-Costly. The instructions are presented in **Appendix B. Experimental**

The difference between the results of 2S-Costly and 2S-Costless was not statistically significant (one-sided Fisher's exact test, $p= 0.244$). Hence, a decrease in the price of destruction did not affect the player's willingness to reduce their partner's endowment. However, the difference between the results of 2S-Costly and 2S-Benefit was statistically significant (one-sided Fisher's exact test, $p= 0.001$). Similarly, the difference between the results of 2S-Costless and 2S-Benefit was statistically significant (one-sided Fisher's exact test, $p= 0.015$). Hence, a monetary gain from destruction did increase the player's willingness to reduce their partner's endowment.

4.2. Unilateral destruction

I also examined the role of negative reciprocity when destruction entailed a monetary benefit. Subjects may have chosen to reduce the other player's endowment because they thought that the other player would do the same. Destruction was, therefore, a pre-emptive retaliation (Gächter and Herrmann, 2009).

In 1S-Benefit treatment, around 50% of subjects chose to reduce their co-participant's endowment. However, comparing 1S-Benefit and 2S-Benefit, I did not find statistically significant differences in the percentage of subjects willing to do this (one-sided Fisher's exact test, $p=0.102$). Thus, negative reciprocity did not play a role when destruction was economically beneficial.¹³ Thus, when destruction was profitable, beliefs about what the opponent would do did not matter.

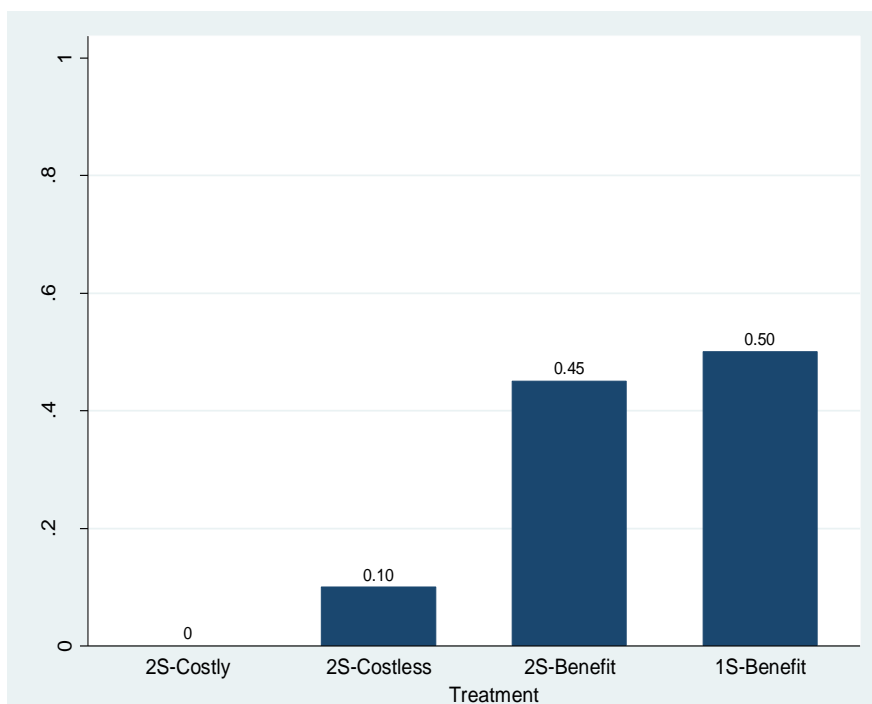
instructions for the neutral treatment. I ran 3 sessions in June 2013. In each session, I had 10 subjects. The experimental protocol in each session was exactly the same as for 2S-Costly. Sessions were also run at the Arts Laboratory of the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia (UEA) to guarantee the same local environment for the individuals. Sessions were conducted and programmed using z-TREE (Fischbacher, 2007). Data showed that only 6% of subjects chose option B (which was equivalent to reducing the co-participant's earnings). No significant differences were detected between 2S-Costly treatment and this treatment (one-sided Fisher's exact test, $p=0.355$). This evidence shows no framing effects when reducing the co-participant's earnings was costly.

¹³ Player 2's Belief in 1S-Benefit Treatment. In our 1S-Benefit treatment, I asked Player 2 in order to elicit subject's expectations towards their partner's behaviour. I find that subjects in the role of player 2 have no

Table 2. Experimental results

Treatment	Number of observations	Subjects who chose to reduce their co-participant's endowment	
		Frequency	Percent
2S-Costly (T1)	20	0	0.00
2S-Costless (T2)	20	2	10.00
2S-Benefit (T3)	20	9	45.00
1S-Benefit (T4)	20	10	50.00
Total	80	21	59.00

Figure 5. Destruction choices



pessimistic expectations about their partner's behaviour did not fear destruction. Overall, 75% of subjects 15 out of 20 in the role of Player 2 did not expect Player 1 would burn.

5. Post-experimental questionnaire

In this section, I used the results from the post experimental questionnaire to investigate more closely the motives subjects chose to reduce the co-participant's earnings.¹⁴ It seems that concern for maximizing their own money was the main motivation for the destructive behaviour of the subjects.

5.1. Bilateral destruction

In the 2S-Costly treatment, where nobody reduced the other's earnings, subjects seemed to maximize their payoff. For instance, one stated: 'I don't mind what my partner choose, and I can't control him/her, I just keep my own money in the lowest probability of losing.'

In the 2S-Costless treatment, in which 10 percent of subjects decided to reduce other's earnings, the cost of destruction and expectations about the co-participants behaviour seemed to play an important role. For instance, one subject wrote: 'I will consider the cost if i decide to reduce my co-participant earnings. However, in this case, it won't cost me any and it means i can keep my earnings if my co-participant decide not to reduce my earnings. And i believe that he/she will not reduce my earnings in normal situation'. Another subject wrote: 'There was no benefit to reducing my co-participant earnings for me.'

In the 2S-Benefit treatment, in which 45 percent of subjects decided to reduce the other player's earnings, pre-emptive retaliation or negative expectations about the co-participant's behaviour again seemed to play an important role. For instance, one subject wrote: 'I considered not reducing my co-participants amount because it'd be fair to both of us to earn £6 but then I changed my mind because I figured s/he would most likely reduce my amount and I made the decision firstly as a defence mechanism and secondly to make it fair.'

¹⁴ Subject's statements were not edited. Additional comments are available upon request.

5.2. Unilateral destruction

In 1S-Benefit, in which 50 percent of subjects decided to reduce the other player's earnings, monetary gain seemed to be the main determinant.¹⁵ For instance, one subject in the role of Player 1 stated: 'Because I want to more earn £0.06, so I chose to reduce my co-participant's £3.'

6. Discussion and conclusion

A broad observation from the literature on experimental games shows a significant proportion of people willing to reduce other's income. In this chapter, I experimentally tested the nastiness hypothesis. To the best of our knowledge, previous studies have not yet done that. The nastiness hypothesis states that people have a genuine pleasure from causing harm to others (Abbink and Sadrieh, 2009). I proposed a clear experimental design that systematically elicited nastiness isolated from inequity aversion and concern for fairness.

The principal result of the experiment is that nastiness alone cannot explain the high prevalence of the antisocial behaviour observed in the previous laboratory experiments. By comparing behaviour across these treatments with varied costs of destruction, I am able to show that people are not nasty for the sake of being nasty. The data are consistent with destructive selfishness. In other words, the data are not consistent with the nastiness hypothesis.

The data revealed that nobody was willing to reduce the co-participant's earnings when destruction is costly 2S-Costly, whereas only 10% of all subjects were willing to reduce the co-participant's earnings when destruction is costless 2S-Costless. This difference was not statistically significant. By contrast, when subjects can make gains from

¹⁵ In 1S-Benefit treatment, I also elicited expectations of Player 2 subjects about their co-participant's behaviour. Most in the role of Player 2 did not fear destruction from the co-participant. Overall, 75% (15 of 20) of subjects in the role of Player 2 did not expect Player 1 to reduce their earnings, despite destruction entailed an economic gain.

destruction, nearly half of the subjects chose to reduce their co-participant's earnings: 45% of all subjects chose to reduce their co-participant's earnings. This is a significant increase relatively to 2S-Costly and relatively to 2S-Costless.

The results from 2S-Benefit are consistent with nastiness, but the data from 2S-Costly and 2S-Costless alone are not. Therefore, this evidence demonstrates that subjects are not nasty for the sake of being nasty. If subjects were nasty, there would have been significant destruction in 2S-Costless, and (although perhaps a lot less) in 2S-Costly. I did not find support for the nastiness hypothesis. However, the data is consistent with Homo oeconomicus. This can explain why destructive decisions jumped when reducing the co-participant's earnings entailed an economic benefit. This evidence poses a challenge for the predictive power of the nastiness hypothesis. The evidence from this study suggest that the observed antisocial behaviour is not exclusively driven by spiteful or antisocial preferences. The evidence is consistent with the Homo economics.

Previous experiments that have examined antisocial preferences have found a high fraction of subjects willing to reduce a partner's earnings, but they have not isolated nastiness from other confounding factors. For instance, in Zizzo and Oswald (2001), subjects chose to burn another player's money motivated by inequality aversion and unfairness. The evidence from this experiment is not consistent with this previous work. However, evidence in this experiment is consistent with Müller et al (2016) who suggest that antisocial behaviour highly depends on the environment that individuals are facing.

The results from 2S-Costly can be compared with Abbink and Herrmann (2001)'s findings. They conducted a mini-joy of destruction game in which subjects received an equal endowment from the experimenter, without performing any task. In the open treatment, subjects can simultaneously reduce the partner's earnings by half. Abbink and Herrmann (2001) found that approximately 8% of the subjects were willing to reduce the other's earnings. This result does not appear at odds with the results from 2S-Costly. However, Abbink and Herrmann (2011) did not run the other treatments, so they were not able to elicit nastiness.

The results from the experiment raised concerns about the possibility of framing effects. The literature on framing effects on antisocial preferences is not abundant. Abbink et al (2011) showed no framing effects on anti-social preferences. It would be interesting to investigate the relevance of framing effects in anti-social experiments further.¹⁶

The simultaneous play opens up the possibility that subjects expect other to destroy thus they destroy. Subjects may also destroy for negative reciprocity. This can lead to destruction. The reason is that subjects may expect the other will reduce their earnings so that they chose to reduce for this reason. To understand whether reciprocity in a simultaneous game plays any role in destruction, I ran a one-sided treatment (1S-Benefit) where destruction is beneficial but now only one of the subject destroy.

The comparison between 2S-Benefit and 1S-Benefit shows that, when destruction is profitable, beliefs about the opponent's action do not matter. This can be interpreted as showing that people are not very reciprocal. Other experimental games have shown that negative reciprocity is important in economic decisions. It will be important to see if no reciprocal consideration extend to the other environments.

The results have two important implications. First, the results emphasize the importance of monetary incentives for the correct understanding of nastiness preferences. This suggests that experiments should pay more attention to selfish motivations as an important incentive to behave nasty. I believe that this framework is crucial in understanding anti-social behaviour in destruction games. Secondly, I also observed that when destruction entails a monetary gain, negative reciprocity does not matter for destruction. Whether or not it matters for other settings can be investigated in future research.

¹⁶ For a review of the experimental evidence on framing effects in pro-social and anti-social experiments, see Abbink and Hennig-Schmidt (2006).

References

Abbink, K. and de Haan, T., 2014. Trust on the brink of Armageddon: The first-strike game. *European Economic Review*, 67, pp.190-196.

Abbink, K. and Hennig-Schmidt, H., 2006. Neutral versus loaded instructions in a bribery experiment. *Experimental Economics*, 9(2), pp.103-121.

Abbink, K. and Herrmann, B., 2011. The moral costs of nastiness. *Economic Inquiry*, 49(2), pp.631-633.

Abbink, K., Masclet, D. and van Veelen, M., 2011. Reference point effects in antisocial preferences. January 31. CIRANO - Scientific Publications No. 2011s-11.

Abbink, K. and Sadrieh, A., 2009. The pleasure of being nasty. *Economics Letters*, 105(3), pp.306-308.

Abbink, K., Masclet, D. and Mirza, D., 2012. Inequality and Inter-Group Conflicts- Experimental Evidence. Condorcet Center for Political Economy. Working paper.

Aguiar, F., Brañas-Garza, P., Cobo-Reyes, R., Jimenez, N. and Miller, L.M., 2009. Are women expected to be more generous?. *Experimental Economics*, 12(1), pp.93-98.

Andreoni, J., Harbaugh, W.T. and Vesterlund, L., 2010. Altruism in experiments. In *Behavioural and experimental economics* (pp. 6-13). Palgrave Macmillan, London.

Andreoni, J. and Miller, J., 2002. Giving according to GARP: An experimental test of the consistency of preferences for altruism. *Econometrica*, 70(2), pp.737-753.

Becker, G.S., 1968. Crime and punishment: An economic approach. In *The economic dimensions of crime* (pp. 13-68). Palgrave Macmillan UK.

Becker, G.S., 2013. *The economic approach to human behavior*. University of Chicago press.

Bolle, F., Tan, J.H. and Zizzo, D.J., 2014. Vendettas. *American Economic Journal: Microeconomics*, 6(2), pp.93-130.

Bolton, G.E. and Ockenfels, A., 2000. ERC: A theory of equity, reciprocity, and competition. *American economic review*, 90(1), pp.166-193.

Brunton, D., Hasan, R. and Mestelman, S., 2001. The 'spite' dilemma: spite or no spite, is there a dilemma?. *Economics Letters*, 71(3), pp.405-412.

Carter, J.R. and Irons, M.D., 1991. Are economists different, and if so, why?. *The Journal of Economic Perspectives*, 5(2), pp.171-177.

Catalano, R.F. and Hawkins, J.D., 1996. A theory of antisocial behavior. *Delinquency and crime: Current theories (1996)*, 149.

Charness, G. and Rabin, M., 2002. Understanding social preferences with simple tests. *The Quarterly Journal of Economics*, 117(3), pp.817-869.

Charness, G., Masclet, D. and Villeval, M.C., 2013. The dark side of competition for status. *Management Science*, 60(1), pp.38-55.

Cooper, D. and Kagel, J.H., 2009. Other regarding preferences: a selective survey of experimental results. *Handbook of experimental economics*, 2.

Croson, R. and Gneezy, U., 2009. Gender differences in preferences. *Journal of Economic literature*, 47(2), pp.448-74.

Douglas, K., 2012. Homo virtuous: the evolution of good and evil. *New Scientist*, 216(2890), pp.42-45.

Eckel, C.C. and Grossman, P.J. 1998. Are women less selfish than men? Evidence from Dictator experiments. *Economic Journal* 108, 726-35.

Eichenberger, R. and Oberholzer-Gee, F., 1998. Rational moralists: The role of fairness in democratic economic politics. *Public Choice*, 94(1-2), pp.191-210.

Engelmann, D. and Strobel, M., 2004. Inequality aversion, efficiency, and maximin preferences in simple distribution experiments. *American economic review*, 94(4), pp.857-869.

Falk, A. and Fischbacher, U., 2002. "Crime" in the lab-detecting social interaction. *European Economic Review*, 46(4), pp.859-869.

Falk, A. and Fischbacher, U., 2006. A theory of reciprocity. *Games and economic behavior*, 54(2), pp.293-315.

Fehr, E. and Gächter, S., 2000. Fairness and retaliation: The economics of reciprocity. *The journal of economic perspectives*, 14(3), pp.159-181.

Fehr, E., Hoff, K. and Kshetramade, M., 2008. Spite and development. *The American Economic Review*, 98(2), pp.494-499.

Fehr, E. and Schmidt, K.M., 1999. A theory of fairness, competition, and cooperation. *The quarterly journal of economics*, 114(3), pp.817-868.

Fehr, D., 2015. *Is increasing inequality harmful? Experimental evidence* (No. SP II 2015-209). WZB Discussion Paper.

Fischbacher, U., 2007. z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental economics*, 10(2), pp.171-178.

Forsythe, R., Horowitz, J.L., Savin, N.E. and Sefton, M., 1994. Fairness in simple bargaining experiments. *Games and Economic behavior*, 6(3), pp.347-369.

Gächter, S. and Herrmann, B., 2009. Reciprocity, culture and human cooperation: previous insights and a new cross-cultural experiment. *Philosophical Transactions of the Royal Society of London B: Biological Sciences*, 364(1518), pp.791-806.

Ghiglieri, M.P., 1999. *The dark side of man: Tracing the origins of male violence*. Perseus Publishing.

Gneezy, U., Leonard, K.L. and List, J.A., 2009. Gender differences in competition: Evidence from a matrilineal and a patriarchal society. *Econometrica*, 77(5), pp.1637-1664.

Greiner, B., 2015. Subject pool recruitment procedures: organizing experiments with ORSEE. *Journal of the Economic Science Association*, 1(1), pp.114-125.

Grossman, P and Komai, M (2012). The Economic Impact of Anti-Social Preferences in a Multi-Period Game with Attacks and Insurance, Discussion Paper 21/12, Monash University, Business and Economics, ISSN 1441-5429, pp.1-31.

Guha, B., 2017. Testing for Malice. *Economics Bulletin*, 37(1), pp.327-335.

Hamilton, W.D., 1970. Selfish and spiteful behaviour in an evolutionary model. *Nature*, 228(5277), p.1218.

Harradine, S., Kodz, J., Lemetti, F. and Jones, B., 2004. *Defining and measuring anti-social behaviour*. Home Office, Research, Development and Statistics Directorate.

Herrmann, B. and Orzen, H., 2008. *The appearance of homo rivalis: Social preferences and the nature of rent seeking* (No. 2008-10). CeDEx discussion paper series.

Herrmann, B., Thöni, C. and Gächter, S., 2008. Antisocial punishment across societies. *Science*, 319(5868), pp.1362-1367.

Heinz, M., Juranek, S. and Rau, H.A., 2012. Do women behave more reciprocally than men? Gender differences in real effort dictator games. *Journal of Economic Behavior & Organization*, 83(1), pp.105-110.

Jensen, K., 2010. Punishment and spite, the dark side of cooperation. *Philosophical Transactions of the Royal Society of London B: Biological Sciences*, 365(1553), pp.2635-2650.

Karakostas, A. and Zizzo, D.J., 2016. Compliance and the role of authority. *Journal of Economic Behavior & Organization*, 124, pp.67-80.

Kessler, E., Ruiz-Martos, M. and Skuse, D., 2012. Destructor Game. *Department of economics Universitat Jaume I*.

Levine, D.K., 1998. Modeling altruism and spitefulness in experiments. *Review of economic dynamics*, 1(3), pp.593-622.

Meier, S., 2006. A survey of economic theories and field evidence on pro-social behavior.

Moffitt, T.E., 2001. *Sex differences in antisocial behaviour: Conduct disorder, delinquency, and violence in the Dunedin Longitudinal Study*. Cambridge university press.

Müller, J., Schwierén, C. and Spitzer, F., 2016. What Drives Destruction? On the Malleability of Anti-Social Behavior.

Mui, V.L., 1995. The economics of envy. *Journal of Economic Behavior & Organization*, 26(3), pp.311-336.

Prediger, S., Vollan, B. and Herrmann, B., 2014. Resource scarcity and antisocial behavior. *Journal of Public Economics*, 119, pp.1-9.

Sadrieh, A. and Schröder, M., 2016. Materialistic, pro-social, anti-social, or mixed-A within-subject examination of self-and other-regarding preferences. *Journal of Behavioral and Experimental Economics*, 63, pp.114-124.

Sadrieh, A. and Schröder, M., 2017. Acts of helping and harming. *Economics Letters*, 153, pp.77-79.

Smith, A., 1776. An inquiry into the nature and causes of the wealth of nations. *London: George Routledge and Sons*.

Straatman, T., 2012. *Do Antisocial preferences exist?*. Erasmus University.

Walras, L., 2014. *Léon Walras: Elements of Theoretical Economics: Or, the Theory of Social Wealth*. Cambridge University Press.

Zhang, L. and Ortmann, A., 2016. Pro-social or anti-social, or both? A within-and between-subjects study of social preferences. *Journal of Behavioral and Experimental Economics*, 62, pp.23-32.

Zizzo, D.J., 2003. Money burning and rank egalitarianism with random Dictators. *Economics Letters*, 81(2), pp.263-266.

Zizzo, D.J. and Oswald, A.J., 2001. Are people willing to pay to reduce others' incomes?. *Annales d'Economie et de Statistique*, pp.39-65.

Appendix to Chapter 1: An Experimental Test of the Nastiness Hypothesis

- A. Experimental instructions
- B. Experimental instructions for neutral treatment
- C. Background of the participants
- D. Background of the participants for neutral treatment
- E. Post-experimental questionnaire
- F. Gender differences in antisocial behaviour
- G. Cultural differences in antisocial behaviour
- H. Data
- I. Decision screen for multinomial choice treatment

Appendix A. Experimental instructions

Note: All treatments (beginning of the experiment)

Thank you for participating in this experiment. In this experiment, you can earn money. What you earn will depend upon your decision, and on the decision of another participant in the room. No data that you provide can be associated with your person; all data will be treated confidentially. Please follow the instructions carefully. These instructions explain how the experiment works. If any of the instructions are unclear, or if you have any questions, please raise your hand and I will come to you. Please do not communicate with any other participant until the experiment is over.

You will be randomly matched with one of the other participants (I call him/her your co-participant). Note that you will not learn who your co-participant is, neither during nor after the session. Likewise, your co-participant will not learn who you are. You will receive £6. Your co-participant will also receive £6.

A.1. Instructions for the 2S-Costly treatment:

You will have to decide whether or not to reduce £3 from your co-participant's earnings. If you decide to reduce your co-participant's earnings, this will cost you £0.60. If you

decide not to reduce your co-participant's earnings, this will cost you nothing. Your co-participant will make the same decision at the same time. In other words, he/she will have to decide whether or not to reduce £3 from your earnings. If he/she decides to reduce your earnings, this will cost him/her £0.60. If he/she decides not to reduce your earnings, this will cost him/her nothing.

After you make a decision, you will not interact with your co-participant again in today's experiment. At the end of the experiment, you will be paid your money earnings, and £2 for showing up. Do you have any questions? If so, please raise your hand.

A.2. Instructions for the 2S-Costless treatment:

You will have to decide whether or not to reduce £3 from your co-participant's earnings. If you decide to reduce your co-participant's earnings, this will cost you nothing. If you decide not to reduce your co-participant's earnings, this will cost you nothing either. Your co-participant will make the same decision at the same time. In other words, he/she will have to decide whether or not to reduce £3 from your earnings. If he/she decides to reduce your earnings, this will cost him/her nothing. If he/she decides not to reduce your earnings, this will cost him/her nothing either.

After you make a decision, you will not interact with your co-participant again in today's experiment. At the end of the experiment, you will be paid your money earnings, and £2 for showing up. Do you have any questions? If so, please raise your hand.

A.3. Instructions for the 2S-Benefit treatment:

You will have to decide whether or not to reduce £3 from your co-participant's earnings. If you decide to reduce your co-participant's earnings, this will increase your earnings by £0.60. If you decide not to reduce your co-participant's earnings, this will increase your earnings by £0. Your co-participant will make the same decision at the same time. In other words, he/she will have to decide whether or not to reduce \$3 from your earnings. If he/she decides to reduce your earnings, this will increase his/her earnings

by £0.60. If he/she decides not to reduce your earnings, this will increase his/her earnings by £0.

After you make a decision, you will not interact with your co-participant again in today's experiment. At the end of the experiment, you will be paid your money earnings, and £2 for showing up. Do you have any questions? If so, please raise your hand.

A.4. Instructions for the 1S-Benefit treatment:

Specific Instructions for Player 1: You will be Player 1 and your co-participant will be Player 2. You will have to decide whether or not to reduce £3 from your co-participant's earnings. If you decide to reduce your co-participant's earnings, this will increase your earnings by £0.60. If you decide not to reduce your co-participant's earnings, this will increase your earnings by £0. Your co-participant Player 2 will not make any decision.

Specific Instructions for Player 2: You will be Player 2 and your co-participant will be Player 1. You will not make any decision. Your co-participant Player 1 will have to decide whether or not to reduce £3 from your earnings. If he decides to reduce your earnings, this will increase his earnings by £0.60. If he decides not to reduce your earnings, this will increase his/her earnings by £0.

After you make a decision, you will not interact with your co-participant again in today's experiment. At the end of the experiment, you will be paid your money earnings, and £2 for showing up. Do you have any questions? If so, please raise your hand.

Appendix B. Experimental instructions for the neutral treatment

Thank you for participating in this experiment. In this experiment, you can earn money. What you earn will depend upon your decision, and on the decision of another participant in the room. No data that you provide can be associated with your person; all data will be treated confidentially. Please follow the instructions carefully. These instructions explain how the experiment works. If any of the instructions are unclear, or if you have any questions, please raise your hand and I will come to you. Please do not communicate with any other participant until the experiment is over.

You will be randomly matched with one of the other participants (I call him/her your co-participant). Note that you will not learn who your co-participant is, neither during nor after the session. Likewise, your co-participant will not learn who you are. You will receive £6. Your co-participant will also receive £6.

You will be choosing between two options: Option A and Option B. Your co-participant will make the same decision at the same time. In other words, he/she will be choosing between two options: Option A and Option B.

If you choose Option A and your co-participant chooses Option A, then you will earn £2.40 and your co-participant will earn £2.40. If you choose Option A and your co-participant chooses Option B, then you will earn £5.40 and your co-participant will earn £3. If you choose Option B and your co-participant chooses Option A, then you will earn £3 and your co-participant will earn £5.40. If you choose Option B and your co-participant chooses Option B, then you will earn £6 and your co-participant will earn £6.

After you make a decision, you will not interact with your co-participant again in today's experiment. At the end of the experiment, you will be paid your money earnings, and £2 for showing up. Do you have any questions? If so, please raise your hand.

Appendix C. Background of the participants

Table 3. Percentage of male and female subjects by treatment

Treatment	Female		Male	
	Frequency	Percent	Frequency	Percent
2S-Costly (T1)	12	60.00	8	40.00
2S-Costless (T2)	8	40.00	12	60.00
2S-Benefit (T3)	12	60.00	8	40.00
1S-Benefit (T4)	24	60.00	16	40.00
Total	56	56.00	44	44.00

Table 4. Percentage of subjects by country

Country	Frequency	Percent
Austria	1	1.00
China	38	38.00
Cyprus	1	1.00
Hong Kong	5	5.00
India	1	1.00
Japan	1	1.00
Korea, Republic of	1	1.00
Malaysia	2	2.00
Mauritius	1	1.00
Netherlands	1	1.00
Nigeria	3	3.00
Phillipines	1	1.00
Pakistan	1	1.00
Sri Lanka	1	1.00
Taiwan	1	1.00
Turkey	1	1.00
UK	33	33.00
United States	1	1.00
Vietnam	6	6.00
Total	100	100.00

Table 5. Percentage of subjects by field of study

Field of Study	Frequency	Percent
Accounting and Finance	9	9.00
Actuarial Sciences	4	4.00
Biology	1	1.00
Business and Management	17	17.00
Chemistry	1	1.00
Computer Sciences	1	1.00
Creative Writing	2	2.00
Economics	21	21.00
Education	2	2.00
Engineering	1	1.00
Environmental Sciences	6	6.00
Geophysics	1	1.00
History	4	4.00
Human Resources	1	1.00
International Development	7	7.00
Journalism	1	1.00
Languages	1	1.00
Law	3	3.00
Marketing	1	1.00
Mathematics	2	2.00
Media	4	4.00
Medicine	2	2.00
Meteorology & Oceanography	1	1.00
Music	1	1.00
Natural Science	1	1.00
Pharmacy	1	1.00
Politics	2	2.00
Social Science	1	1.00
Statistics	1	1.00
Total	100	100.00

Table 6. Descriptive statistics for age by treatment

Treatment	Observations	Age (years)				
		Mean	Median	S.D	Min	Max
2S-Costly (T1)	20	21.15	22.00	1.93	18	24
2S-Costless (T2)	20	23.20	21.50	4.84	19	39
2S-Benefit (T3)	20	23.05	22.00	4.42	18	35
1S-Benefit (T4)	40	22.10	22.50	2.25	18	27
Total	100	22.32	22.00	3.40	18	39

Table 7. Descriptive statistics for experimental earnings by treatment

Treatment	Observations	Experimental Earnings (in British Pounds)				
		Mean	Median	S.D	Min	Max
2S-Costly (T1)	20	8.00	8.00	0.00	8.00	8.00
2S-Costless (T2)	20	7.70	8.00	0.92	5.00	8.00
2S-Benefit (T3)	20	6.92	8.00	1.56	5.00	8.60
1S-Benefit (T4)	40	7.40	8.00	1.43	5.00	8.60
Total	100	7.48	8.00	1.25	5.00	8.60

Note: Experimental earnings include the participation fee of £2

Appendix D. Background of the participants in the neutral treatment

Table 8. Percentage of subjects by gender

Gender	Frequency	Percent
Female	19	63.33
Male	11	36.67
Total	30	100.00

Table 9. Percentage of subjects by country

Country	Frequency	Percent
China	12	40.00
Kenya	1	3.33
Korea, Republic of	1	3.33
Saudi Arabia	1	3.33
Taiwan	1	3.33
UK	13	43.33
United States	1	3.33
Total	30	100.00

Table 10. Descriptive statistics for age and experimental earnings

	Observations	Mean	Median	S.D	Min	Max
Age (years)	30	23.67	23	6.79	18	57
Earnings (£)	30	7.80	8	0.76	5	8

Note: Experimental earnings include the participation fee of £2

Table 11. Percentage of subjects by field of study

Field of Study	Frequency	Percent
Accounting and Finance	1	3.33
Actuarial Sciences	1	3.33
Brand Leadership	2	6.67
Business and Management	4	13.33
Creative Writing	1	3.33
Economics	9	30.00
Education	1	3.33
Environmental Sciences	2	6.67
Geophysics	1	3.33
Human Resources	1	3.33
International Development	1	3.33
Law	1	3.33
Mathematics	1	3.33
Media	2	6.67
Medicine	1	3.33
Politics	1	3.33
Total	30	100.00

Appendix E. Post-experimental questionnaire (all treatments)

Figure 6. Post experimental questionnaire's screens

Please answer the following questions. Your answers will stay anonymous.

What is your age?

What is your gender? Male
 Female

What is your field of study?

What is your native language? English
 Another language

What country were you born in?

Please provide feedback about the experiment here:

Appendix F. Gender differences in antisocial behaviour

In this section, I examine the data to test for gender differences in antisocial behaviour. This analysis is motivated by the experimental evidence, which suggest that gender is an important determinant of a variety of economic and strategic decisions (Eckel and Grossman, 1998; Croson and Gneezy, 2009), and on subjects' beliefs about the altruistic behaviour of men and women (Aguiar et al, 2009).

There are only a few experimental economics studies dealing with gender differences in antisocial behaviour. Abbink and Hermann (2011) and Kessler et al (2012) found no evidence for gender effects in antisocial behaviour, however, in a non-experimental study, Ghiglieri (1999) showed evidence that males are far more aggressive than females.

Table 12 and Figure 7. Destruction choices across gender and treatment show the percentage of male and female subjects who chose to reduce their co-participant's earnings by treatment. Overall, 26 percent of males chose to reduce their co-participant's earnings and 27 percent of females chose to reduce their co-participant's earnings. There was not statistically significant difference across gender.

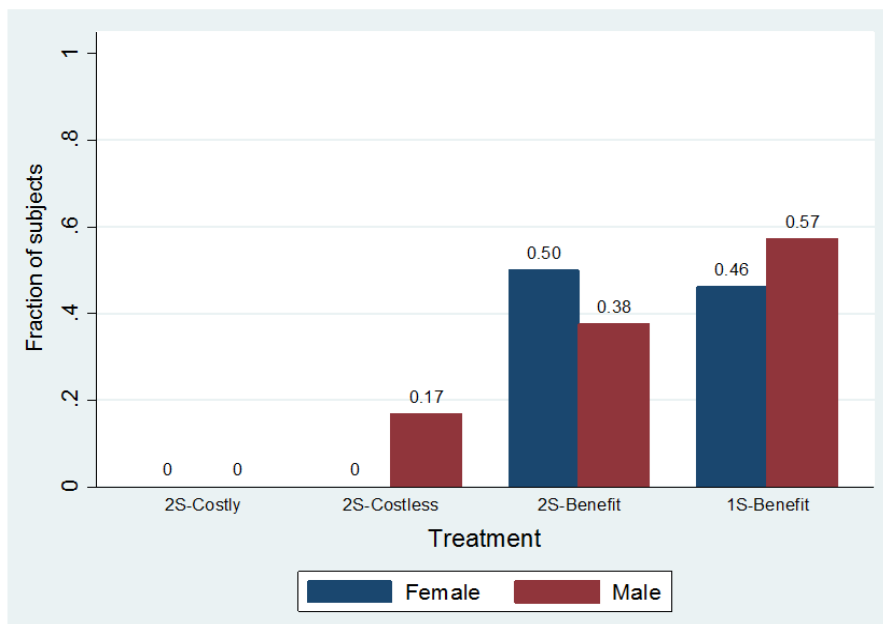
Within each treatment, I did not find any gender effect. In 2S-Costly, when destruction is costly, no one chose to reduce the other partner's earnings. In 2S-Costless, males were more likely to reduce their co-participant's earnings than females. The difference, however, was not statistically significant. In 2S-Benefit, more females chose to reduce their co-participant's earnings than males. The difference however was not statistically significant. Therefore, male and female subjects acted remarkably similarly with respect to the decision to reduce their co-participant's earnings.

Across treatments, more males were willing to decrease their co-participant's earning when destruction was costly, and even more when they earned from destruction. In contrast, more women were willing to decrease their co-participant's earning when they earned from destruction.

Table 12. Fraction of subjects who chose to reduce by treatment and gender

Treatments	Male	Female	Fisher's exact (Male vs. Female)
Total	0.26	0.27	No difference (p=0.565)
2S-Costly (T1)	0.00	0.00	-
2S-Costless (T2)	0.17	0.00	No difference (p=0.347)
2S-Benefit (T3)	0.38	0.50	No difference (p=0.465)
1S-Benefit (T4)	0.57	0.46	No difference (p=0.500)
Fisher's exact	Difference at 5% (p=0.048)	Difference at 1% (p=0.002)	-

Figure 7. Destruction choices across gender and treatment



Appendix G. Cultural differences in antisocial behaviour

The experiment in this chapter was conducted in the United Kingdom with subjects with different cultural backgrounds. In this section, I examine the data to test for cultural differences in antisocial behaviour. Previous experimental evidence showed that economic decisions might also vary across cultures (e.g. Gächter and Herrmann, 2009).

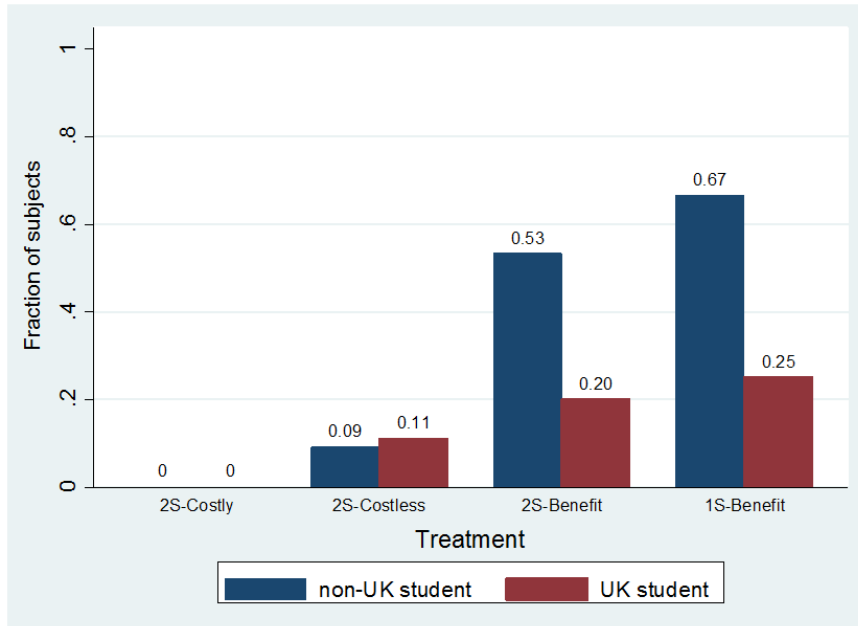
Table 13 and Figure 8 display the fraction of UK and non-UK subjects who chose to reduce their co-participant's earnings in each treatment. Overall, non-UK subjects are more likely to reduce their co-participant's earnings than UK subjects are.

I found differences between UK and non-UK students in antisocial behaviour in 1S-Benefit. Non-UK subjects displayed statistically significantly higher rates of destruction than UK subjects did (1-sided Fisher's test, $p = 0.085$). Future research may wish to explore the extent to which these findings hold across other different nationalities.

Table 13. Fraction of subjects who chose to reduce by treatment and nationality

Treatments	UK student	non-UK student	Fisher's exact (UK vs. non-UK student)
Total	0.15	0.32	Different at 10% ($p=0.080$)
2S-Costly (T1)	0.00	0.00	-
2S-Costless (T2)	0.11	0.091	No difference ($p=0.711$)
2S-Benefit (T3)	0.20	0.53	No difference ($p=0.221$)
1S-Benefit (T4)	0.25	0.67	Different at 10% ($p=0.085$)
Fisher's exact	No difference ($p=0.733$)	Different at 1% ($p=0.000$)	-

Figure 8. Destruction choices across culture and treatment



Appendix H. Data

Table 14. Data for 2S-Costly treatment

2S-Costly treatment (T1)			
Subject	Session 1	Subject	Session 2
1	0	1	0
2	0	2	0
3	0	3	0
4	0	4	0
5	0	5	0
6	0	6	0
7	0	7	0
8	0	8	0
9	0	9	0
10	0	10	0

Note: 1 = Subject chooses to reduce the other subject's earnings; 0 = Subject chooses to do not reduce the other subject's earnings.

Table 15. Data for 2S-Costless treatment

2S-Costless treatment (T2)			
Subject	Session 1	Subject	Session 2
1	1	1	0
2	0	2	1
3	0	3	0
4	0	4	0
5	0	5	0
6	0	6	0
7	0	7	0
8	0	8	0
9	0	9	0
10	0	10	0

Note: 1 = Subject chooses to reduce the other subject's earnings; 0 = Subject chooses to do not reduce the other subject's earnings.

Table 16. Data for 2S-Benefit treatment

2S-Benefit treatment (T3)			
Subject	Session 1	Subject	Session 2
1	1	1	1
2	0	2	0
3	1	3	0
4	0	4	1
5	0	5	1
6	0	6	0
7	1	7	1
8	0	8	1
9	0	9	0
10	0	10	1

Note: 1 = Subject chooses to reduce the other subject's earnings; 0 = Subject chooses to do not reduce the other subject's earnings.

Table 17. Data for 1S-Benefit treatment

1S-Benefit treatment (T4)							
Subject	Session 1	Subject	Session 2	Subject	Session 3	Subject	Session 4
1	-	1	-	1	1	1	-
2	-	2	0	2	-	2	0
3	-	3	1	3	-	3	-
4	0	4	1	4	-	4	0
5	-	5	-	5	-	5	0
6	1	6	-	6	0	6	1
7	-	7	-	7	-	7	0
8	0	8	-	8	1	8	-
9	1	9	0	9	0	9	-
10	0	10	1	10	1	10	-

Note: '-' indicates that the subject was Player 2. Hence, no decision was made. 1 = Subject chooses to reduce the other subject's earnings; 0 = Subject chooses to do not reduce the other subject's earnings.

Table 18. Data for Neutral treatment

Neutral treatment					
Subject	Session 1	Subject	Session 2	Subject	Session 3
1	B	1	B	1	B
2	B	2	B	2	A
3	B	3	B	3	B
4	B	4	B	4	B
5	B	5	B	5	B
6	B	6	B	6	B
7	B	7	B	7	B
8	B	8	B	8	A
9	B	9	B	9	B
10	B	10	B	10	B

Note: A indicates subject chooses Option A ("reduce the other subjects's earnings"); B indicates subject chooses Option B ("do not reduce the other subject's earnings").

Chapter 2

The Effects of the Relative Price and Monetary Stakes on the Dictator's Behaviour in Giving, Taking and Burning

1. Introduction

Dictator games have been used as a vehicle for studying unselfish behaviour. In a standard Dictator game (Kahneman et al., 1986; Forsythe et al., 1994) two subjects are randomly matched and assigned either to the role of Dictator or Recipient. The Dictator receives an endowment and chooses the amount of his/her endowment to transfer to the Recipient. The Recipient must accept the division. The sub-game perfect Nash equilibrium of the Dictator game is for the Dictator to transfer zero.

A large body of research has shown that many Dictators are willing to allocate a non-trivial amount of money to the Recipient.¹⁷ Several possible explanations have been offered to explain this behaviour. For instance, models of inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), altruism (Andreoni and Miller 2002) and Rawlsian 'social welfare' preferences (Charness and Rabin, 2002).

In a seminal paper, Andreoni and Miller (2002, earlier version 1998) introduced modified Dictator games. Subjects played a series of Dictator games where the amount to divide and the relative price of giving varied across games. Andreoni and Miller (2002) found that altruism is rational. Moreover, there is substantial heterogeneity across subjects:

¹⁷ See Camerer (2011), Guala and Mittone (2010), and Engel (2011) for excellent surveys on experimental evidence.

some dictators do leave nothing, but others give away as much as 50% of the pie.¹⁸ In this experiment, altruism was highly sensitive to the price of giving.¹⁹

More recently, researchers (List, 2007; Bardsley, 2008; Cappelen, 2013) have showed that the introduction of additional taking options in a dictator game decreased the amount transferred to the Recipient. For instance, Eichenberger and Oberholzer-Gee (1998), List (2007), and Bardsley (2008) showed that Dictators are less willing to share money when the choice set also includes the option to take money.²⁰ This finding holds, even though there is no-uncertainty about the origin of the endowment (Cappelen et al. 2013). This evidence is not consistent with previous models of social preferences.²¹ Possible explanations for this behaviour include experimenter demand effect (Oechssler, 2010; Zizzo, 2010), procedural preferences (Chlaß and Moffatt, 2012), social norms (Krupka and Weber, 2013), and context-dependence (Schotter et al., 1996).

This chapter explores the effects of the relative price and monetary stakes on the Dictators' decision in three different contexts. More specifically, I ask whether and to what extent Dictators are willing to transfer, take, and burn money, even at cost to

¹⁸ A series of experimental studies has employed Andreoni and Miller's (2002) game. Fisman et al (2007) allowed for non-linear budget constraints, and analysed three-person dictator games. Andreoni and Vesterlund (2001), and Visser and Roelofs (2011) investigated the demand for altruism by gender. Harrison and Johnson (2006) examined whether altruism depends upon who is specified as the residual claimant.

¹⁹ Eckel and Grossmann (1998) showed that the demand for fairness was elastic for women, but inelastic for men. The effect of the monetary stakes on the amount transferred has also been investigated. For instance, Telser (1993) argued that if the monetary stake raises, the amount offered decreases because the price of fairness increases. Hoffman et al. (1996), however, did not find such an effect. Brañas-Garza (2006) found that if the Dictator has information about the Recipient's wealth, the amount transferred increases.

²⁰ In addition, Engel (2011) ran a meta-analysis and found no effect of 'taking' in dictators' allocations. On the contrary, Zhang and Ortmann (2014) used Engel (2011)'s data and found that take options have negative and significant effect on giving.

²¹ In a similar vein, other experimental studies showed significant behavioural changes in Dictator games. For instance, Dana et al. (2006), Broberg et al (2007) and Lazear et al (2012) showed that Dictators are willing to sacrifice their own payoffs to avoid an opportunity to be generous. Furthermore, Dana et al. (2007) revealed that when Dictators are given the opportunity to remain ignorant of how their choices affect others, many altruistic Dictators revert to selfish choices.

themselves, and how the individuals' decisions in these situations depend on the initial endowment and the relative price.²²

The experimental set up employed variants of the Dictator game. Subjects were randomly divided into two groups. Half were assigned to the role of Dictator and the other half to the role of Recipient. Both Dictators and Recipients received an initial endowment. Roles remained fixed throughout the whole experiment. Each Dictator played in eighteen scenarios. For each Dictator, scenarios were shown in random order. In each scenario, Dictators were required to allocate a 'self' and 'other' payoff. The relative price or the trade-off between own payoff and Recipient's payoff, as well as the Dictator's initial endowment, varied across decisions.²³ Furthermore, Dictators made decisions in three separate contexts. Dictators can increase the Recipient's payoff by decreasing their own payoff (giving), decrease the Recipient's payoff by increasing his own payoff (taking), or decrease the Recipient's payoff by decreasing their own payoff (burning). Dictators did not received any feedback between decisions. After making their decisions, one of the games was randomly chosen and the payoffs were implemented in accordance with the decision made in the chosen game.

The data showed that approximately 68% of our Dictators made stable choices across giving and 48% of Dictators made stable choices across taking. In addition, the effect of the monetary stake, however, depends on the experimental context. The monetary stake effect is strong and significant on giving, but weak and insignificant on taking. In the case of burning, the picture is ambiguous: an increase in the price of burning, not necessarily decreased burning; and a decrease in the Dictator's endowment, no necessarily encouraged burning as posited by inequality aversion models.

²² Experimental evidence also shows that a significant proportion of subjects are willing to harm others, even though it is costly to themselves (Zizzo and Oswald, 2001; Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011; Sadrieh and Schröder, 2017).

²³ In this chapter, the set of payoff possibilities available to the Dictator changed across tasks, thus the experiment was not designed to test for framing effects. See Cookson (2008) for framing effects in public goods and Abbink et al (2011) for framing effects in antisocial preferences.

Similar to what has been reported by Fehr and Smith (1999), Dictators appear to be primarily concerned with inequality aversion in giving, but this concern disappears when the Dictator can take money from the Recipient to themselves. I interpret this result as implying that the experimental context has an important effect on the variation of the Dictator's endowment. The context changed from dividing the Dictator's endowment to dividing the Recipient's endowment.

This study contributes to the experimental literature in two important aspects. First, I extend the analysis of the choice set effect by introducing burning options. Existing literature on the choice set rarely confront subjects with burning decisions.²⁴ Thus, this study provides a broad picture of individuals' decisions. Second, in terms of the analysis of the choice set at individual level, most existing research on this area does not separate the effect of 'give' and 'take' options.²⁵ This study isolate the effect of the variation of the choice set.

The remainder of this chapter is organised as follows. Section 2 reviews the related literature. Section 3 describes the experimental design and the procedures. Section 4 presents the theoretical predictions based on the models of social preferences. Section 5 presents the experimental results. Section 6 discusses the results and concludes.

2. Related literature

This study is related to an influential line of research that examines other-regarding preferences across different contexts, but rely on individual-level analysis. For instance, Suvoy (2003) implemented modified dictator games to investigate framing effects in social preferences. In one frame, the Dictator chooses how much to transfer to the

²⁴ In one session, Andreoni and Miller (2002) elicited antisocial preferences, but subjects chose the value of a token for herself and for another player.

²⁵ Bardsley (2008) ran a third experiment where he separate giving and taking. He found that the average Recipient's payoff decreased when Dictators only take. However, he used a between-subject design. Zizzo (2004) allowed subjects to burn money or take money from another player. He found that when stealing was allowed burning rates decreased significantly compared to burning alone. In the stealing condition, subjects were rewarded for their actions.

Recipient. In the other frame, the Dictator chooses how much to take from the Recipient's endowment. Suvoy (2003) found no framing effects on allocations. In the current study, I do not study framing effects, so I varied the payoffs possibilities across choice sets.

Brosig et al (2007) used modified dictator games to investigate the consistency of individual behaviour. Dictators made decision in two frames. In one frame, Dictators could increase the Recipient's payoff by decreasing their own payoff. In another frame, Dictators could increase their own payoff by decreasing the Recipient's payoffs. Brosig et al. (2007) found that social preferences were not stable across frames nor across time, but behaviour was stable across the same games. In the current study, Dictators played a series of games only once. I also randomised the sequence of the games at individual level.

Heinrich et al (2009) used modified dictator games to investigate framing effects. Dictators played in two different frames. The relative price of giving varies across games. Dictators chose a payoff distribution, while the Recipients made choices by selecting a game. Heinrich et al (2009) found that Dictators did not respond to the price of giving neither in the frame of taking nor in the frame of giving, but they were affected by whether the Recipient chose the game. The main difference between Heinrich et al (2009) and the current study is that Heinrich et al (2009) used the strategy method and the sequence of the games was predetermined.

Blanco et al (2011) used a within-subject design and examined other-regarding preferences across different contexts. Subjects played an ultimatum game, a dictator game, a sequential prisoner's dilemma, and a public good game. Blanco et al (2011) found that that the inequality aversion model explained choices at the aggregate level, but performed less well at the individual level. In the current study, I used only modified dictator games.

Iriberry and Rey-Biel (2011) used a within-subject design to investigate the role of role uncertainty in Dictator games. All subjects made decisions in 16 scenarios. In each scenario, Deciders chose among three actions: a selfish action, an altruistic costly behaviour and a spiteful costly behaviour. The order of the options varied across games.

In one session, subjects knew whether they were Deciders or Receivers, whereas in another session, they did not have this information. Iriberry and Rey-Biel (2011) found that altruistic choices were more frequent in role uncertainty than in role certainty, selfish choices were the same in both treatments, and spiteful choices were not relevant.

Chlaß and Moffatt (2012) used a within-subject design, where the same subject played a Dictator giving, and a Dictator taking. They found that giving was lower when subjects played a Dictator taking compared to the case when subjects played a Dictator giving. However, subjects in the Dictator taking could also give money to the Recipient.

Iriberry and Rey-Biel (2013) examined whether altruistic behaviour is affected by the belief on the altruistic behaviour of other dictators. Iriberry and Rey-Biel (2013) found that subjects' responses to information about other Dictators' giving varied systematically across social preference types. Social information had little effect on 'selfish' types and tended to affect those with interdependent preferences in the direction of fostering more selfishness.

Jakiela (2013) implemented a within-subject design and examined responses to price changes in two frames. In one stage, Dictators performed a task to earn money. In another stage, both subjects chose how to divide the money. Jakiela (2013) found that changing the source of the budget set, changed preferences for altruism (subjects transferred more of their budget set in the taking frame than in the giving frame), but it did not change willingness to trade-off equity and efficiency. Jakiela (2013) did not separate Dictators and Recipients nor control for the subject's initial endowment.

Korenok et al (2014) examined whether no taking is equivalent to giving. Subjects played in nine scenarios. Depending on the scenario, subjects chose how much to give or to take. The amounts of Dictator's endowment and the amount of the Recipient's endowment that the Dictator may take varies across scenario. In all scenarios, the sum of final payoffs was \$20. The minimum payoff to the Recipient varied across scenarios. Korenok et al (2014) found that giving was not equivalent to not taking: the average payoff to the Recipient increased with the introduction of the taking option. They suggested that the 'cold prickle' of taking was stronger than the 'warm glow' of giving.

Brandts et al (2015) examined the impact of the relative position and the price of sacrificing money. They ran an experiment using a binary version of the Dictator game. Subjects were given a list of 21 pairs of payoff vectors, and they had to choose one of the two payoff vectors in all 21 cases. In some cases, the Dictator increased the other's payoff. In other cases, the Dictator decreased the other's payoff. The cost of increasing and decreasing varied across games. At the end of the experiment, one of the 21 payoff vector pairs was randomly chosen. Brandts et al (2015) found that subjects were influenced by the price of sacrificing money and by whether they chose from a strong or from a weak position. In these games, the initial endowment did not change.

Zhang and Ortmann (2016) examined whether pro-social and anti-social behaviour was context-dependent. There were three different treatments. In the baseline, subjects made decisions in a Dictator game and in a joy-of destruction game. In the other two treatments, subjects made decisions in a Dictator game with taking options and in a joy of destruction game with giving options. The amounts to take varied across treatments. Zhang and Ortmann (2016) found that fewer subjects chose to destroy money in the joy of destruction games when the choice set includes the option of adding money to Recipients' endowment. The current study differs from this paper in that destruction choices were costly. I also separate contexts and randomised the order of the games.

Sadrieh and Schröder (2016) examined the relationship between anti-social and pro-social behaviour on an individual level in a money burning game. Each Dictator made two decisions: he/she decided how much to give and how much to destroy. Dictators could neither give nor destroy. After both decisions were made, a random draw determines which decision is payoff relevant for the Dictator and the Receiver. Sadrieh and Schröder (2016) found that within very similar games, individuals behaved both pro- and antisocially towards the same individuals, where the extent of giving was generally higher than the extent of destruction.

3. Experimental design and procedures

The experimental design employed a series of variants of Dictator games. Subjects were randomly divided into two groups. Half were assigned to the role of Dictator and the other half to the role of Recipient. Both Dictators and Recipients received an initial endowment. Roles remained fixed throughout the whole experiment.

Each dictator engaged in up to eighteen allocation decisions. Dictators were required to allocate a 'self' and 'other' payoff. The Dictator's initial endowment varied across decisions. To isolate the effect of the choice set, Dictators made decisions in three separate contexts. Dictators can increase the Recipient's payoff by decreasing their own payoff, decrease the Recipient's payoff by increasing his own payoff, or decrease the Recipient's payoff by decreasing their own payoff. The set of payoff possibilities available to the Dictator changed across tasks.

After making their decisions, Dictators were matched with a Recipient. Only one of the eighteen decisions was randomly chosen for the final payoffs. I describe the experimental design in more detail below.²⁶

3.1. Dictator giving

Dictators can transfer to the Recipient any amount of their own endowment in increments of 50 pence. (E_D) is the Dictator's endowment and (E_R) is the Recipient's endowment. The amount transferred g was multiplied by a transfer rate p . Hence, the Recipient earned $g \times p$. The transfer rate and the Dictator's initial endowment varied. The Dictator's payoff and the Recipient's payoff function, π_D and π_R , were respectively:

$$\begin{aligned}\pi_D &= E_D - g \\ \pi_R &= E_R + g \times p\end{aligned}\tag{1}$$

²⁶ It is noteworthy that the manipulation of the parameters for the relative price and the size of the Dictator's endowment was identical across domains

The relative price of giving indicates the amount of money that the Dictator must give up in order to increase by one unit of money the Recipient's payoff. Dictators made allocations in nine games. Table 1 shows a summary of the games. Consider for example Game 1, where the Dictator's endowment is £3 and the Recipient's endowment is £6. The transfer rate is two. If the Dictator chooses to transfer £1.5, the Dictator's payoff is £1.5 and the Recipient's payoff is £9.²⁷

Table 1. Dictator games giving

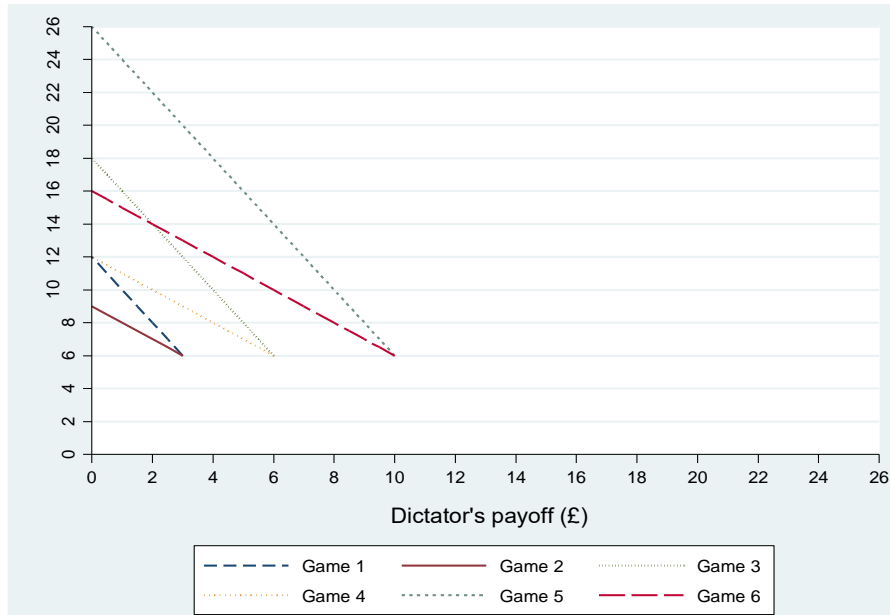
Game	Domain	Number of dictators	Dictator's endowment (£)	Recipient's endowment (£)	Minimum transfer (£)	Transfer rate	Relative price of giving	Dictator's payoff (£)		Recipient's payoff (£)	
								Max	Min	Max	Min
1	Giving	69	3	6	0.50	2	0.50	3.00	0.00	12.00	6.00
2	Giving	69	3	6	0.50	1	1.00	3.00	0.00	9.00	6.00
3	Giving	69	6	6	0.50	2	0.50	6.00	0.00	18.00	6.00
4	Giving	69	6	6	0.50	1	1.00	6.00	0.00	12.00	6.00
5	Giving	69	10	6	0.50	2	0.50	10.00	0.00	26.00	6.00
6	Giving	69	10	6	0.50	1	1.00	10.00	0.00	16.00	6.00

Note: Dictator's payoffs and Recipient's payoffs do not include the show-up fee.

Figure 1 displays the payoff possibilities in each game. The X-axis shows the Dictator's payoff and the Y-axis shows the Recipient's payoff. The set of payoff possibilities differed across games. Each game represents a downward sloping 'budget line'. These games are presented in random order to each Dictator.

²⁷ In a standard dictator game, the dictator's endowment is fixed and the relative price of giving, that is, the amount the dictator has to give up to increase by one unit the recipient's payoff, is one. Thus, the recipient's payoff is completely determined by the amount passed to the recipient

Figure 1. Payoffs available to the Dictator for each Dictator giving



3.2. Dictator taking

Dictators can take any amount from the Recipient's endowment in increments of 50 pence. (E_D) is the Dictator's endowment and (E_R) is the Recipient's endowment. The amount taken t was multiplied by a take rate h . Hence, the Dictator earned $t \times h$. The take rate and the Dictator's initial endowment varied. The Dictator's payoff and the Recipient's payoff functions, π_D and π_R , were respectively:

$$\pi_D = E_D + t \times h \quad (2)$$

$$\pi_R = E_R - t$$

The relative price of taking indicates the amount of money that the Dictator must take from the Recipient to increase by one unit of money his/her own payoff. The inverse of the relative price of taking indicates the relative price of giving. Dictators made allocations in nine games. Table 2 shows a summary of the games. Consider for example Game 11, the Dictator's endowment is £3, the Recipient's endowment is £6, and the take rate is one. If the Dictator chooses to take £1.5, the Dictator's payoff is £4.5 and the Recipient's payoff is £4.5.

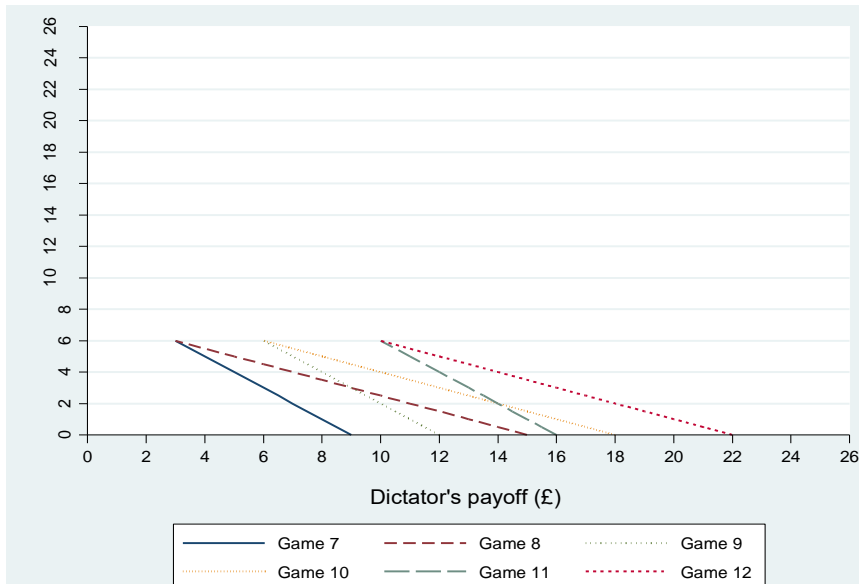
Table 2. Dictator games taking

Game	Domain	Number of dictators	Dictator's endowment (£)	Recipient's endowment (£)	Minimum take (£)	Take rate	Relative price of taking	Dictator's payoff (£)		Recipient's payoff (£)	
								Max	Min	Max	Min
7	Taking	69	3	6	0.50	1	1.00	9.00	3.00	6.00	0.00
8	Taking	69	3	6	0.50	2	0.50	15.00	3.00	6.00	0.00
9	Taking	69	6	6	0.50	1	1.00	12.00	6.00	6.00	0.00
10	Taking	69	6	6	0.50	2	0.50	18.00	6.00	6.00	0.00
11	Taking	69	10	6	0.50	1	1.00	16.00	10.00	6.00	0.00
12	Taking	69	10	6	0.50	2	0.50	22.00	10.00	6.00	0.00

Note: Dictator's payoffs and Recipient's payoffs do not include the show-up fee.

Figure 2 displays the payoff possibilities available to the Dictator for each game (in British pounds). The X-axis represents the Dictator's payoff and the Y-axis represents the Recipient's payoff. The Dictator chooses a payoff to self and a payoff to the Recipient along this line. These games are presented in random order to each Dictator.

Figure 2. Payoffs available to the Dictator for each Dictator taking



3.3. Dictator burning

Dictators can reduce (burn) any amount from the Recipient's endowment in increments of 50 pence. (E_D) is the Dictator's endowment and (E_R) is the Recipient's endowment. The amount burnt b was multiplied by a burn rate q . Hence, the recipient lost $b \times q$. The burn rate and the Dictator's initial endowment varied. Burning also decreased the Dictator's payoff. The Dictator's payoff and the Recipient's payoff functions, π_D and π_R , were respectively:

$$\begin{aligned}\pi_D &= E_D - b \\ \pi_R &= E_R - b \times q\end{aligned}\tag{3}$$

The relative price of burning indicates the amount of money that the Dictator must give up in order to decrease by one unit of money the Recipient's payoff. Dictators made allocations in nine games. Table 3 shows a summary of the games.

Table 3. Dictator games burning

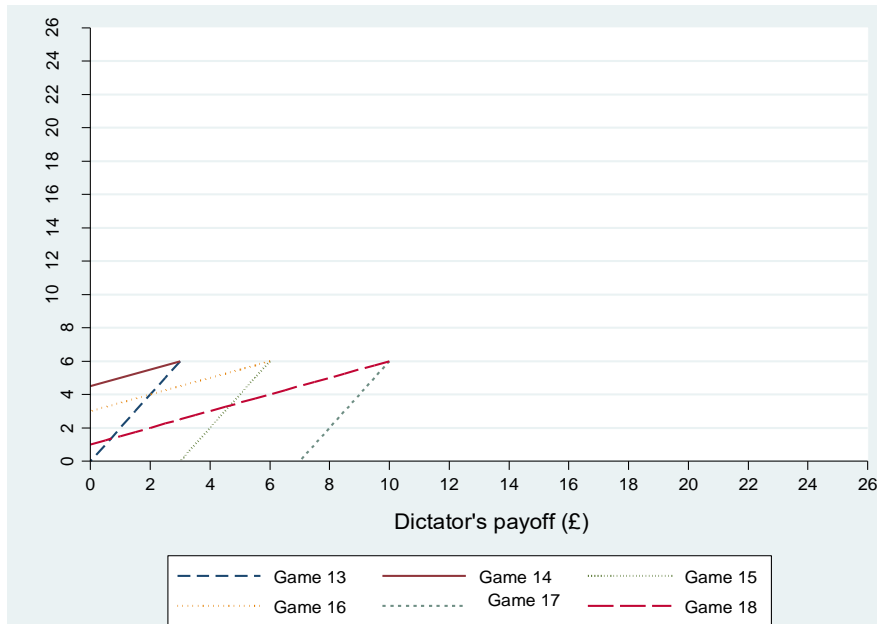
Game	Domain	Number of dictators	Dictator's endowment	Recipient's endowment	Minimum burn	Burn rate	Relative price of burning	Dictator's payoff (£)		Recipient's payoff (£)	
			(£)	(£)	(£)			Max	Min	Max	Min
13	Burning	69	3	6	0.50	2	0.50	3.00	0.00	6.00	0.00
14	Burning	69	3	6	0.50	0.5	2.00	3.00	0.00	6.00	4.50
15	Burning	69	6	6	0.50	2	0.50	6.00	3.00	6.00	0.00
16	Burning	69	6	6	0.50	0.5	2.00	6.00	0.00	6.00	3.00
17	Burning	69	10	6	0.50	2	0.50	10.00	7.00	6.00	0.00
18	Burning	69	10	6	0.50	0.5	2.00	10.00	4.00	6.00	1.00

Note: Dictator's payoffs and Recipient's payoffs do not include the show-up fee.

Figure 3 displays the payoff possibilities available to the Dictator for each game in the domain of burning. The X-axis represents the Dictator's payoff and the Y-axis represents the Recipient's payoff. Each set of payoff possibilities differs across games. Each game

represents an upward sloping 'budget line'. The Dictator chooses a payoff to self and a payoff to the Recipient along this line. These games are presented in random order to each Dictator.

Figure 3. Payoffs available to the Dictator for each Dictator burning



3.4. Procedures

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) computerized laboratory at the University of East Anglia (UEA) between May and June 2014.²⁸ The participants were undergraduate students recruited from a wide range of disciplines using the ORSEE on-line recruitment system (Greiner, 2015). None of the subjects had previously participated in a similar experiment. The experiment was programmed and conducted using the software z-Tree (Fischbacher, 2007).

The experiment took place over seven experimental sessions, six with 20 subjects and one session with 18 subjects. In total, 138 subjects participated in the experiment. Each

²⁸ Sessions 1 and 2 were run in May 2014, and sessions 3, 4, 5, 6 and 7 were run in June 2014.

subject participated in only one session. All subjects received a participation fee of £3, regardless of their decisions.

For each session, subjects were assembled in a single room. Upon arrival, subjects were randomly seated at visually separated computer terminals in order to avoid facial or verbal communication between subjects. Subjects were given printed instruction, a blank sheet of paper, a receipt form, and a calculator. A copy of these instructions can be found in this chapter's **Appendix A. Experimental instructions**.

The presentation of the experimental instructions was as neutral as possible avoiding terms such as 'giving', 'taking', 'burning', or 'game'. The experimenter read the instructions aloud as the students followed along on their own computer's screens. After reading the instructions, subjects were given an opportunity to ask questions. Communication between subjects was prohibited. The experimenter answered the questions about the experiment individually.

Among the participants, 15 studied Economics and 7 studied Business and Management. The average age of the subjects was 23.42 years (Std. Dev. 5.79, min 18 max 66). The percentage of women was 61.59%, and 38.41% of the subjects speak English as their first language. **Appendix B. Background of the participants** of this chapter provides the characteristics of the subjects.

Before the experiment began, all subjects were required to answer a series of control questions to verify subjects' understanding of the instructions. Four of the eight questions were intended to test whether subjects could correctly calculate payoffs. Subjects were encouraged to use their calculators and the blank paper to make any calculation. If a subject answered a question incorrectly or forgot to answer a question, a pop-up window came out on the subject's screen instructing the subject to revise her/his choice or complete the question. None of the subjects failed to answer all the required questions. Figure 4 shows the control questions' screens.

Figure 4. Control questions' screens

Question 1: Suppose that you are Player 1 and another participant is Player 2. You have been given £16 and Player 2 has been given £10. Each £0.50 that you give to Player 2 will increase Player 2's earnings by £1 and will reduce your earnings by £0.50. You chose to give £3 to Player 2.

a. What would be your earnings in this scenario?

b. What would be Player 2's earnings in this scenario?

Question 2: Suppose that you are Player 1 and another participant is Player 2. You have been given £7 and Player 2 has been given £10. Each £0.50 that you reduce from Player 2 will decrease Player 2's earnings by £0.50 and will increase your earnings by £1. You chose to reduce £5 from Player 2.

a. What would be your earnings in this scenario?

b. What would be Player 2's earnings in this scenario?

Question 3: Suppose that you are Player 1 and another participant is Player 2. You have been given £5 and Player 2 has been given £5. Each £0.50 that you reduce from Player 2 will decrease Player 2's earnings by £0.50 and will decrease your earnings by £0.50. You chose to reduce £1 from Player 2.

a. What would be your earnings in this scenario?

b. What would be Player 2's earnings in this scenario?

OK

Question 4: Suppose that you are Player 2 and another participant is Player 1. You have been given £12 and Player 1 has been given £9. Each £0.50 that Player 1 will reduce from your money will decrease your earnings by £1 and will decrease Player 1's earnings by £0.50. If your earnings would have been £6 and Player 1's earnings would have been £6.

How many pounds Player 1 chose to reduce from your earnings?

Question 5: How many decisions will Player 1 have to make?

Question 6: How many decisions will Player 2 have to make?

Question 7: How many decisions of Player 1's decisions will be randomly chosen by the computer to determine your earnings?

Question 8: How many pounds will you be paid for showing up, in addition to your earnings from this experiment?

OK

After all questions were answered correctly, subjects were randomly divided by the computer into two groups. Anonymity was preserved throughout the experiment. Players 1 did not learn who Players 2 were, neither during nor after the session. Likewise, Players 2 did not learn who Players 1 were.

The computer presented to each Dictator the scenarios in random order. Player 2 never made any decision throughout the session, but they knew about the procedures in

advance.²⁹ In each scenario, Dictators were asked to choose a payoff to self and a payoff to the recipient.

Figure 5 shows a sample screenshot of one of the scenarios faced by a Dictator. In each scenario, Dictators had information about the recipient's wealth. In addition, Dictators were shown a list of different payoffs distributions associated to their decision. This made it easy for the Dictators to know the outcome of his/her decision. All payoff earnings were shown in British pounds. Dictators made their decisions by selecting one of the radio buttons.

Figure 5. Dictator task's screen for game 2



After making his decision, a pop up window came out on Player 1's screen to confirm his/her decision. Once Player 1 confirmed his decision, the next scenario was shown and Player 1 was not able to go back to her previous decision. All decisions were to be made without any information on the decision of the other Dictators.

Only when all participants had made their decisions in the eighteen games, the computer randomly paired one players in the first group with a player in the second group, then

²⁹ Recipients were given a quiz to reduce the boredom recipients may face while waiting for dictators to make their choices.

the computer chose randomly one of games. The decision made by the Dictator in this game determined the final payoffs.³⁰

At the end of the experiment, all subjects were privately informed about the random chosen game and the relevant decision in that game. This information was shown on the subject's screen. At no point were subjects told their partner's payoffs nor other subjects' earnings.

I also asked subjects to complete a socio-demographic questionnaire, which had no influence on their payoffs. The questionnaire can be found in the **Appendix C. Post-experimental questionnaire** at the end of the chapter. The questionnaire was designed to gather data on gender, field of study, country of origin, and language. Players also provided feedback on the experiment. The questionnaire was also computerized using z-Tree software (Fischbacher 2007).

Subjects were called out individually to receive their payment in cash. They left the room one at a time. The subjects were paid according to the payoffs in the randomly selected scenario. An experimental session lasted about an hour (including reading the instructions and filling out questionnaires). On average, a subject earned approximately was £6.59 (this amount does not include the participation fee).

4. Theoretical predictions

In this section, I present three models social preferences, which can be relevant to the experimental design in this study. The first model is altruism; the second model is spitefulness; and the third model is inequity aversion. I derive theoretical predictions.

³⁰ Hey and Lee (2005) found no differences between subjects being paid for one randomly chosen decisions and paid for all the decisions.

4.1. Narrow self-interest

Before introducing the models of social preferences, I present the narrow self-interest model, which will be used as a benchmark. The self-interest model is based on the assumption that individual $i = 1, 2, \dots, n$ only cares about his/her own monetary payoff. Therefore, individual's preferences can be represented by the following utility function:

$$U_1 = \pi_1 \tag{1}$$

where π_1 , indicates the monetary payoff to the Dictator. It follows from (1) that $\frac{dU_1}{d\pi_1} = 1$. The higher the individual's monetary payoff, the higher the individual's utility. The model of self-interest makes the following propositions:

Proposition 1: *A self-interested Dictator will choose to transfer zero.*

An increase in the amount transfer to the Recipient will always decrease the Dictator's payoff and, consequently, will decrease the Dictator's utility. A rational self-interested Dictator who cares solely about his/her own monetary payoff will choose to transfer zero.

Proposition 2: *A self-interested Dictator will choose to take £6.*

An increase in the amount taken from the Recipient's endowment will always increase the Dictator's payoff and, consequently, will increase the Dictator's utility. A rational self-interested Dictator who cares solely about his/her own monetary payoff will choose to take the maximum amount from the Recipient, which is £6.

Proposition 3: *A self-interested Dictator will choose to burn zero.*

Since burning is costly, an increase in the amount burnt from the Recipient's endowment will always decrease the Dictator's payoff and hence will decrease the Dictator's utility.

A rational self-interested Dictator who cares solely about his/her own monetary payoff will therefore burn zero.

4.2. Altruism

The model of altruism (Andreoni and Miller, 2002, Andreoni 2010) is based on the assumption that individual $i = 1, 2, \dots, n$ cares not only about his/her own monetary payoff, but also cares positively about the monetary payoff of the other. Therefore, in the two-individual version, an individual's preferences can be represented by the following utility function:

$$U_1(\pi_1, \pi_2) = \pi_1 + \gamma_1 \pi_2 \quad (2)$$

where π_1 , indicates the monetary payoff to the Dictator; π_2 indicates the monetary payoff to the Recipient; and γ_1 is a parameter of altruism. It is assumed that $0 < \gamma_1 \leq 1$. The larger the parameter γ_1 , the more the Dictator cares about the monetary payoff to other. If $\gamma_1 = 1$, the Dictator cares about the monetary payoff of other as much as he cares about his own monetary payoff. The model of altruism makes the following propositions:

Proposition 4: *An altruistic Dictator will choose to transfer the entire endowment if and only if the degree of altruism (γ_1) exceeds the relative price of giving. However, if γ_1 is lower than the relative price of giving, he will choose to transfer zero.*

An increase in the amount transferred to the Recipient will increase the Recipient's monetary payoff and will decrease the Dictator's monetary payoff. Depending on the altruism parameter γ_1 , an altruistic Dictator will choose either to transfer zero or the entire endowment.

If γ_1 is greater than the relative price of giving, the monetary loss from transferring a positive amount of money to the Recipient will be relatively small to the degree of altruism. Therefore, an altruistic Dictator will always transfer the entire endowment.

If γ_1 is lower than the relative price of giving, the monetary loss from transferring a positive amount of money to the Recipient will be relatively high to the degree of

altruism. Therefore, an altruistic Dictator will always choose to transfer zero. If γ_1 is equal to the relative price of giving, an altruistic Dictator will be indifferent between transferring zero and transferring the entire endowment.

Proposition 5: *An altruistic Dictator will choose to take zero if and only if the degree of altruism γ_1 exceeds the relative price of giving. However, if γ_1 is lower than the relative price of giving, he/she will choose to take £6.*

An increase in the amount taken will decrease the Recipient's monetary payoff and will increase the Dictator's monetary payoff. Depending on the altruism parameter γ_1 , an altruistic Dictator will choose either to take zero or take everything.

If γ_1 is greater than the relative price of giving, the monetary gain from taking money will be relatively small to the degree of altruism. Therefore, an altruistic Dictator will always choose to take zero.

If γ_1 is smaller than the relative price of giving, an altruistic Dictator will always choose to take everything. If γ_1 is equal to the relative price of giving, an altruistic Dictator will be indifferent between taking zero and taking everything.

Proposition 6: *An altruistic Dictator will choose to burn zero*

Burning is costly; hence, an increase in the amount burnt from the Recipient's endowment will decrease the Dictator's payoff and will decrease the Recipient's endowment. Therefore, an altruistic Dictator will always choose to burn zero.

4.3. Spitefulness

The model of spitefulness is based on the assumption that individual $i = 1, 2, \dots, n$ cares positively about his/her own monetary payoff, but dislikes being worse off than the other. If the monetary payoff of other is larger than his/her own monetary payoff, the individual will be willing to decrease other's monetary payoff to reduce the

disadvantageous inequality. Therefore, in the two-individual version, an individual's preferences can be represented by the following utility function:

$$U_1(\pi_1, \pi_2) = \pi_1 - \delta_1 (\pi_2 - \pi_1) \quad (3)$$

where π_1 , indicates the monetary payoff to the Dictator; π_2 indicates the monetary payoff to the Recipient; and δ_1 is a measure of spite. It is assumed that $0 < \delta_1 \leq 1$. The larger δ_1 , the more Dictator dislikes the disadvantageous inequality. If $\delta_1 = 1$, the Dictator cares about the disadvantageous inequality as much as he cares about his/her own monetary payoff. The model of spitefulness predicts that:

Proposition 7: *A spiteful Dictator will choose to transfer zero*

An increase in the amount given to the Recipient will decrease the Dictator's monetary payoff and will increase the disadvantageous inequality. Therefore, a spiteful Dictator will always choose to give zero.

Proposition 8: *A spiteful Dictator will choose to take £6.*

An increase in the amount taken will increase the Dictator's payoffs and will decrease the disadvantageous inequality. Therefore, a spiteful Dictator will always choose to take the maximum amount from the Recipient, which is £6.

Proposition 9: *A spiteful Dictator will choose to burn the entire Recipient's endowment if and only if the degree of spite δ_1 is greater than the relative price of burning. Otherwise, he will choose to burn zero.*

Burning is costly. Thus, an increase in the amount burnt from the Recipient's endowment will decrease the Recipient's payoff and will decrease the Dictator's payoff. Depending on the relative price of burning and the spite parameter δ_1 , a spiteful Dictator will, therefore, choose either to burn zero or to burn everything.

If δ_1 is greater than the relative price of burning or δ_1 is sufficiently high, the monetary loss from burning money to the Recipient will be relatively small to the degree of spite. Therefore, a spiteful Dictator will burn the entire Recipient's endowment. If δ_1 is lower than the relative price of burning, or δ_1 is sufficiently low, the monetary loss from burning money to the Recipient will be relatively high in relation to the degree of spite. Therefore, a spiteful Dictator will burn zero. If δ_1 is equal to the relative price of burning, the Dictator cares negatively about the other as much as he/she cares about his/her own monetary payoff. Therefore, a spiteful Dictator will be indifferent between burning zero and burning the entire Recipient's endowment

4.4. Inequality aversion

The model of inequality aversion (Fehr and Schmidt, 1999) is based on the assumption that individual $i = 1, 2, \dots, n$ cares positively about his/her own payoff and negatively about the difference between his/her own monetary payoff and the monetary payoff of the other individual. Thus, an individual who dislikes inequality may be willing to decrease his own monetary payoff to reach an egalitarian outcome. In the two-individual version, an individual's preferences can be represented by the following utility function:³¹

$$U_1(\pi_1, \pi_2) = \pi_1 - \alpha_1 \max\{\pi_2 - \pi_1, 0\} - \beta_1 \max\{\pi_1 - \pi_2, 0\}, \quad (4)$$

where π_1 , indicates the monetary payoff to the Dictator; π_2 , indicates the monetary payoff to the Recipient. The second term of equation (4) measures the loss of utility from disadvantageous inequality. The larger the parameter α_1 , the more the Dictator dislikes disadvantageous inequality. The third term of the equation (4) measures the loss of utility from advantageous inequality. The larger the parameter β_1 , the more the Dictator dislikes advantageous inequality. In this model, it is assumed that $\alpha_1 \geq \beta_1$. This assumption means that individual dislikes disadvantageous inequality more than advantageous inequality. It is also assumed that $0 \leq \beta_1 < 1$. Individuals do not burn

³¹ Bolton and Ockenfels (2000) developed a similar model of inequality version, but utility is no linear.

their own money to reduce advantage inequality nor do they enjoy advantageous inequality. The model predicts that:

Proposition 10: *An inequity averse Dictator will choose to transfer zero if the value of the Dictator's endowment is lower or equal than the Recipient's endowment. However, if the value of the Dictator's endowment is greater than the Recipient's endowment, an inequity averse Dictator will give either zero or the amount that equals the payoffs. If β_1 is high, an inequity averse Dictator will give the amount that makes payoffs equals. Otherwise, he will give zero.*

If the value of the Dictator's endowment is lower or equal to the Recipient's endowment, there will be a disadvantageous inequality. An increase in the amount given will increase the Recipient's payoff and will decrease the Dictator's payoff. Disadvantageous inequality will increase. An inequity averse Dictator will, therefore, choose to give zero.

Inequality aversion implies that the optimal amount transferred exceeds zero whenever the Dictator's endowment exceeds the Recipient's, and that the optimal amount transferred falls to zero when the two endowments are equal.

If the value of the Dictator's endowment is greater than the Recipient's endowment, there will be advantage inequality. An increase in the amount given will increase the Recipient's payoff and will decrease the Dictator's payoff. Advantageous inequality will decrease. The amount given will depend on the parameter β_1 , an inequity averse Dictator will, therefore, choose either to give zero or to give the amount that equals the monetary payoffs.

If β_1 is sufficiently high, the psychological loss of being in an advantage position will be greater than the monetary loss from transferring money to the Recipient, the Dictator will decrease his/her monetary payoff to achieve a more equal outcome. The Dictator will, therefore, choose to give the amount that equal payoffs. Otherwise, Dictator will give zero.

If β_1 is equal, the Dictator cares about advantageous inequality as much as he cares about his own monetary payoff. Therefore, an altruistic Dictator will be indifferent between giving zero and giving the amount that equals payoffs.

Proposition 11: *An inequity averse Dictator will choose to take everything if the value of the Recipient's endowment is lower or equal than the Dictator's endowment (advantageous inequality). If the value of the Recipient's endowment is greater than the Dictator's endowment (disadvantageous inequality), depending on β_1 , an inequity averse Dictator either will take £6 or will take the amount that makes payoffs equals.*

In the taking domain, if the value of the Recipient's endowment is higher or equal than the Dictator's endowment (disadvantageous inequality), an increase in the amount taken from the Recipient's endowment will decrease the Recipient's payoff and will decrease the disadvantageous inequality. Depending on the parameter β_1 , an inequity averse Dictator will, therefore, choose either to take everything or take the amount that equals payoffs.

If β_1 is sufficiently high, the psychological loss of being in a disadvantage position will be greater than the monetary loss from taken money from the Recipient, the Dictator will increase his monetary payoff to achieve more equal outcome. Therefore, a Dictator will choose to take the amount that equal payoffs. Otherwise, a Dictator will choose to take everything.

If β_1 is equal, the Dictator cares about disadvantageous inequality as much as he cares about his own monetary payoff. Therefore, an inequity averse Dictator will be indifferent between taking everything and taking the amount that equals payoffs.

If the value of the Recipient's endowment is lower than the Dictator's endowment (advantageous inequality), an increase in the amount taken from the Recipient's endowment will decrease the Recipient's payoff and will increase the Dictator's payoff and will decrease the disadvantageous inequality. Depending on the parameter β_1 , an inequity averse Dictator will, therefore, choose either to take everything or take the amount that equals payoffs. If β_1 is sufficiently large, the Dictator will choose to take the

amount that equals payoffs. Otherwise, the Dictator will choose to take the maximum amount, which is £6.

Proposition 12: *A Dictator with inequality aversion will choose to burn zero, if the value of the Dictator's endowment is greater or equal than the value of the Recipient's endowment. If the value of the Dictator's endowment is lower than the value of the Recipient's endowment, depending on β_1 , a Dictator with inequality aversion either will burn £6 or will burn that makes payoffs equals.*

In the burning domain if the value of the Recipient's endowment is higher than the value of the Dictator's endowment (disadvantageous inequality), an increase in the amount burnt from the Recipient's endowment will decrease the Recipient's payoff. Because burning is costly, it will also decrease the Dictator's payoff and will increase disadvantageous inequality. Therefore, the Dictator will burn zero.

If the value of the Recipient's endowment is lower than the value of the Dictator's endowment (advantageous inequality), an increase in the amount burnt from the Recipient's endowment will decrease the Recipient's payoff.

Because burning is costly, it will also decrease the Dictator's payoff and will increase advantageous inequality. Depending on β_1 the Dictator either will burn the maximum amount from the Recipient or will burn the amount that makes payoffs equals. If β_1 is sufficiently large, the Dictator will, therefore, choose to burn the amount that equals payoffs. Otherwise, the Dictator will choose to burn the maximum amount, which is £6.

5. Results

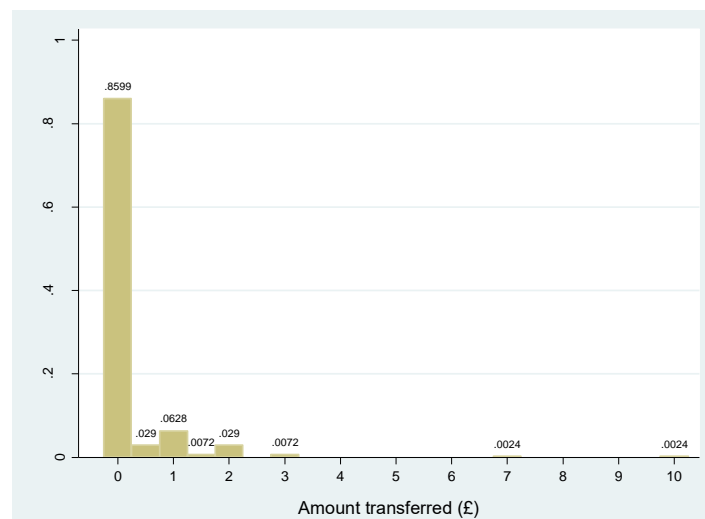
5.1. Description of the data

Figure 6 shows a histogram of the amount transferred to the Recipient. In total, I had 414 independent observations. The distribution was right-skewed. Dictators transferred zero in 88% of the cases, and £1 in 5% of the cases. In 0.2% of the cases, the amount transferred was £10.

On average, Dictators transferred 21 pence (approximately 4% of their endowment) and the corresponding median was zero. The amount of money transferred is significantly greater than zero ($z=5.5574$, $p=0.0000$). The average Dictator's payoff was £6.12, while the Recipient's payoff was £6.32.³²

The difference between the Dictator's payoff and the Recipient's payoff was not statistically significant (Wilcoxon test, $z=-1.279$, $p=0.2009$). The standard deviation was £0.76. The maximum amount transferred was £10.

Figure 6. Histogram of the amount transferred (n=414)



Moving on to the dictator games taking, the behavior of the Dictators looks quite different if we compare the Dictators' behaviour across giving and taking. Figure 7 shows a histogram of the amount taken from the Recipient's endowment. In sum, I had 414 independent observations. The distribution was left-skewed. There is more behaviour variability in the dictator games taking than in the dictator games giving. Dictators took £6 in 60% of the cases and zero in 8% of the cases. In 3% of the cases, Dictators took half of the Recipient's endowment. The percentage of amount left to the Recipient increases above the amount transferred.

³² Note that the average payoffs do not include the £3 participation fee. The data from the experiment can be found in Appendix E of this chapter.

Dictators on average took £4.69 (approximately 78% of the recipient's endowment) and the corresponding median was six. As a result, the average Dictator's payoff was £13.40 whereas the average Recipient's payoff was £1.31. The difference between the Dictator's payoff and the Recipient's payoff was statistically significant (Wilcoxon test, $z=-17.537$, $p=0.0000$). The standard deviation was £2.03. The maximum amount taken was £6 (where dictators took the entire recipient's endowment) and the minimum was zero.

Figure 7. Histogram of the amount taken (n=414)

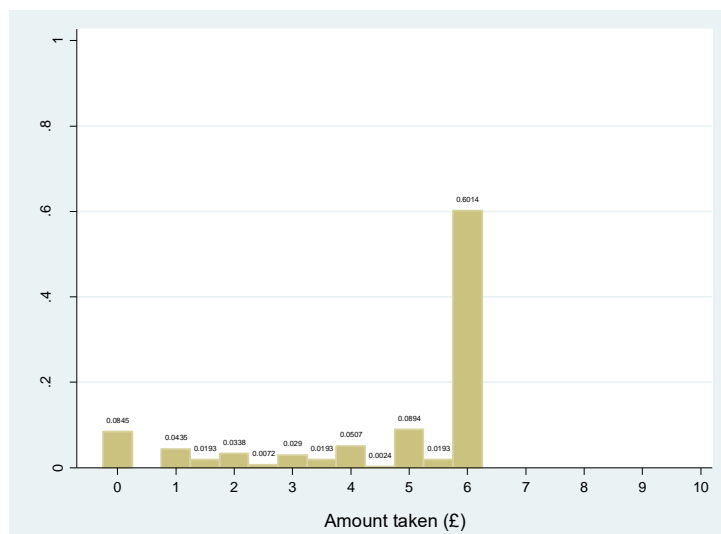
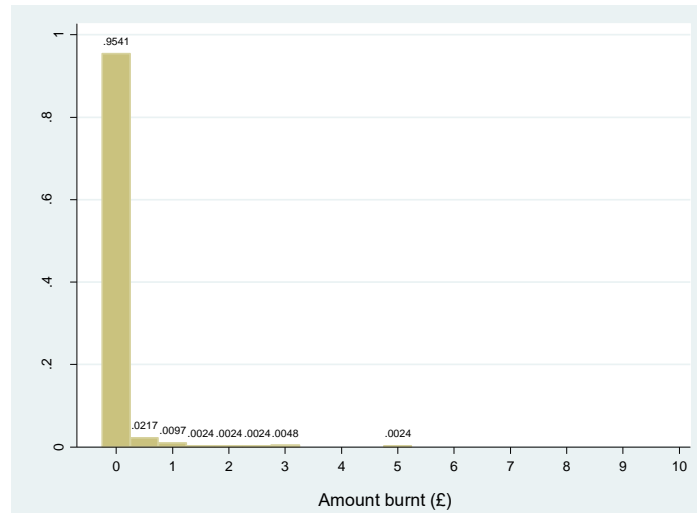


Figure 8 shows a histogram of the amount burnt from the Recipient's endowment. In sum, I had 414 independent observations. Dictators burnt zero in 80% of the cases, and never burnt more than half of the Recipient's endowment. In 0.2% of the cases, Dictators burnt half of the endowment.

Dictators burnt on average 5 pence (0.8% of the recipient's endowment) and the corresponding median was zero. As a result, the average Recipient's payoff was £5.90, while the average Dictator's payoff was £6.27. The difference between the Dictator's payoff and the Recipient's payoff was not statistically significant (Wilcoxon test, $z=-4.922$, $p=0.0000$). The standard deviation was 33 pence and the maximum amount burnt was £5.

Figure 8. Histogram of the amount burnt (n=414)



5.2. Comparison across games

Table 4 shows the average amount transferred to the Recipient disaggregated by game. Dictators transferred on average 5 pence in Game 1, 3 pence in Game 2, 17 pence in Game 3, 11 pence in Game 4, 43 pence in Game 5, and 47 pence in Game 6. A non-parametric test suggests that the difference between average amounts transferred across games was significant (Kruskal-Wallis test, $\chi^2(5)=28.596$, $p=0.0001$). Hence, the average amount transferred changed across games (there is some variation in the amounts transferred to the Recipient).

The amount transferred rises as the Dictator's endowment increases. This evidence suggest that giving is a 'normal good'. The Spearman non-parametric correlation coefficient between the amount transferred and the Dictator's endowment was 0.255 and significant at the 1% level ($p=0.000$). On the other hand, the Recipient's payoff falls as the price of giving increases. The Spearman non-parametric correlation coefficient between the amount transferred and relative price of giving was -0.041 , but not significant ($p=0.410$).

Table 4. Descriptive statistics: amount transferred to the Recipient (£)

Game	Observations	Mean	Median	Std.Dev	Min	Max
1	69	0.05	0.00	0.27	0.00	2.00
2	69	0.03	0.00	0.15	0.00	1.00
3	69	0.17	0.00	0.50	0.00	3.00
4	69	0.11	0.00	0.42	0.00	3.00
5	69	0.43	0.00	1.02	0.00	7.00
6	69	0.47	0.00	1.35	0.00	10.00
Total	414	0.21	0.00	0.76	0.00	10.00

Table 5 shows the average amount taken from the Recipient disaggregated by game. Dictators took on average £4.97 in Game 7, £4.87 in Game 8, £4.49 in Game 9, £4.78 in Game 10, £4.39 in Game 11, and £4.63 in Game 12. A non-parametric test suggests that the difference between average amounts taken across games was not significant (Kruskal-Wallis test, $\chi^2(5)=2.511$, $p=0.7749$). There is a lot of variation in the amounts taken from the Recipient.

Table 5. Descriptive statistics: amount taken from the Recipient (£)

Game	Observations	Mean	Median	Std.Dev	Min	Max
7	69	4.97	6.00	1.58	0.00	6.00
8	69	4.87	6.00	1.83	0.00	6.00
9	69	4.49	6.00	2.25	0.00	6.00
10	69	4.78	6.00	2.03	0.00	6.00
11	69	4.39	6.00	2.29	0.00	6.00
12	69	4.63	6.00	2.13	0.00	6.00
Total	414	4.69	6.00	2.03	0.00	6.00

Table 6 shows the average amount burnt from the Recipient disaggregated by game. Dictators burnt on average 8 pence in Game 13, 3 pence in Game 14, 1 pence in Game 15, 1 pence in Game 16, 14 pence in Game 17, and 9 pence in Game 18. A non-parametric test suggests that the difference between average amounts burnt across games was not significant (Kruskal-Wallis test, $\chi^2(5)=4.418$, $p=0.4909$).

Table 6. Descriptive statistics: amount burnt from the Recipient (£)

Game	Observations	Mean	Median	Std.Dev	Min	Max
13	69	0.08	0.00	0.40	0.00	3.00
14	69	0.03	0.00	0.19	0.00	1.50
15	69	0.01	0.00	0.08	0.00	0.50
16	69	0.01	0.00	0.08	0.00	0.50
17	69	0.14	0.00	0.53	0.00	3.00
18	69	0.09	0.00	0.61	0.00	5.00
Total	414	0.06	0.00	0.38	0.00	5.00

To investigate more formally the proportion of Dictators transferring a positive amount to the Recipient, I plot the distribution of the amount transferred by game in Figure 9. Histogram of the amount transferred by game (n=69 in each game) The proportion of Dictators who transferred a positive amount to the Recipient turned out to be significantly different across games (Fisher's exact test, $p=0.000$). Many Dictators chose to transfer zero.

Figure 9. Histogram of the amount transferred by game (n=69 in each game)

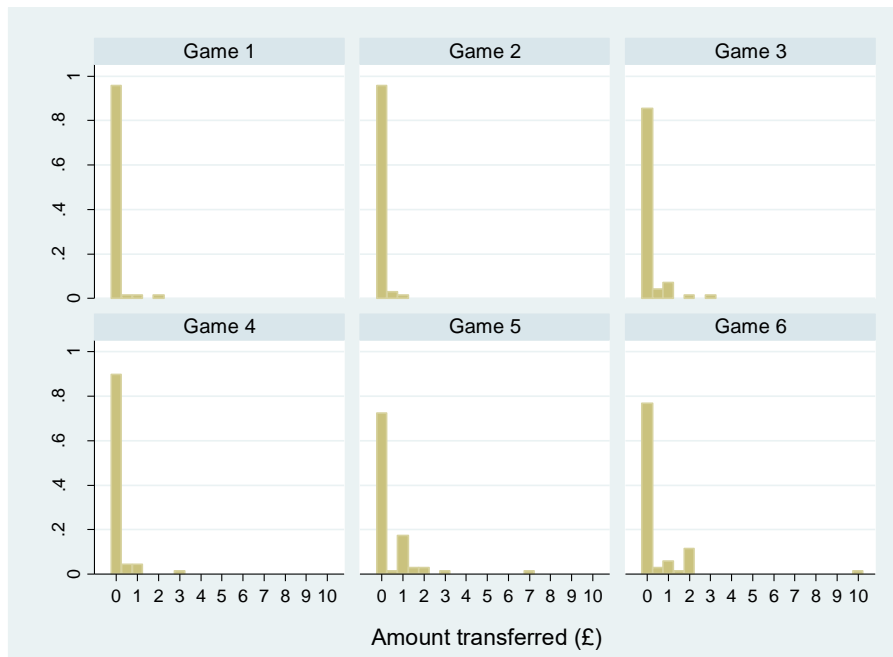


Figure 10 depicts graphically the distribution of the amount taken for each game. The proportion of Dictators taking £6 across games turned out to be significantly different across games (Pearson $\chi^2(55) = 105.8825$, $p = 0.000$). The Spearman non-parametric correlation coefficient between the amount taken and the Dictator's endowment was -0.049 and not significant ($p=0.316$). Likewise, the Spearman non-parametric correlation coefficient between the amount taken and relative price of taking was -0.059 and not significant ($p=0.232$). Many Dictators chose to take the maximum amount of £6.

Figure 10. Histogram of the amount taken by game (n=69 in each game)

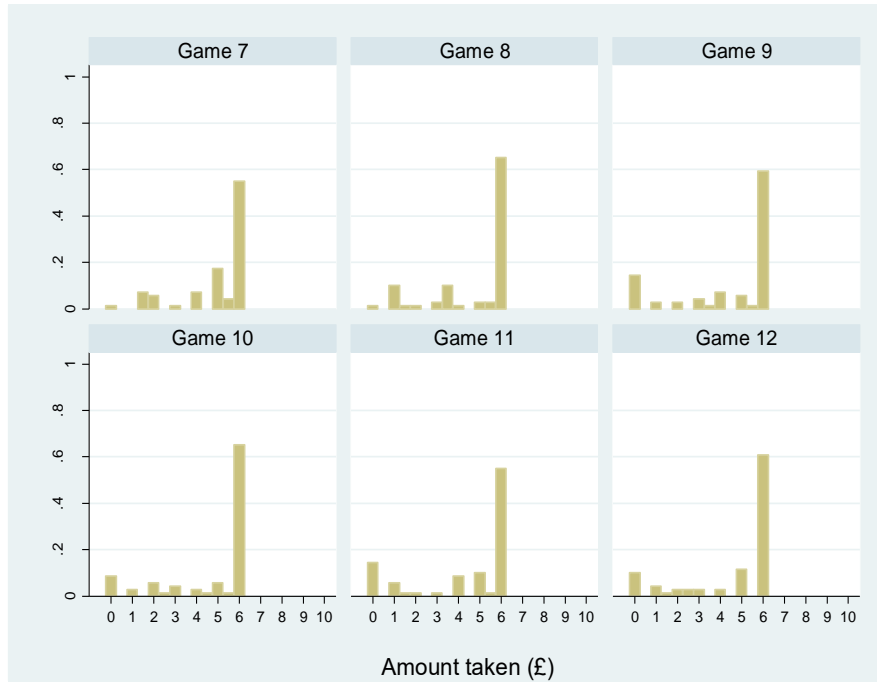
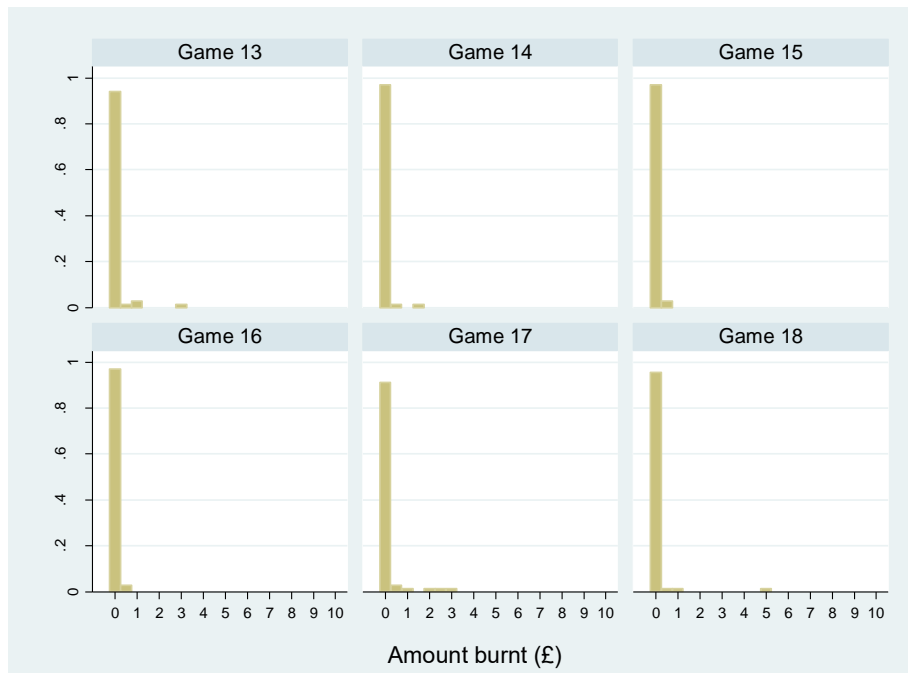


Figure 11 depicts graphically the distribution of the amount burnt for each game. The differences in the proportion of Dictators burning £0 across games were not statistically significant (Fisher's exact test, $p=0.906$). It is difficult to depict a clear-cut pattern between amount burnt and the Dictator's initial endowment. The Spearman non-parametric correlation coefficient between amount burnt and the Dictator's endowment was 0.043 and not significant ($p=0.381$). Likewise, the Spearman non-parametric correlation coefficient between amount burnt and relative price of burning was -0.058 and not significant ($p=0.237$). Many Dictators chose to burn zero. There is very little variation in the amounts burnt from the Recipient's endowment.

Figure 11. Histogram of the amount burnt by game (n=69 in each game)



5.3. Regression analysis

To examine the relative price effect and the effect of the size of the Dictator's endowment, I run a series of OLS regressions. Table 7 reports the results from an ordinary least squares (OLS) regression.³³ The dependent variable was the amount transferred to the Recipient (in British pounds) over all Dictator giving, and the independent variables were the relative price of giving and the Dictator's endowment.

The coefficient of the relative price of giving was negative and not statistically significant, after controlling for the Dictator's endowment. The coefficient of the Dictator's endowment was positive and statistically significant, even after controlling for the relative price of giving. Therefore, an increase in the Dictator's endowment by one unit, *ceteris paribus*, increased the amount transferred by 0.06 units. Hence, giving is a 'normal good'. These findings are in line with the results from the non-parametric test.

³³ I ran a Tobit model with clustering at the subject level, with the amount transferred as the dependent variable. I found that the significance of the coefficients does not change in the Tobit model.

Table 7. OLS regression on the amount transferred

	Coefficient	Standard error	p-value
Relative price of giving	-0.024	0.147	0.870
Dictator's endowment	0.060	0.013	0.000
Constant	-0.150	0.142	0.290
Observations	414		
Prob>F	0.000		
Adj R-square	0.045		

Notes: *p < 0.1, **p < 0.05, and *** p < 0.01.

Table 8 reports the results from an ordinary least squares (OLS) regression. The dependent variable was the amount taken from the Recipient's endowment (in British pounds) over all Dictator taking, while the independent variables were the relative price of taking and the Dictator's endowment. The coefficient of the relative price of taking was negative and not significant. Similarly, the coefficient of the Dictator's endowment was negative and not significant. The results suggest that an increase in the monetary stakes does not automatically lead to other-regarding behaviour. Rather, it depends on the experimental context.

Table 8. OLS regression on the amount taken

	Coefficient	Standard error	p-value
Relative price of taking	-0.280	0.399	0.483
Dictator's endowment	-0.057	0.035	0.102
Constant	5.259	0.384	0.000
Observations	414		
Prob>F	0.205		
Adj R-square	0.003		

Notes: *p < 0.1, **p < 0.05, and *** p < 0.01.

Table 9 reports the results from an ordinary least squares (OLS) regression. The dependent variable was the amount burnt from the Recipient's endowment (in British pounds) over all Dictator burning, and the independent variables were the relative price of burning and the Dictator's endowment. The coefficient of the relative price of burning was negative and not statistically significant. The coefficient of the Dictator's endowment was positive and not statistically significant.

Table 9. OLS regression on the amount burnt

	Coefficient	Standard error	p-value
Relative price of burning	-0.063	0.075	0.403
Dictator's endowment	0.010	0.007	0.139
Constant	0.047	0.072	0.513
Observations	621		
Prob>F	0.0472		
Adj R-square	0.0066		

Notes: *p < 0.1, **p < 0.05, and *** p < 0.01.

5.4. Individual preferences

Now I turn to the individual level data and examine stability of behaviour across decision contexts. Figure 12, Figure 13, and Figure 14 show bubble plots for each game in the giving domain, taking and burning respectively. The X-axis shows the Dictator's payoff and the Y-axis shows the Recipient's payoff. The size of the bubbles corresponds to the number of Dictators making each possible choice about the payoff to self and the payoff to the Recipient for each game. The bigger the bubble, the higher the number of Dictators on the corresponding choice. These payoffs do not include the participation's fee.

The plots reveal some heterogeneity between subjects in Dictator taking games and Dictator giving games. I found that 47 out of 69 Dictators (approximately 68%) made stable choices across Dictator giving. Dictators consistently chose to transfer the same

amount of money: 46 Dictators always chose to transfer zero and only one Dictator always chose to transfer £0.5. For instance, Subject 12 always transfers zero in all the Dictator games giving. Subject 40 always transfers £0.5 in all the Dictator games giving.

In the Dictator taking, I found 33 out of 69 Dictators (approximately 48%) made stable choices across Dictator taking games: 30 Dictators always chose to take the maximum amount of money (£6), one Dictator always chose to take zero, one Dictator always chose to take £4 and one Dictator always chose to take £5. These Dictators made stable choices. I also found that 12 Dictators never took the maximum amount of £6.

In the Dictator burning, 61 out of 69 Dictators (approximately 88%) made stable choices across Dictator burning games: 60 Dictators always chose to burn zero and one Dictator always chose to burn £0.5.

Figure 12. Bubble plots for each Dictator game giving (n=69 in each game)

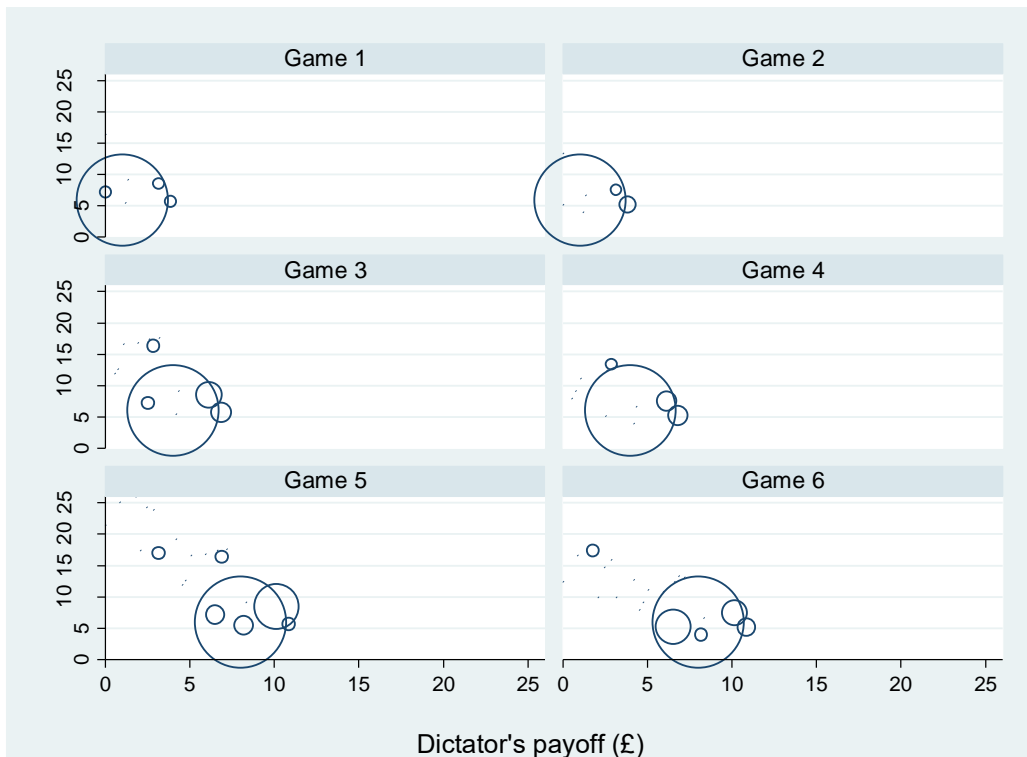


Figure 13. Bubble plots for each Dictator game taking (n=69 in each game)

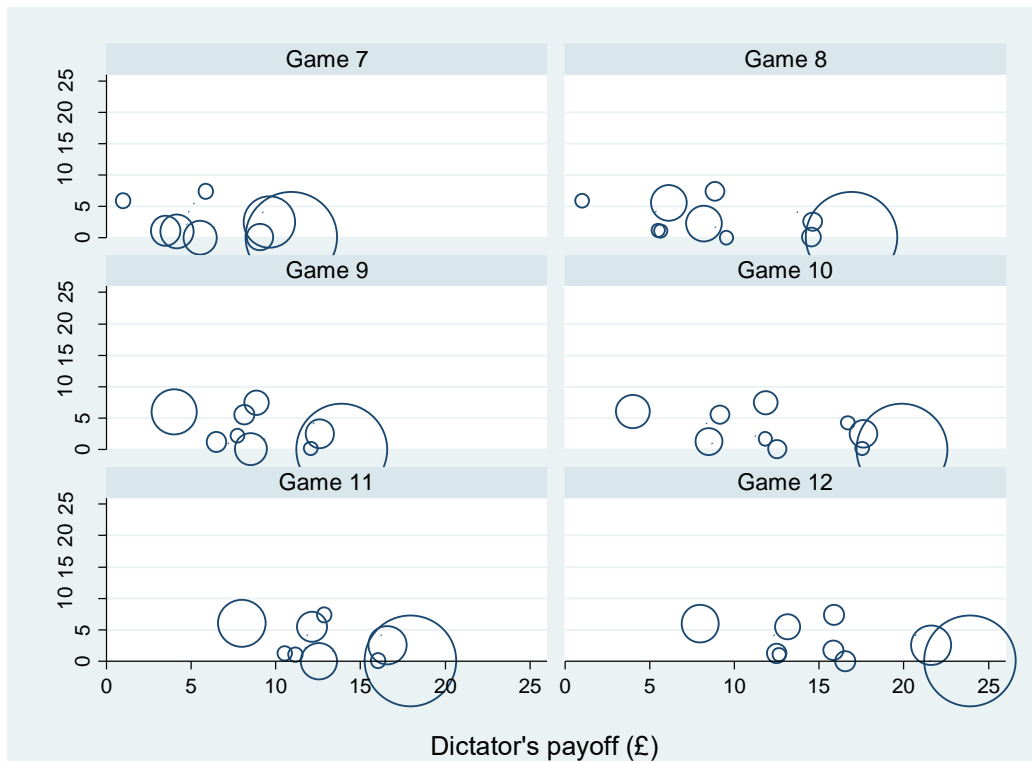
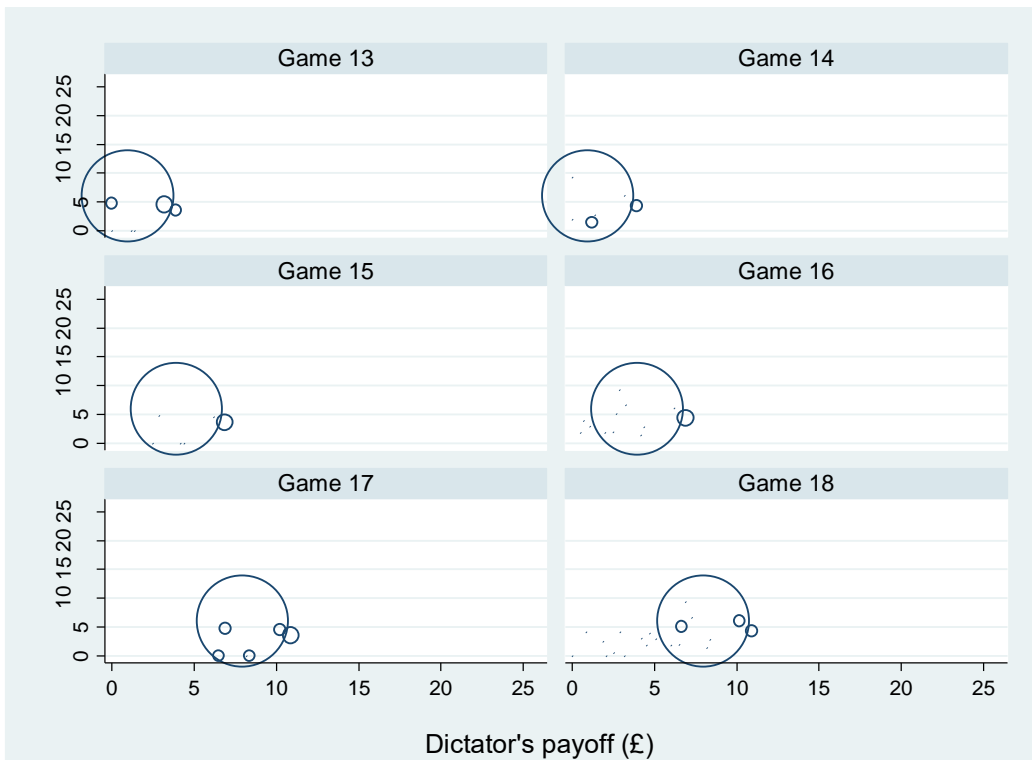


Figure 14. Bubble plots for each Dictator game burning (n=69 in each game)



Taken all games together, 24 out of 69 Dictators (approximately 35%) transferred zero, took £6 and burnt zero. For instance, Subject 1 is a clear example of a Dictator making consistent choices. He/she always transfers zero in all the Dictator giving; takes the maximum amount £6 in all the Dictator taking; and burnt zero in all the Dictator burning. Therefore, Player 1's behaviour is consistent with self-interest. In addition, one Dictator always transferred zero, took £5, and burnt zero.

I also found that Dictators who transferred money in Dictator giving also burnt money in the Dictator burning games. For instance, Subject 40 always gives £0.5 in the Dictator giving; takes £5.5 in the Dictator taking; and burn £0.5 in some of the Dictator burning. Therefore, the behaviour of subject 40 is consistent with pro-social and anti-social behaviour. This is consistent with the results in Sadrieh and Schröder (2016), who found a large fraction of the subjects who exhibited pro-social and anti-social behaviour.

6. Discussion and conclusion

The main aim of this study was to explore the interplay of the experimental context, the relative price, and the size of the Dictator's endowment. I used a within-subject design, where Dictators make allocation decisions in three separate contexts. Dictators can transfer any part of the endowment to the Recipient, take any amount of the Recipient's endowment, and burn any amount from the Recipient's endowment. In each context, Dictators played in nine different scenarios. In total, Dictators played in eighteen scenarios. The eighteen scenarios were given to each dictator in random order.

Based on the results reported from this experiment, it seems fair to say that some Dictators are willing to transfer money to the Recipient. This is consistent with the results from previous Dictator games. Many Dictators are also willing to take money from the Recipient's endowment. However, Dictators are not willing to burn money from the Recipient's endowment.

Before interpreting the results, several features of the experimental design need to be considered. First, Recipient's has an initial endowment. Providing an initial endowment

to the recipient might discourage altruistic behaviour (Engel, 2011; Korenok et al 2013). Second, the size of the Dictator's endowment varied, and consequently the Dictator's endowment was higher, equal, or lower than the Recipient's endowment, thus poor Dictators were expected to transfer zero. The difference between Dictator's endowment and Recipient's endowment also implied that transferring zero could lead to equal payoffs.

Third, the Dictator was informed about the Recipient's wealth before making his/her decision. This could affect giving, which was consistent with the 'poverty effect' (Brañas-Garza, 2006). Brañas-Garza (2006) found that when information about the Recipient's wealth is provided, the amount transferred was significantly higher than when the Dictator does not have information about the Recipient's wealth. He, therefore, suggested that Dictators become more generous under the 'poverty effect'.

The results from the current experiment showed that many Dictators chose to transfer zero to the Recipient. Previous studies on Dictator games (Andreoni and Miller, 2002; Fisman et al 2007; Cappelen et al., 2007; Jakiela, 2013) have dealt with zero observations by using two-limit Tobit regression models.³⁴ Moffatt (2016) assumed a Stone-Geary utility function over own payoff and other payoff and use data from Andreoni and Miller (2002) to estimate the parameters of a utility function that incorporates zero observations as corner solutions in the Dictator's constrained utility maximisation problem. These models assumed heterogeneity between subjects.

The asymmetric effect of the monetary stake serves as a warning against generalizations of conclusions derived from Dictator games giving. Behaviour changed when the context changed from dividing the Dictator's endowment (giving) to dividing the Recipient's endowment (taking). This finding is in line with previous studies that reported the change of preferences across different situations (List, 2007; Bardsley, 2008). One might ask whether and to what extent decision in one game correlates with the decision in another game. Can we use individuals' decisions to predict behaviour in different settings? Future research could try to answer these questions.

³⁴ Breitmoser (2013) showed how these 'structural models' account for noise in the choice process.

References

Andreoni, J., 1990. Impure altruism and donations to public goods: A theory of warm-glow giving. *The economic journal*, 100(401), pp.464-477.

Andreoni, J., 1995. Warm-glow versus cold-prickle: the effects of positive and negative framing on cooperation in experiments. *The Quarterly Journal of Economics*, 110(1), pp.1-21.

Andreoni, J. and Miller, J.H., 1998. Giving According to GARP: An Experiment on Rationality and Altruism. *Social Systems Research Institute Working Paper*, 9902.

Andreoni, J. and Miller, J., 2002. Giving according to GARP: An experimental test of the consistency of preferences for altruism. *Econometrica*, 70(2), pp.737-753.

Andreoni, J. and Vesterlund, L., 2001. Which is the fair sex? Gender differences in altruism. *The Quarterly Journal of Economics*, 116(1), pp.293-312.

Andreoni, J., Harbaugh, W.T. and Vesterlund, L., 2010. Altruism in experiments. In *Behavioural and Experimental Economics* (pp. 6-13). Palgrave Macmillan UK.

Abbink, K. and Herrmann, B., 2011. The moral costs of nastiness. *Economic Inquiry*, 49(2), pp.631-633.

Abbink, K. and Sadrieh, A., 2009. The pleasure of being nasty. *Economics Letters*, 105(3), pp.306-308.

Abbink, K., Masclet, D. and van Veelen, M., 2011. Reference point effects in antisocial preferences.

Andreoni, J., 1989. Giving with impure altruism: applications to charity and Ricardian equivalence. *Journal of Political Economic*, 97 (6), 1447-1458.

Andreoni, J., 1989. Giving with impure altruism: Applications to charity and Ricardian equivalence. *Journal of political Economy*, 97(6), pp.1447-1458.

Andreoni, J., 2006. Philanthropy. *Handbook of the economics of giving, altruism and reciprocity*, 2, pp.1201-1269.

Andreoni, J., 1993. An experimental test of the public-goods crowding-out hypothesis. *The American Economic Review*, pp.1317-1327.

Andreoni, J., 1990. Impure altruism and donations to public goods: A theory of warm-glow giving. *The economic journal*, 100(401), pp.464-477.

Armantier, O., 2006. Do wealth differences affect fairness considerations?. *International Economic Review*, 47(2), pp.391-429.

Bardsley, N., 2008. Dictator game giving: altruism or artefact?. *Experimental Economics*, 11(2), pp.122-133.

Becker, G.S., 1974. A theory of social interactions. *Journal of political economy*, 82(6), pp.1063-1093.

Blanco, M., Engelmann, D. and Normann, H.T., 2011. A within-subject analysis of other-regarding preferences. *Games and Economic Behavior*, 72(2), pp.321-338.

Bolton, G.E., Katok, E. and Zwick, R. 1998. Dictator game giving: rules of fairness versus acts of kindness. *International Journal of Game Theory* 27, 269-99.

Bolton, G.E. and Ockenfels, A., 2000. ERC: A theory of equity, reciprocity, and competition. *American economic review*, pp.166-193.

Bowles, S. and Polania-Reyes, S., 2012. Economic incentives and social preferences: substitutes or complements?. *Journal of Economic Literature*, 50(2), pp.368-425.

Brandts, J., Fatas, E., Haruvy, E. and Lagos, F., 2015. The impact of relative position and returns on sacrifice and reciprocity: an experimental study using individual decisions. *Social Choice and Welfare*, 45(3), pp.489-511.

Brañas-Garza, P., 2006. Poverty in dictator games: Awakening solidarity. *Journal of Economic Behavior & Organization*, 60(3), pp.306-320.

Brañas-Garza, P., 2007. Promoting helping behavior with framing in dictator games. *Journal of Economic Psychology*, 28(4), pp.477-486.

Breitmoser, Y., 2013. Estimation of social preferences in generalized dictator games. *Economics Letters*, 121(2), pp.192-197.

Broberg, T., Ellingsen, T. and Johannesson, M., 2007. Is generosity involuntary?. *Economics Letters*, 94(1), pp.32-37.

Brosig, J., Riechmann, T. and Weimann, J., 2007. Selfish in the end?: An investigation of consistency and stability of individual behavior.

Camerer, C.F., 2011. *Behavioral game theory: Experiments in strategic interaction*. Princeton University Press.

Camerer, C. and Thaler, R.H., 1995. Anomalies: Ultimatums, dictators and manners. *The Journal of Economic Perspectives*, 9(2), pp.209-219.

Cappelen, A.W., Nielsen, U.H., Sørensen, E.Ø., Tungodden, B. and Tyran, J.R., 2013. Give and take in dictator games. *Economics Letters*, 118(2), pp.280-283.

Carpenter, J., Verhoogen, E. and Burks, S., 2005. The effect of stakes in distribution experiments. *Economics Letters*, 86(3), pp.393-398.

Cooper, D.J. and Kagel, J.H., 2016. Other-regarding preferences. *The Handbook of Experimental Economics, Volume 2: The Handbook of Experimental Economics*, p.217.

Charness, G. and Rabin, M., 2002. Understanding social preferences with simple tests. *The Quarterly Journal of Economics*, 117(3), pp.817-869.

Cherry, T.L., Frykblom, P. and Shogren, J.F., 2002. Hardnose the dictator. *The American Economic Review*, 92(4), pp.1218-1221.

Chlaß, N. and Moffatt, P.G., 2012. *Giving in dictator games: Experimenter demand effect or preference over the rules of the game?* (No. 2012, 044). Jena Economic Research Papers.

Cookson, R., 2000. Framing effects in public goods experiments. *Experimental Economics*, 3(1), pp.55-79.

Crumpler, H. and Grossman, P.J., 2008. An experimental test of warm glow giving. *Journal of public Economics*, 92(5), pp.1011-1021.

Dana, J., Cain, D.M. and Dawes, R.M., 2006. What you don't know won't hurt me: Costly (but quiet) exit in dictator games. *Organizational Behavior and human decision Processes*, 100(2), pp.193-201.

Dana, J., Weber, R.A. and Kuang, J.X., 2007. Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness. *Economic Theory*, 33(1), pp.67-80.

Dreber, A., Ellingsen, T., Johannesson, M. and Rand, D.G., 2013. Do people care about social context? Framing effects in dictator games. *Experimental Economics*, 16(3), pp.349-371.

Eckel, C.C. and Grossman, P.J., 1996. The relative price of fairness: Gender differences in a punishment game. *Journal of Economic Behavior & Organization*, 30(2), pp.143-158.

Eichenberger, R. and Oberholzer-Gee, F., 1998. Rational moralists: The role of fairness in democratic economic politics. *Public Choice*, 94(1-2), pp.191-210.

Engel, C., 2011. Dictator games: A meta study. *Experimental Economics*, 14(4), pp.583-610.

Fehr, E. and Schmidt, K.M., 1999. A theory of fairness, competition, and cooperation. *The quarterly journal of economics*, 114(3), pp.817-868.

Fehr, E. and Schmidt, K.M., 2006. The economics of fairness, reciprocity and altruism—experimental evidence and new theories. *Handbook of the economics of giving, altruism and reciprocity*, 1, pp.615-691.

Fischbacher, U., 2007. z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental economics*, 10(2), pp.171-178.

Fisman, R., Jakiela, P., Kariv, S. and Markovits, D., 2015. The distributional preferences of an elite. *Science*, 349(6254), p.aab0096.

Fisman, R., Jakiela, P. and Kariv, S., 2017. Distributional preferences and political behavior. *Journal of Public Economics*, 155, pp.1-10.

Forsythe, R., Horowitz, J.L., Savin, N.E. and Sefton, M., 1994. Fairness in simple bargaining experiments. *Games and Economic behavior*, 6(3), pp.347-369.

Franzen, A. and Pointner, S., 2012. Anonymity in the dictator game revisited. *Journal of Economic Behavior & Organization*, 81(1), pp.74-81.

Gneezy, A., Imas, A., Brown, A., Nelson, L.D. and Norton, M.I., 2012. Paying to be nice: Consistency and costly prosocial behavior. *Management Science*, 58(1), pp.179-187.

Greiner, B., 2015. Subject pool recruitment procedures: organizing experiments with ORSEE. *Journal of the Economic Science Association*, 1(1), pp.114-125.

Guala, F. and Mittone, L., 2010. Paradigmatic experiments: the dictator game. *The Journal of Socio-Economics*, 39(5), pp.578-584.

Harrison, G.W. and Johnson, L.T., 2006. Identifying altruism in the laboratory. In *Experiments Investigating Fundraising and Charitable Contributors* (pp. 177-223). Emerald Group Publishing Limited.

Heinrich, T. and Weimann, J., 2013. A note on reciprocity and modified dictator games. *Economics Letters*, 121(2), pp.202-205.

Hoffman, E., McCabe, K. and Smith, V.L., 1996. Social distance and other-regarding behavior in dictator games. *The American Economic Review*, 86(3), pp.653-660.

Hoffman, M., 2011. Does higher income make you more altruistic? evidence from the holocaust. *Review of Economics and Statistics*, 93(3), pp.876-887.

Iriberry, N. and Rey-Biel, P., 2011. The role of role uncertainty in modified dictator games. *Experimental Economics*, 14(2), pp.160-180.

Iriberry, N. and Rey-Biel, P., 2013. Elicited beliefs and social information in modified dictator games: What do dictators believe other dictators do?. *Quantitative Economics*, 4(3), pp.515-547.

Jakiela, P., 2013. Equity vs. efficiency vs. self-interest: on the use of dictator games to measure distributional preferences. *Experimental Economics*, 16(2), pp.208-221.

Kahneman, D., Knetsch, J.L. and Thaler, R.H., 1986. Fairness and the assumptions of economics. *Journal of business*, pp.S285-S300.

Karakostas, A. and Zizzo, D.J., 2016. Compliance and the power of authority. *Journal of Economic Behavior & Organization*, 124, pp.67-80.

Konow, J., 2010. Mixed feelings: Theories of and evidence on giving. *Journal of Public Economics*, 94(3), pp.279-297.

Konow, J., 1996. A positive theory of economic fairness. *Journal of Economic Behavior & Organization*, 31(1), pp.13-35.

Korenok, O., Millner, E.L. and Razzolini, L., 2012. Are dictators averse to inequality?. *Journal of Economic Behavior & Organization*, 82(2), pp.543-547.

Korenok, O., Millner, E.L. and Razzolini, L., 2013. Impure altruism in dictators' giving. *Journal of Public Economics*, 97, pp.1-8.

Korenok, O., Millner, E.L. and Razzolini, L., 2014. Taking, giving, and impure altruism in dictator games. *Experimental Economics*, 17(3), pp.488-500.

Krupka, E.L. and Weber, R.A., 2013. Identifying social norms using coordination games: Why does dictator game sharing vary?. *Journal of the European Economic Association*, 11(3), pp.495-524.

Lazear, E.P., Malmendier, U. and Weber, R.A., 2012. Sorting in experiments with application to social preferences. *American Economic Journal: Applied Economics*, 4(1), pp.136-163.

Levine, D.K., 1998. Modeling altruism and spitefulness in experiments. *Review of economic dynamics*, 1(3), pp.593-622.

List, J.A., 2007. On the interpretation of giving in dictator games. *Journal of Political economy*, 115(3), pp.482-493.

List, J.A. and Cherry, T.L., 2008. Examining the role of fairness in high stakes allocation decisions. *Journal of Economic Behavior & Organization*, 65(1), pp.1-8.

Luccasen, A. and Grossman, P.J., 2013. *A robust test of warm glow giving and spiteful pleasure in a real donation experiment with and without earned endowments*. Monash Economics Working Papers 29-13, Monash University, Department of Economics.

Moffatt, P.G., 2015. *Experimetrics: Econometrics for experimental economics*. Palgrave Macmillan.

Nagel, T., 1978. *The possibility of altruism*. Princeton University Press.

Neilson, W.S., 2009. A theory of kindness, reluctance, and shame for social preferences. *Games and Economic Behavior*, 66(1), pp.394-403.

Nelson, W.R., 2001. Incorporating fairness into game theory and economics: Comment. *American Economic Review*, 91(4), pp.1180-1183.

Nickerson, M., Kariv, S. and Markovits, D., 2007. Individual preferences for giving. *The American Economic Review*, 97(5), pp.1858-1876.

Oberholzer-Gee, F. and Eichenberger, R., 2008. Fairness in extended dictator game experiments. *The BE Journal of Economic Analysis & Policy*, 8(1).

Oechssler, J., 2010. Searching beyond the lamppost: Let's focus on economically relevant questions. *Journal of economic behavior & organization*, 73(1), pp.65-67.

Rabin, M., 1993. Incorporating fairness into game theory and economics. *The American economic review*, pp.1281-1302.

Raihani, N.J., Mace, R. and Lamba, S., 2013. The effect of \$1, \$5 and \$10 stakes in an online dictator game. *PloS one*, 8(8), p.e73131.

Sadrieh, A. and Schröder, M., 2016. Materialistic, pro-social, anti-social, or mixed—A within-subject examination of self-and other-regarding preferences. *Journal of Behavioral and Experimental Economics*, 63, pp.114-124.

Sadrieh, A. and Schröder, M., 2017. Acts of helping and harming. *Economics Letters*, 153, pp.77-79.

Schotter, A., Weiss, A. and Zapater, I., 1996. Fairness and survival in ultimatum and dictatorship games. *Journal of Economic Behavior & Organization*, 31(1), pp.37-56.

Suvoy, R., 2003. *The effects of give and take framing in a dictator game* (Doctoral dissertation, University of Oregon).

Visser, M.S. and Roelofs, M.R., 2011. Heterogeneous preferences for altruism: gender and personality, social status, giving and taking. *Experimental Economics*, 14(4), pp.490-506.

Zhang, L. and Ortmann, A., 2014. The effects of the take-option in dictator-game experiments: a comment on Engel's (2011) meta-study. *Experimental Economics*, 17(3), pp.414-420.

Zhang, L. and Ortmann, A., 2016. Pro-social or anti-social, or both? A within-and between-subjects study of social preferences. *Journal of Behavioral and Experimental Economics*, 62, pp.23-32.

Zizzo, D.J., 2010. Experimenter demand effects in economic experiments. *Experimental Economics*, 13(1), pp.75-98.

Zizzo, D.J. and Oswald, A.J., 2001. Are people willing to pay to reduce others' incomes?. *Annales d'Economie et de Statistique*, pp.39-65.

Zizzo, D.J., 2004. Inequality and procedural fairness in a money-burning and stealing experiment. In *Inequality, Welfare and Income Distribution: Experimental Approaches* (pp. 215-247). Emerald Group Publishing Limited.

Appendix to Chapter 2: The Effects of the Relative Price and Monetary Stakes on the Dictator's Behaviour in Giving, Taking and Burning

- A. Experimental instructions
- B. Background of the participants
- C. Post-experimental questionnaire
- D. Proofs of propositions
- E. Additional analysis
- F. Data

Appendix A. Experimental instructions

Note: Instructions specific to Player 1 and to Player 2 were shown separately to Player 1 and Player 2, respectively, on their computer screens.

Thank you for participating in this experiment. In this experiment, you can earn money. What you earn will depend upon your decision, and on the decision of another participant in the room. No data that you provide can be associated with your person; all data will be treated confidentially. Please follow the instructions carefully. These instructions explain how the experiment works. If any of the instructions are unclear, or if you have any questions, please raise your hand and I will come to you. Please do not communicate with any other participant until the experiment is over.

In this experiment, half of the participants in the room will be randomly assigned to the role of Player 1 and the other half of participants will be assigned to the role of Player 2. You will hold this role throughout the experiment. Note that Player 1 will not learn who Player 2 is, neither during nor after the session. Likewise, Player 2 will not learn who Player 1 is. The experiment is expected to last no more than an hour.

Instructions for Player 1:

You will be Player 1 and another participant will be Player 2. You will be asked to make a series of decisions in 18 scenarios shown in random order.

In each scenario, you will have to decide whether to increase Player 2's earnings by decreasing your earnings, or whether to decrease Player 2's earnings either by increasing or decreasing your earnings. If you decide neither to increase nor to decrease Player 2's earnings, your earnings and Player 2's earnings will remain unchanged. Player 2 will not make any decisions.

Your payment will depend on your decisions alone. You will have 60 seconds to make a decision in each scenario. After you make your decisions in the 18 scenarios, you will be randomly matched with one of the participants in the Player 2 role.

The computer will randomly choose one of your 18 decisions. You will be paid what you chose as your earnings and Player 2 will be paid what you chose as Player 2's earnings in the random selected scenario. At the end of the experiment, you will be paid your money earnings plus £2 for showing up. You will not interact with Player 2 again in today's experiment.

Instructions for Player 2:

You will be Player 2 and another participant will be Player 1. You will not make any decisions. Player 1 will be asked to make a series of decisions in 18 scenarios shown in random order.

In each scenario Player 1 will have to decide whether to increase your earnings by decreasing his/her earnings, or whether to decrease your earnings either by increasing or decreasing his/her earnings. If Player 1 decides neither to increase nor to decrease your earnings, your earnings and Player 1's earnings will remain unchanged.

After each Player 1 makes his/her decisions in the 18 scenarios, you will be randomly matched with one of the participants in the Player 1 role

The computer will randomly choose one of the Player 1's decisions. You will be paid what Player 1 chose as your earnings and Player 1 will be paid what he/she chose as

his/her earnings in the random selected scenario. At the end of the experiment, you will be paid your money earnings plus £2 for showing up.

Before the experiment begins:

You will have to correctly answer 8 questions. These questions check your understanding of the experimental instructions.

Appendix B. Background of the participants

Table 10. Percentage of male and female subjects by session

Session	Female		Male	
	Frequency	Percent	Frequency	Percent
1	13	72.22	5	27.78
2	15	75.00	5	25.00
3	13	65.00	7	35.00
4	12	60.00	8	40.00
5	9	45.00	11	55.00
6	14	70.00	6	30.00
7	9	45.00	11	55.00
Total	85	61.59	53	38.41

Table 11. Descriptive statistics for age by session

Session	Number of subjects	Age (years)				
		Mean	Median	S.D	Min	Max
1	18	21.67	21.50	2.30	18	27
2	20	22.25	21.50	2.75	19	29
3	20	21.35	21.00	1.84	19	25
4	20	25.80	22.00	10.76	19	66
5	20	23.95	23.00	5.62	19	41
6	20	25.60	22.00	5.92	20	38
7	20	23.15	22.00	4.83	19	39
Total	138	23.42	22.00	5.79	18	66

Table 12. Percentage of subjects by field of study

Field of study	Frequency	Percent
Accounting and Finance	19	13.77
Biological science	1	0.72
Biology	4	2.90
Biomedicine	6	4.35
Branding	2	1.45
Business and Management	7	5.07
Chemistry	5	3.62
Computer Science	3	2.17
Computing Science	1	0.72
Creative Writing	2	1.45
Drama	1	0.72
Ecology	2	1.45
Economics	15	10.87
Education	1	0.72
Environmental Sciences	18	13.04
History	3	2.17
International Development	7	5.07
International Relations and Politics	2	1.45
Law	12	8.70
Linguistics & Philosophy	1	0.72
Mathematics	4	2.90
Media, Society and Culture	5	3.62
Medicine	1	0.72
Natural Sciences	1	0.72
Occupational Therapy	1	0.72
Pharmacy	7	5.07
Psychology	3	2.17
Public Policy	2	1.45
Speech and Language Therapy	2	1.45
Total	138	100.00

Table 13. Percentage of subjects by country

Country	Frequency	Percent
Azerbaijan	1	0.72
Bangladesh	1	0.72
Brazil	1	0.72
Bulgaria	6	4.35
China	22	15.94
Germany	1	0.72
Hong Kong	14	10.14
Hungary	1	0.72
India	2	1.45
Indonesia	2	1.45
Japan	3	2.17
Kuwait	1	0.72
Latvia	1	0.72
Lithuania	1	0.72
Malawi	1	0.72
Malaysia	4	2.90
Nigeria	6	4.35
Philippines	2	1.45
Poland	2	1.45
ROC	1	0.72
ROU	1	0.72
Russia	2	1.45
Slovakia	1	0.72
South Africa	1	0.72
South Korea	1	0.72
Sri Lanka	1	0.72
Thailand	2	1.45
UK	40	28.99
USA	3	2.17
Ukraine	2	1.45
Venezuela	1	0.72
Vietnam	10	7.25
Total	138	100.00

Table 14. Descriptive statistics for experimental earnings by session

Session	Number of subjects	Experimental earnings (£)				
		Mean	Median	S.D	Min	Max
1	18	6.33	6.00	5.17	0.00	22.00
2	20	6.85	6.00	3.99	0.00	20.00
3	20	6.55	6.00	3.32	0.00	14.00
4	20	6.65	6.00	3.80	0.00	16.00
5	20	6.38	6.00	3.94	0.00	18.00
6	20	7.30	6.00	6.65	0.00	22.00
7	20	6.08	6.00	4.22	0.00	18.00
Total	138	6.59	6.00	4.47	0.00	22.00

Note: Experimental earnings do not include the participation fee of £3

Appendix C. Post-experimental questionnaire (all treatments)

Figure 15. Post-experimental questionnaire's screens

Please answer the following questions. Your answers will stay anonymous.

What is your age?

What is your gender?
 Male
 Female

What is your field of study?

What is your native language?
 English
 Another language

What country were you born in?

Please provide feedback about the experiment here:

Appendix D. Additional analysis

In this section, I examined the effect of the choice set on the proportion of selfish choices in Dictator games giving and Dictator games taking. I compared three different pairs of games: Game 2 and Game 7, Game 4 and Game 9, and Game 6 and Game 11. In all these games, the relative price of giving (or taking) was one, and the Dictator can make transfers from the Recipient to self and from self to the Recipient in increments of 50 pence.

Table 15 summarises the results. The proportion of dictators transferring zero falls from 89.86% in Game 5 to 59.42% in Game 14. In addition, the proportion of dictators transferring zero falls from 76.81% in Game 4 to 55.07% in Game 9. The difference in proportions was statistically significant.

The proportion of dictators transferring zero falls from 89.86% in Game 5 to 59.42% in Game. In these games, the Dictator's initial endowment was £10 and the Recipient's initial endowment was £6. The difference in proportions was statistically significant at the 1% (One-sided Fisher's exact test, $p=0.006$). Therefore, taking decreases the proportion of Dictators transferring zero.

Table 15. Summary of the results

Game	Number of observations	Task	Dictator's endowment (£)	Recipient's endowment (£)	Relative price	Average amount transferred (£)	Average amount taken (£)	Percentage of Dictators transferring zero	Percentage of Dictators taking £6	Fisher's exact test (transfer zero vs. take £6)
2	69	Give	3	6	1.00	0.03	-	95.65	-	Different at 1% ($p=0.000$)
7	69	Take	3	6	1.00	-	4.97	-	55.07	
4	69	Give	6	6	1.00	0.11	-	89.86	-	Different at 1% ($p=0.000$)
9	69	Take	6	6	1.00	-	4.49	-	59.42	
6	69	Give	10	6	1.00	0.47	-	76.81	-	Different at 1% ($p=0.006$)
11	69	Take	10	6	1.00	-	4.39	-	55.07	

Appendix E. Data

Session	Subject	Game 1	Game 2	Game 3	Game 4	Game 5	Game 6
1	1	0	0	0	0	0	0
1	2	0	0	0	0	0	0
1	3	0	0	0	0	0	0
1	4	0	0	0	0	0	0
1	5	0	0	0	0	0	0
1	6	0	0	0	0	0	2
1	7	0	0	0	0	0	0
1	8	0	0	0	0	0	0
1	9	0	0	0	0	1	1
2	10	0	0	0	0	1	1
2	11	0	0	0	0	0	0
2	12	0	0	0	0	0	0
2	13	0	0	0	0.5	1.5	1.5
2	14	0	0	0	0	1	10
2	15	0	1	1	1	1	2
2	16	0	0	0	0	1	2
2	17	0	0	3	3	7	2
2	18	0	0	0	0	0	0
2	19	0	0	0	1	1	0
3	20	0	0	0	0	0	0
3	21	0	0	0	0	0	0
3	22	0	0	0	0	0	0
3	23	0	0	0	0	0	0
3	24	0	0	0	0	0	0
3	25	0	0	0	0	1	2
3	26	0	0	0	0	0	0
3	27	0	0	0	0	0	0
3	28	1	0	0	0	1	0
3	29	0	0	0	0	0	0
4	30	0	0	0.5	0	0	0
4	31	0	0	0	0	0	0
4	32	0	0	0	0	2	0
4	33	0	0	0	0	0	0
4	34	0	0	0	0	0	0
4	35	0	0	0	0	0	0
4	36	0	0	0	0	0	0
4	37	0	0	0	0	0	0
4	38	0	0	0	0	0	0
4	39	0	0	1	0	1	2
5	40	0.5	0.5	0.5	0.5	0.5	0.5
5	41	0	0	0	0	0	0
5	42	0	0	1	0	1	0
5	43	0	0	1	1	2	1
5	44	2	0	0	0	0	0
5	45	0	0	0	0	0	0
5	46	0	0	0	0	1.5	2
5	47	0	0	2	0	3	1
5	48	0	0	0	0	1	2
5	49	0	0	0	0	0	0
6	50	0	0	0	0	0	0
6	51	0	0	0	0	0	0
6	52	0	0	0	0	0	0
6	53	0	0	0	0	0	0
6	54	0	0	0	0	0	0
6	55	0	0	0	0	0	0
6	56	0	0	0	0	0	0
6	57	0	0	0	0	0	0
6	58	0	0	0	0	0	0
6	59	0	0	0	0	0	0
7	60	0	0	0	0	0	0
7	61	0	0.5	0.5	0.5	1	0.5
7	62	0	0	0	0	0	0
7	63	0	0	0	0	0	0
7	64	0	0	1	0	0	0
7	65	0	0	0	0	0	0
7	66	0	0	0	0	0	0
7	67	0	0	0	0	0	0
7	68	0	0	0	0	0	0
7	69	0	0	0	0	0	0

Session	Subject	Game 7	Game 8	Game 9	Game 10	Game 11	Game 12
1	1	6	6	6	6	6	6
1	2	6	6	6	6	6	6
1	3	6	6	6	6	6	6
1	4	6	6	6	6	6	6
1	5	6	6	6	6	6	6
1	6	5	6	6	6	6	6
1	7	6	5.5	6	6	5	0
1	8	6	6	6	6	6	6
1	9	3	2	1	2	0	1
2	10	2	1.5	6	2	1	3
2	11	6	6	6	6	6	5
2	12	5	3.5	4	6	4	6
2	13	5.5	6	6	4	4	5
2	14	1.5	1	0	0	0	0
2	15	6	6	0	1	0	6
2	16	6	3.5	2	2	1	1
2	17	0	0	0	0	0	0
2	18	6	6	6	6	6	5
2	19	5	3.5	4	4.5	4	1
3	20	6	6	6	6	6	6
3	21	6	6	6	6	6	6
3	22	4	3	6	6	4	4
3	23	6	6	6	6	6	6
3	24	5	1	1	6	0	1.5
3	25	1.5	1	0	0	0	0
3	26	6	6	6	6	6	6
3	27	5	5	0	5	5	5
3	28	4	4	4	4	4	4
3	29	4	3.5	3	3	2	2.5
4	30	6	6	6	6	6	6
4	31	5	6	5	5	5	5
4	32	1.5	1	0	2	0	0
4	33	6	6	6	6	6	6
4	34	6	6	6	6	6	6
4	35	6	6	6	6	6	6
4	36	6	6	6	6	6	6
4	37	6	6	6	6	6	6
4	38	5	5	5	5	5	5
4	39	5	6	6	6	5	6
5	40	5.5	5.5	0	5.5	5.5	5
5	41	6	6	6	6	6	6
5	42	2	6	0	6	6	6
5	43	4	3.5	3	3	4	3
5	44	6	6	6	6	6	6
5	45	5	6	5	6	5	6
5	46	2	1	0	0	0	2
5	47	4	3.5	3	2.5	3	2.5
5	48	1.5	1	0	0	0	0
5	49	6	6	6	6	0	6
6	50	6	6	6	6	6	6
6	51	6	6	6	6	6	6
6	52	6	6	6	6	6	6
6	53	6	6	6	6	6	6
6	54	5.5	6	5.5	6	6	6
6	55	6	6	6	6	6	6
6	56	5	6	5	5	5	5
6	57	6	6	6	6	6	6
6	58	6	6	6	6	6	6
6	59	6	3	6	6	6	6
7	60	5	3.5	3.5	3	1.5	0
7	61	2	1	2	6	1	2
7	62	6	6	6	6	6	6
7	63	6	6	6	6	6	6
7	64	6	6	4	1	1	6
7	65	6	6	6	6	6	6
7	66	6	6	6	6	6	6
7	67	5	6	4	0	6	6
7	68	1.5	6	6	6	6	6
7	69	6	6	6	6	6	6

Session	Subject	Game 13	Game 14	Game 15	Game 16	Game 17	Game 18
1	1	0	0	0	0	0.5	0
1	2	0	0	0	0	0	0
1	3	1	0	0	0	0	0
1	4	0	0	0	0	0	0
1	5	0	0	0	0	0	0
1	6	0	0	0	0	0	0
1	7	0	0	0	0	0	0
1	8	0	0	0	0	0	0
1	9	0	0	0	0	0	0
2	10	0	0	0	0	0	0
2	11	0	0	0	0	0	0
2	12	0	0	0	0	0	0
2	13	0	1.5	0	0	0	5
2	14	0	0	0	0	0	0
2	15	0	0	0	0	0	0
2	16	0	0	0	0	0	0
2	17	0	0	0	0	0	0
2	18	0	0	0	0	0	0
2	19	0	0	0	0.5	0	1
3	20	0	0	0	0	0	0
3	21	0	0	0	0	0	0
3	22	0	0	0	0	0	0
3	23	0	0	0	0	0	0
3	24	0	0	0	0	0	0
3	25	0	0	0	0	0	0
3	26	0	0	0	0	2	0
3	27	0	0	0	0	0	0
3	28	0	0	0	0	0	0
3	29	0	0	0	0	0	0
4	30	0	0	0	0	0	0
4	31	0	0	0	0	0	0
4	32	0	0	0	0	0	0
4	33	0	0	0	0	0	0
4	34	0	0	0	0	0	0
4	35	0	0	0	0	3	0
4	36	0	0	0	0	0	0
4	37	0	0	0	0	0	0
4	38	0	0	0	0	0	0
4	39	0	0	0	0	0	0
5	40	0.5	0.5	0.5	0.5	0.5	0.5
5	41	0	0	0	0	0	0
5	42	0	0	0	0	0	0
5	43	0	0	0	0	0	0
5	44	0	0	0	0	0	0
5	45	0	0	0	0	0	0
5	46	0	0	0	0	0	0
5	47	0	0	0	0	0	0
5	48	0	0	0	0	0	0
5	49	0	0	0	0	0	0
6	50	0	0	0	0	0	0
6	51	0	0	0	0	0	0
6	52	0	0	0	0	0	0
6	53	0	0	0	0	0	0
6	54	0	0	0	0	0	0
6	55	0	0	0	0	0	0
6	56	0	0	0	0	0	0
6	57	0	0	0	0	0	0
6	58	0	0	0	0	0	0
6	59	0	0	0	0	0	0
7	60	0	0	0	0	0	0
7	61	3	0	0.5	0	1	0
7	62	0	0	0	0	0	0
7	63	0	0	0	0	0	0
7	64	0	0	0	0	0	0
7	65	0	0	0	0	0	0
7	66	0	0	0	0	0	0
7	67	1	0	0	0	2.5	0
7	68	0	0	0	0	0	0
7	69	0	0	0	0	0	0

Chapter 3

The Effect of Ex-Post Information Acquisition in Sender Receiver Games: Experimental Evidence

1. Introduction

Behavioural economists have used Sender-Receiver games (Gneezy, 2005) to investigate factors affecting information transmission in asymmetric information situations where one party to an exchange possesses important information, but the other party does not.

In a simple Sender-Receiver game, two players randomly matched are assigned to the role of Sender and to the role of Receiver. There are two options, *A* and *B*. Each option specifies a monetary payoff for the Sender and for the Receiver. Incentives to deceive exist. The Sender knows how much money each of the players gets from each option, but the Receiver has no information at all. There are two messages. The Sender sends one of the two messages to the Receiver. The Receiver reads the message and choose one of the two options. Each player is awarded a monetary payoff from the chosen option.¹ In a seminal paper, Crawford and Sobel (1982) showed that, in situations in which monetary incentives to deceive exist, the amount of information transmitted is related to the similarity of the individuals' interests. The more similar the individuals' interests, the more information is transmitted.²

However, many laboratory experiments on Sender-Receiver games have demonstrated that subjects do not frequently lie (see Gneezy (2005); Cai and Wang (2006); Dreber and

¹ Other experimental studies have studied unobserved lying behaviour by tracing its distribution from subjects' reported results of a dice roll, coin flip or matrix task (e.g. Fischbacher and Föllmi-Heusi, 2013; Hao and Houser, 2012, Bucciol and Piovesan, 2011; Mazar and Ariely, 2006).

² Subsequent researchers have applied variants of the Crawford and Sobel (1982) model. For instance, Austen-Smith and Banks (1987) allowed the informed party to use both costless and costly messages.

Johannesson (2008); Sánchez-Pagés and Vorsatz (2009); Hurkens and Kartik (2009); Rode (2010)). Sutter (2009) found that some Senders do not lie because they expect others not to follow the Sender's message. Erat and Gneezy (2012) found that many subjects are also unwilling to tell Pareto improving white lies.³ The results from these experiment have been interpreted as showing that people have lie aversion.

The literature, however, has little to say about how deception is affected by the possibility that the Receiver learns about the payoffs. In the above studies, deception is totally disclosed to the experimenter, but the Receiver never learns about the payoffs. Except for Behnk et al (2014), there is no much research on the role of ex post information about the payoffs. In this study, I aim to fill this gap.

This study investigates the role of ex-post information about the payoffs in simple Sender Receiver games. The Receiver, after making his/her decision, can find out whether he has been lied to. I ask three important questions: does the possibility of obtaining information decrease the propensity to lie? Will the Receiver be more likely to follow the Sender's message because he believes that the Sender will be more likely to tell the truth? How many subjects would pay to know the truth?

To the best of my knowledge, previous studies have not investigated willingness to acquire ex post information in sender receiver games. Eliaz and Schotter (2010) studied the demand for non-instrumental information and showed that when people are about to make a decision under risk, they are willing to acquire information even if this information does not affect their decision. There are also experimental studies of social learning in which a significant proportion of subjects acquire non-informative signals (Kübler and Weizsäcker, 2004).

To gain insight into these questions I conducted five different treatments. Treatment 1, called NoInfo, is a standard Sender-Receiver game, where the Receiver never learns

³ Rosaz and Villeval (2010) examined deception in a field experiment. In their experiment, supervisors report workers performance to the experimenter. They found that more than one third of supervisors engaged in deception.

about the payoffs. It is the baseline. In treatment 2 called ExoInfo the Receiver, after choosing between option A and B, automatically learns about the payoffs. By comparing this treatment to the baseline, I study the effect of (automatic) ex-post information in reducing deception.

In treatment 3, the Receiver, after choosing between option A and B, decides whether she wants to learn about the payoffs. The Receiver pays £1 to get this information. Treatment 4 is identical to treatment 3, but now information is free. Treatment 5 is also identical to treatment 3, except that the Receiver earns £0.10 if she decides to learn about the payoffs. Across treatments 3, 4, and 5, I examined whether and to what extent the Receiver is willing to acquire ex-post information about the payoffs.

I also examine the Receiver's respond to the Sender's message. To investigate this behaviour, I allowed Receivers to send a message back to the Senders, regardless of whether or not they learn the payoffs, and recorded these messages.⁴ I asked whether Receivers wanted to punish/reward the Sender by sending a harsh/kind message. Previous studies have showed that people are willing to punish or reward others' action even though punish or reward does not have any monetary consequences (e.g. Xiao and Houser, 2009)

In this study, I also elicit the Sender's belief about the Receiver' behaviour and the Receiver's belief about the Sender's message. For instance, does the Sender believes the Receiver will follow the message? Do Senders believe the Receiver will learn about the payoffs? Do Receivers believe the Sender will send the true message?

The main results emerging from this study can be summarised as follows. First, providing ex post information, compared to non-information, can significantly reduce the amount of deception. More specifically, the probability to lie decreases by 20%. This result is hard to reconcile with Senders being exclusively motivated by lying aversion, but it is consistent with guilt and shame aversion. Second, a significant minority of

⁴ There is an extensive literature on emotions that show that people react emotionally when treated unfairly/fairly (see Phelps, 2009 for a review of the literature)

Receivers get ex post information about the payoffs even though it is costly to themselves. This finding contradicts the standard assumption that Receivers view information as merely a means for making better decisions, in which case no one should get ex post information about the payoffs.

Moreover, the analysis of the Receivers' messages sent back to the Senders suggests that most of the Receivers do not get angry to the Sender, but they sent a sarcasm if they know the Sender has lied, and they send a thankful message if they know the Sender has told the truth. Note that in the current study, strategic reasons to punish/reward the Sender do not exist because the game is a one-shot game.

Finally, I move beyond the case of fixed messages and study how the possibility to write a free message affects deception. For instance, how many Senders lie in this case? Will they tell the truth or a straight lie? Will they send ambiguous messages that are not lies, but still intended to make the other person make the choice that the message Senders would like? To the best of my knowledge, no previous studies have explored free text message in Sender-Receiver games. The analysis of the Senders' messages reveals that free form communication leads to a range of messages. For instance, some Senders make a recommendation or leave it up to the Receiver. This result calls for considering free text messages when studying deception.

The contributions of this paper can be summarized as follows. First, the experiment in this paper provides new evidence on the effect of ex-post information in promoting truth telling in environments with asymmetric information. Apart from Behnk et al (2014), no previous study has experimentally examined the role of ex-post acquisition information in Sender-Receiver games. Second, the data supports economic models incorporating psychological factors such as guilt and shame aversion (Battigalli and Dufwenberg 2013; López-Pérez 2010). Third, I extend the analysis of experimentalists who have looked at the demand for non-instrumental information by collecting data on costly, costless and beneficial information. Furthermore, I add to the analysis of non-monetary punishment by examining Receivers' messages in a context where punishment has not strategic implications for deception. Fourth, I provide evidence that many Senders are not willing to tell straight lies.

The remainder of this chapter is organised as follows: Section 2 presents the related literature. Section 3 describes the experimental design and procedures. Section 4 introduces the theory and the experimental hypotheses. Section 5 presents the experimental results. Section 6 reports on a free-form message experiment. Section 7 discusses and Section 8 concludes.

2. Related literature

This study is closely related to three strands of literature. First, this study is related to the literature that investigates the role of ex-post information about the payoffs. To the best of my knowledge, except for one paper, the role of ex-post information about the payoffs in Sender-Receiver games has not been a focus of extant systematic investigation. The closest paper to the current study is that by Behnk et al (2014).

These authors investigated how costless ex-post information about the payoffs affects decision making in a Sender-Receiver game. There were three scenarios. Each of the scenarios contained three options. Every option consisted of a payoff for the Sender and a payoff for the Receiver. The Sender chose one message in each scenario. The computer randomly selected one of the three scenarios. The Receiver could “accept” or “reject” the message. If the Receiver accepted the message, the option in the message determined the final payoffs for both players. If she rejected the message, one of the other two remaining options was randomly chosen by the computer to determine the final payoffs for both players. Behnk et al (2014) found that lying decreased when information about the payoffs was disclosed. This finding is robust in the situation when the Sender obtained a small gain at the expense of a comparatively big loss for the Receiver. In addition, the Receivers did not anticipate an increase in honest messages when it took place. Behnk et al (2014) interpret the decrease in lying as motivated by social image concern.

The experimental design in the current study differs in several aspects from Behnk et al (2014). First, Behnk et al (2014) used a within-subject design. In the current setup, by contrast, I use a between-subject design. Second, in the current study, the Sender chooses between two messages; however, in Behnk et al (2014) the Sender chose between three

messages. Third, Behnk et al 2014 did not vary the cost of ex post information. Hence, they are not able to study the demand for ex post information about the payoffs. Despite the differences in the design, Behnk et al's (2014) main findings were broadly confirmed in the current study.

In another paper, Greenberg et al (2016) examined the role of ex-post disclosure information in a modified version of a Sender Receiver game. Greenberg et al (2016) found that Senders send the truthful message approximately twice as often in the treatment in which Receivers can detect whether the Senders lied compared to a treatment without ex post information. However, Greenberg et al (2016) used a different experimental set up. The Sender can lie about the result of a six-sided die roll. In addition, the Sender sends two messages. Furthermore, Greenberg et al (2016) used the strategy method to elicit choices for both players.

This study is also related to the literature that demonstrates that telling the truth is driven by guilt and shame aversion. Senders showed apparently concern for not letting others down another. If the Sender believed the Receiver expected an honest message, the Sender will tell the truth. Charness and Dufwenberg (2006) found that the second mover's promise often promotes the frequencies of the first mover's trustful actions and the second mover's trustworthy actions. Charness and Dufwenberg (2006) concluded that guilt aversion makes people keep their promises. If the Sender believed the Receiver believed him/her, the Sender would wish to keep his/her promise. Battigali and Dufwenberg (2013) showed that that people feel guilty when they betray another person's expectation.

Lundquist et al (2009) showed that some people would experience a disutility from lying that exceeds the corresponding expected monetary gains. Battigalli et al (2013) showed that guilt aversion can explain Gneezy's (2005) data. Vanberg (2008) showed that people have preferences for keeping their promises per se.

3. Experimental design and procedures

The experimental set up builds on the classical Sender-Receiver game introduced in Gneezy (2005). There are two players, the Sender and the Receiver. I sometimes refer to these as Player 1 and 2, respectively. There are two exogenously given options, A and B. Each option specifies a monetary payoff for the Sender and for the Receiver. The Sender knows how much money each of them gets from each option. The Receiver has no information at all. There are two messages. The Sender sends one of the two message to the Receiver. The Receiver reads the message, and then chooses an option, A or B. Each player receives his or her money earnings from the chosen option, and this terminates the game. The messages were:

Message 1: "Option A will earn you more money than option B."

Message 2: "Option B will earn you more money than option A."

Table 1 shows the monetary payoff for Option A and Option B for the Sender and for the Receiver. According to the payoff matrix, Message 1 tells the truth, whereas Message 2 tells a lie. It is noteworthy that unlike Gneezy (2005), one of the options (option A) provides the same payoff for both players. In addition, the total payoff from Option A equalled the total payoff from Option B, so I ruled out efficiency concerns. The experiment encompassed five different treatments (NoInfo, ExoInfo, EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Gain). Table 2 summarized the experimental treatments.

Table 1. The payoff matrix

	Sender's payoff (£)	Receiver's payoff (£)
Option A	10	10
Option B	14	6

In treatment 1 (henceforth called NoInfo), after choosing between options A and B, the Receiver learns about his own payoff, but never learns about the Sender's payoff. This served as a baseline treatment. I examined how truthful will the Sender's message be and to what extent will it be believed.

Treatment 2 (henceforth called ExoInfo) was identical to the NoInfo treatment, except that now the Receiver, after choosing between options A and B, automatically learns about all the payoffs. I investigated whether disclosing ex-post information would have an effect on deceptive behaviour. As a further dimension to the experimental design, I allowed Receivers to send back a text message to the Sender.

The Receiver had 120 seconds to write one or several text messages and send them to the Sender. She could also decide not to write anything. The Sender read the message. Since this task was administered after the Receiver got ex-post information in ExoInfo, the messages sent by the Receiver cannot have any strategic implications for the Receiver. Instead, the messages can provide an opportunity for the Receiver to punish/reward the Sender's behaviour if the Sender has lied/told the truth by sending a harsh/kind message.

Treatment 3 (henceforth called EndoInfo-Costly) was identical to ExoInfo, except that now the Receiver, after choosing between options A and B, decides whether or not learn about the payoffs. If the Receiver chooses to learn about the payoffs, she pays £1. Regardless whether or not the Receiver learns about the payoffs, the Receiver can still send a message back to the Sender. A rational Receiver should never, regardless of his or her social preferences, pay a positive sum of money for such information.

Treatment 4 (henceforth called EndoInfo-Free), was identical to EndoInfo-Costly except that now information is free. Treatment 5 (henceforth called EndoInfo-Gain) was identical to EndoInfo-Costly, except that now if the Receiver chooses to learn about the payoffs, she earns £0.10. Using data in EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Costly, I study whether and to what extent the Receiver is willing to acquire ex-post information about the payoffs.

Table 2. Experimental treatments

Treatment	Number of sessions	Number of subjects	Receiver gets ex-post information about the payoffs	Ex-post information about the payoffs	Amount paid by the Receiver to get ex-post information about the payoffs (£)	Receiver sends a message back to the Sender
NoInfo (T1)	2	44	No		-	No
ExoInfo (T2)	2	40	Yes	Exogenous	-	Yes
EndoInfo-Costly (T3)	4	40	Yes	Endogenous	1.00	Yes
EndoInfo-Free (T4)	2	36	Yes	Endogenous	0.00	Yes
EndoInfo-Gain (T5)	2	28	Yes	Endogenous	-0.10	Yes

3.1. Elicitation of beliefs

After making his/her decision, I asked the Sender to state which option he/she expected his/her counterpart to choose. I also asked Senders to estimate the total percentage of Receivers in the session who decided to follow the Sender's message. Likewise, after the Receiver made her decision, I asked Receivers to state their expectations about the Sender's message.

I also elicited the Sender's beliefs about the Receiver's information decision by asking whether she expected her counterpart to learn about the payoffs. In addition, I asked Senders to estimate the total percentage of Receivers in the session who decided to learn about the payoffs. None of the answers to these questions were monetary incentivised.

All elicitations of beliefs were made before feedback on final payoffs were given. A sample screenshot of the questions faced by the Sender and a screenshot of the questions faced by the Receiver can be found in this chapter's Appendix E. Belief elicitation's questionnaires.⁵

⁵ Note that question 1 was shown for all treatments, question 2 was shown for all treatments with fixed messages NoInfo, ExoInfo, EndoInfo-Costly, EndoInfo-Free, EndoInfo-Gain, and question 3 and question 4 were shown for all treatments with endogenous ex-post information about the payoff EndoInfo-Costly, EndoInfo-Free, EndoInfo-Gain, and FreeMess-EndoInfo-Costly.

3.2. Procedures

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) computerized laboratory at the University of East between November 2015 and June 2016.⁶ Subjects were undergraduate students, randomly recruited from a variety of disciplines using the on-line recruitment system ORSEE (Greiner, 2015). The experiment was fully programmed and conducted using z-Tree package (Fischbacher, 2007).

Each treatment had two sessions; except for EndoInfo-Costly treatment, which had four sessions. In total, I had 272 subjects: 188 (94 Senders and 94 Receivers) subjects for the fixed messages treatments. None of the subjects participated in more than one session. The average age was approximately 20.14 years (S.D. 2.03, min. 18, max. 31), 48.40% of the subjects were female, 87.77% were from United Kingdom, 19% studied Economics and 4% studied Business Management. Details of the socio-economic background of the experimental participants are available in Appendix C. Background of the participants in fixed messages.

On arrival, subjects were immediately sent in pairs to the lab with had visually separated computer terminals to ensure anonymity. Each computer terminal had a number.⁷ I placed next to each computer a blank paper, a pen, and a receipt form. Before entering the lab, each subject drew a numbered ball without replacement from a cotton bag; the number on the ball indicated their seat in the computer laboratory. Communication between subjects was prohibited.

At the start of each session, all subjects received computerized common instructions on their screen. The experimenter read the instructions aloud as the students followed along

⁶ Treatment NoInfo was run November 2015, ExoInfo was run in December 2015, EndoInfo-Costly was run in January 2016, EndoInfo-Free in May 2016, EndoInfo-Gain was run in February 2016.

⁷ As soon as subjects arrived, they were immediately sent to the laboratory in pairs in order to minimise the likelihood of subjects interacting with each other. I also decided to pay a turn-away fee of £3 to the subjects who signed up and arrived on time but were not able to participate; such a payment was pre-announced

on their computer screens. Instructions were as neutral as possible. A sample of the experimental instructions is included in Appendix A. Experimental instructions for fixed messages of this chapter.⁸

Once the common instructions were read by the experimenter, instructions specific to role Player 1 or 2 were shown on the respective screens. Subjects read instructions specific to their role in silence. If a subject did not understand any part of the instructions, he/she raised his/her hand for clarification. All clarifications were given privately by the experimenter. The experiment started when everybody indicated they had fully understood the instructions.

In each session, subjects were randomly matched in pairs by the computer. One of them was assigned to the role of Player 1 (the Sender) and the other to the role of Player 2 (the Receiver). They held their roles throughout the experiment. Anonymity was preserved at all times during or after the experiment: Player 1 did not know the identity of Player 2 and vice versa.

Figure 1. Sender's decision screen shows a screenshot of the task faced by the Sender. There are two buttons. One button reading: "I choose to send Message 1" and another button reading "I choose to send Message 2". The Sender makes her decision by clicking on one of the two buttons.

⁸ The complete experimental screenshots, the instructions specific to Sender and to Receiver, and the complete data set is available on request.

Figure 1. Sender's decision screen

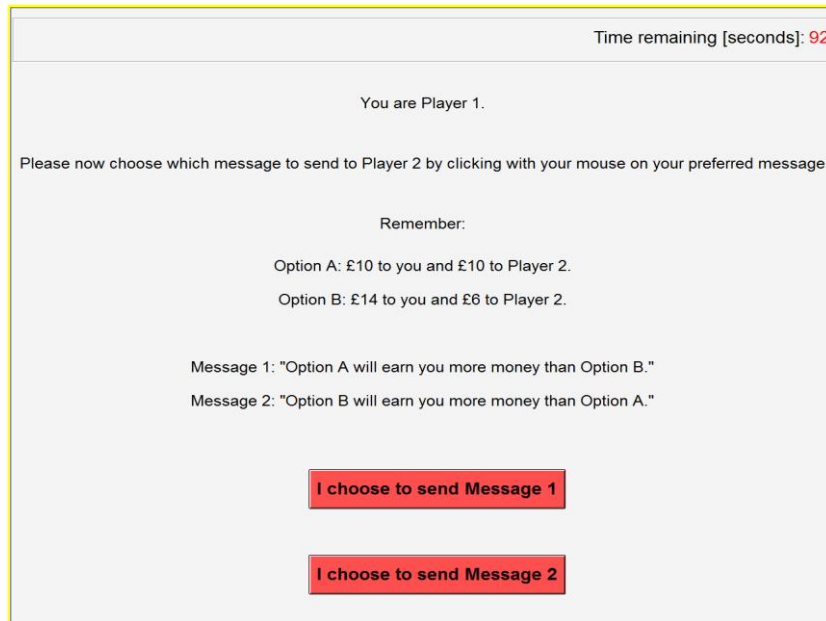


Figure 2 shows a screenshot of the task faced by the Receiver. There are two buttons. One button reads "I choose option A" and another button reads "I choose option B." The Receiver makes her decision by clicking on one of the two buttons. The Receiver can read the message sent by the Sender.

Figure 2. Receiver's decision screen

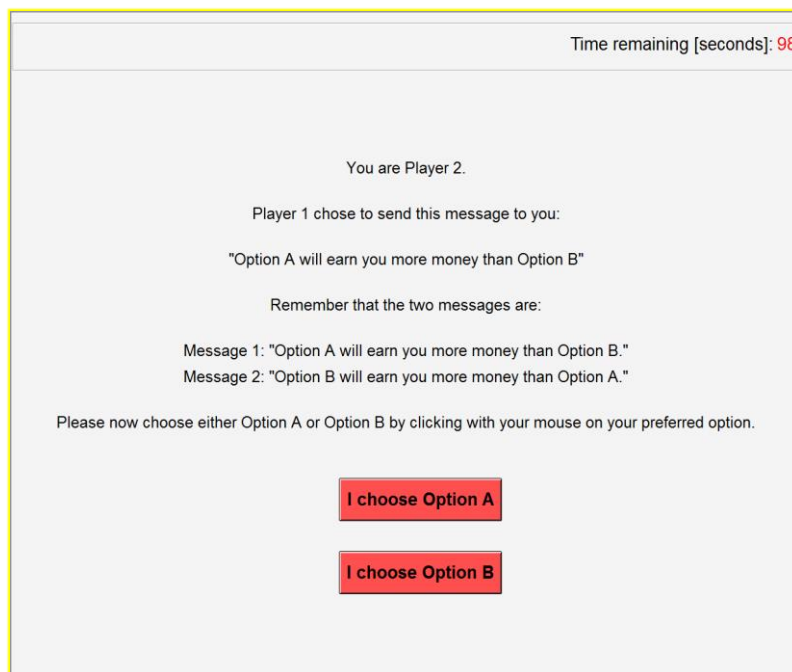


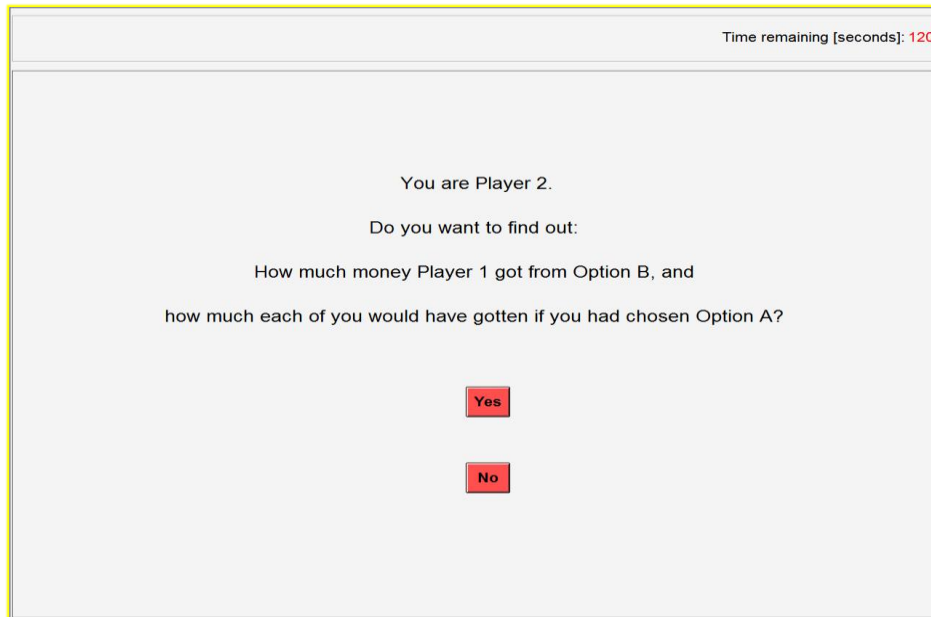
Figure 3 shows a screenshot for the task faced by the Receiver, when the Receiver has to choose whether to learn the payoffs or not. There are two buttons. One button reads “Yes” and another button reads “No.” I randomized the order of the appearance of the buttons at the individual level. The Receiver makes her decision by pressing on one of the buttons.

I also gathered information about receiver’s motivation to learn the payoffs by asking all receivers to state their reasons for choosing or not to learn about the payoffs. Answers to this question uncover motivations for learning or not the payoffs.

After stating their reasons, subjects received information about their payoffs on their own screen. Receivers who did not choose to learn about the payoffs were not informed about the Sender’s payoffs. At the end of the experiment, each participant filled a demographic questionnaire (see Appendix F. **Post-experimental questionnaire**), which gathered data on gender, field of study, country of origin and feedback about the experiment. The questionnaire was also computerized using programme z-Tree (Fischbacher 2007).

Each session lasted on average 45 minutes. The average earnings across sessions was £11.57 for Senders (S.D. 1.96, min.10, and max. 14) and £8.40 for Receivers (S.D.1.98, min. £5, and max £10.1). Each participant individually signed and dated a payment receipt form and received payment. Subjects were called out one by one to receive their payment.

Figure 3. Receiver's screen for ex-post information decision about the payoffs



4. Theoretical predictions

4.1. Self-interest and risk neutrality

In this experiment, the Sender has information about all the payoffs, but the Receiver knows nothing about the payoffs. I do not know the Receiver's prior beliefs regarding these payoffs. Suppose the Receiver is self-interested and only cares about her own monetary payoff. In assessing the truthfulness of the messages, she will ask the following questions: How much conflict do I think there is between us – do I prefer the same, or a different, option? I do not know the Receiver's beliefs about the extent of conflict between the two players.

In order to develop predictions, I assume that the Receiver is equally likely to think that there are divergent as common interests. I say the Receiver 'follows the message' if she chooses the option that the received message says is best. As a benchmark, I assume that both the Sender and Receiver are self-interested and risk neutral, and that this is common knowledge. Therefore, I hypothesized that:

Hypothesis 1. *(Self-interest and risk-neutrality): The Sender sends the message that maximizes his expected monetary payoff, given his beliefs about the Receiver's likelihood of following the message.*

Hypothesis 2. *(Self-interest and risk-neutrality): The Receiver follows the message if she believes that the message is sufficiently likely to be true.*

Under self-interest, the Sender sends the false message if he believes the Receiver is sufficiently likely to follow the message. Otherwise, the Sender sends the true message. In this experiment, I ask Senders and Receivers for their beliefs (about whether the Receiver will follow the message or not, and whether the Sender's message is true, respectively). I use the data to assess the specific predictions based on self-interest and risk neutrality. Regarding ex post information acquisition, I hypothesized that:

Hypothesis 3. *(Self-interest and risk-neutrality): The Receiver avoids information in Endo-Info Costly, is indifferent between getting info in Endo-Info Free, and obtains information in Endo-info Gain*

4.2. Inequality aversion

Existing experimental evidence show that individuals might also hold preferences for fairness or inequality aversion (Fehr and Schmidt, 1999; and Bolton and Ockenfels, 2000). Suppose then the Sender and the Receiver are sufficiently inequity averse, such that the Sender prefers payoffs (10, 10) to (14, 6). Therefore, I hypothesized that:

Hypothesis 4. *(Inequity averse): The Sender sends the true message if he believes the Receiver is sufficiently likely to follow the message. Otherwise, he sends the false message.*

Note that if the Sender is sufficiently likely to believe that the Receiver will follow the message, then the message sent will reveal whether the Sender is driven by self-interest preferences or inequity aversion. Consider, for example, a Sender who assigns low probability to the Receiver following the message. A self-interested Sender will send the true message, while an inequity averse Sender will send the false message.

4.3. Lying aversion

Existing laboratory studies on sender receiver games have also suggested that people are averse to lying. Dufwenberg and Kirchsteiger (2004), Vanberg (2008) developed models to capture the role of lying aversion in Sender-Receiver games. According to these models, people experience some disutility when they tell a lie, regardless of other people's expectations. Suppose the Sender expects not to be followed, but is averse to lying. He would prefer (10, 10) to (14, 6). Now, suppose the Sender expect to be followed. He would still prefer (10, 10) to (14, 6). Therefore, I hypothesized that:

Hypothesis 5. (*Lying averse*): *The Sender sends the true message, regardless of whether he believes the Receiver is sufficiently likely to follow the message or not.*

4.4. Guilt and shame aversion

An important feature of the design is that the Receiver can learn about the payoffs.⁹ López-Pérez (2010) offered a formal analysis of how guilt and shame affect preferences and concluded that people experience some utility loss (feel guilty) if they believe that they have failed to meet another person's expectations. Hence, if the Sender feels guilt and shame, the possibility of revealing the payoffs can influence the Sender's behaviour.¹⁰

Suppose the Sender is self-interested that he will prefer (14, 6) to (10, 10). Suppose that the Sender expects the Receiver to follow the message. Then, he will send the false

⁹ Others models of guilt aversion include Charness and Dufwenberg (2006), Battigalli and Dufwenberg (2013), and Reuben et al (2009). Usually guilt is 'internal' or 'private' while shame is 'external' or 'public'. None of the models includes ex-post information about the payoffs as a variable in the analysis. Guilt and shame are considered in the psychological literature as self-conscious emotions.

¹⁰ However, guilt aversion and shame aversion have been studied separately. Shame is an external, a social or 'public' matter, while guilt is a personal, an internal or 'private' matter. López-Pérez (2010) reviewed the most recent psychological literature on guilt and shame aversion and showed that the literature does not support the view that there is a difference between guilt and shame aversion.

message. Would he choose to lie if he knew the Receiver will know the truth? If the Sender does not have any feeling of guilt and shame, and cares only about material outcomes, then his behaviour should stay the same over the treatments. Therefore, the Sender's message is not affected by whether the Receiver is informed or not (ExoInfo and NoInfo) or by whether the Receiver can get ex-post information or not (NoInfo and EndoInfo-Free, NoInfo and EndoInfo-Costly, NoInfo and EndoInfo-Gain).

Suppose then the Sender is sufficiently guilt and shame averse, such that the Sender cares how the Receiver will think about him. Then, it should be more costly for him to lie if the Sender believes that the Receiver will get ex-post information about the payoffs. If the sender feels guilt and shame, he will not be comfortable lying, expecting that the Receiver will find out the truth. Therefore, the Sender will send the true message.

Hypothesis 6. *(Guilt and shame averse): The Sender sends the true message if she believes that the Receiver is sufficiently likely to learn about all the payoffs.*

Under guilt and shame aversion, more Senders will send the true message in ExoInfo than in NoInfo. In this experiment, I ask Senders for their beliefs about whether the Receiver will get ex-post information about the payoffs or not. If Senders suffer from guilt and shame aversion, then this hypothesis will explain why people tell the truth even though they have a monetary incentive to lie. I use the data to assess the specific predictions based on guilt and shame aversion.

In addition, the Sender's beliefs about whether the Receiver gets ex-post information about the payoffs can also be affected by the cost of information. For instance, guilt and shame aversion can become stronger with an increase in the probability that the payoffs will be revealed to the Receiver. The higher the cost of the information, the lower the probability to get ex-post information, the higher the probability to send a false message. Therefore, I hypothesized that:

Hypothesis 7. *(Guilt and shame averse): More Senders will send the true message in EndoInfo-Gain and EndoInfo-Free than in EndoInfo-Costly.*

The probability to follow the Sender's message can also be affected by the availability of ex post information about the payoffs. If the Receiver anticipates an increase in the probability of receiving a true message, more receivers will follow the message in ExoInfo than in NoInfo. Similarly, more Receivers will follow the message in EndoInfo-Gain and EndoInfo-Free than in EndoInfo-Costly. The higher the cost of ex-post information, the lower the probability to get ex-post information, and higher the probability of receiving a false message.

Hypothesis 8. (*Guilt and shame averse*): *More Receivers will follow the message in ExoInfo than in NoInfo.*

5. Results

This section is organised as follows. First, I begin by establishing that the results are consistent with the results obtained in previous standard Sender-Receiver games. Second, I conduct a descriptive analysis characterising the Sender and Receiver's behaviour. Then, I conduct a regression analysis to test the main hypotheses. Afterwards, I present an analysis of the content of the Receiver's message sent back to the Sender.

Table 3 reports the result of the experiment. There were 22 sender receiver pairs in NoInfo, 20 sender receiver pairs in ExoInfo, 20 sender receiver pairs EndoInfo-Costly, 18 sender receiver pairs in EndoInfo-Free treatment, and 14 sender receiver pairs in EndoInfo-Gain.

5.1. External validity

Finding 1: *The results in NoInfo are consistent with the results generally reported from previous Sender Receiver games*

NoInfo corresponds to the standard Sender-Receiver game without ex post information about the payoffs. The results in NoInfo are consistent with the results generally

reported from previous Sender-Receiver games. As expected, a majority of Senders send Message 1. Further, a high proportion of Receivers follow the Sender's message.

5.2. Descriptive statistics

Finding 2: *Ex-post information about the payoffs leads to higher frequency of truthful messages than without ex post information about the payoffs.*

Row (1) of Table 3 indicates the relative frequency of sending Message 2. Overall, Senders chose Message 2 in 31% of cases. The frequency of sending Message 2 was lower in the treatments with ex post information compared to the baseline NoInfo (41%). However, the frequencies of sending Message 2 were not statistically different across treatments (Fisher's exact test, p-value = 0.674). This preliminary evidence will be verified later in the regression analysis.

Finding 3: *The proportion of Receivers who follow the Sender's message does not change significantly across treatments.*

Row (3) of Table 3 indicates the relative frequency of Receivers following the Sender's message. The Receiver follows the message if she chooses the option that the Sender says is the best. Overall, Receivers followed the Sender's message in 87% of the cases. The fact that a vast majority of Receivers followed the Sender's message suggested that messages were not cheap talk.

Considering all treatments, the frequencies of following the Sender's message were not statistically different across treatments (Fisher's exact test, p-value = 0.963). Therefore, the Receiver's decision to follow the Sender's message was not affected by the experimental treatment.

Finding 4: *The proportion of Senders who expect the Receiver to follow the chosen message does not change across treatments.*

Row (4) of Table 3 shows the relative frequency of Senders expecting the Receiver to follow the chosen message. The Sender expected the receiver to follow the message if he/she expects the Receiver to implement the option that was indicated in the chosen message. Overall, Senders expected the Receiver to follow the chosen message in 87% of cases.

Considering all treatments, the frequencies of expecting that the Receiver will follow the chosen message were not significantly different across treatments (Fisher's exact test, p -value=0.338). Therefore, Sender's expectations were not affected by ex post information.

Finding 5: *Ex-post information about the payoffs leads to higher frequency of Receivers expecting truthful messages than without ex post information about the payoffs.*

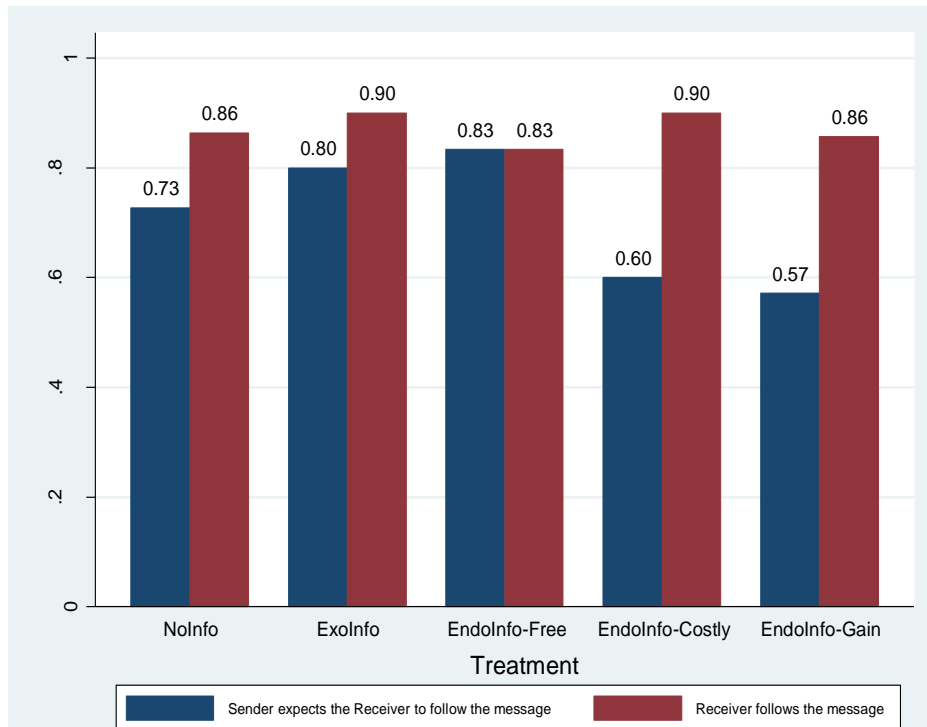
Row (6) of Table 3 indicates the relative frequency of Receivers expecting the Sender to send Message 1. Overall, Receivers expect the Sender to send Message 1 in 78% of the cases. The proportion of Receivers expecting the Sender to send Message 1 is lower in treatments with ex post information than in the baseline with no information. This evidence suggests that the Receiver anticipated the Sender to tell the truth when information about the payoffs was available. However, considering all these treatments, there were not significant differences across treatments (Fisher's exact test, p -value=0.249).

Finding 6: *Senders have correct expectations about how likely the Receiver will follow the chosen message, except in EndoInfo-Costly and EndoInfo-Gain.*

Figure 4 illustrates how the relative frequency of Senders expecting the Receiver to follow the chosen message mirrored the relative frequency of Receivers actually following the chosen message in each of the five treatments. However, a test of proportion showed that the two frequencies are statistically different at the 5% level in EndoInfo-Costly (p = 0.0285) and at the 10% level in EndoInfo-Gain (p = 0.0943).

It is also important to note that Senders expect on average 60% of the Receiver population to implement the Sender’s message, and these expectations do not seem to change across treatments (see Row (9) of Table 3).

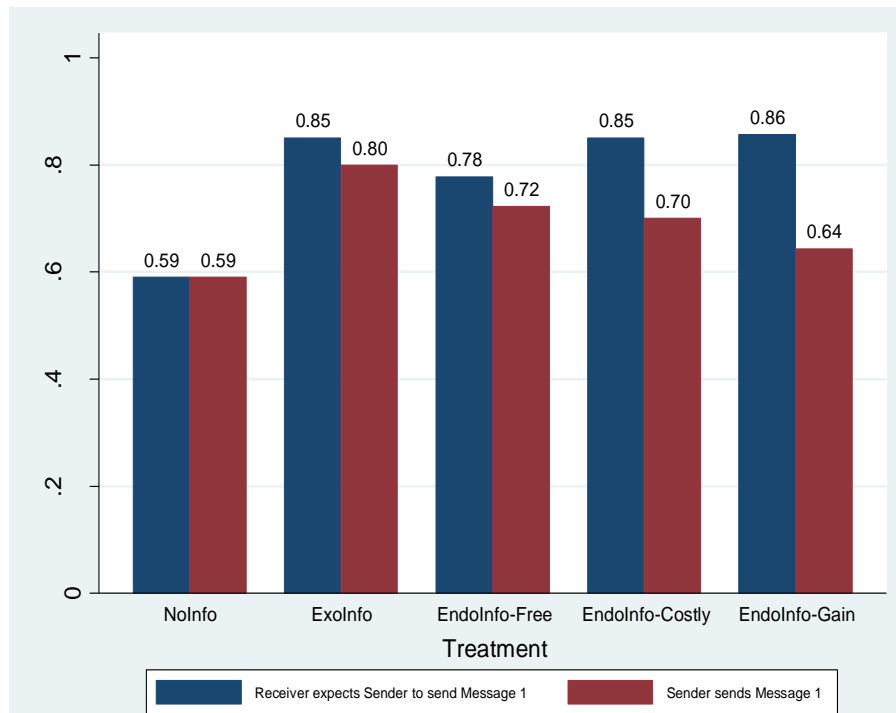
Figure 4. Fraction of Senders who expect the Receiver to follow the chosen message and Receivers who actually follow the Sender’s message



Finding 7: *Receivers do not necessarily anticipate an increase in the number of truthful messages by the Sender*

Figure 5 shows how similar the relative frequency of Receivers expecting the Sender to send Message 1 was to the relative frequency of Sender actually sending Message 1 in each of the five treatments. The figure shows that Receiver’s expectations does not change drastically across treatments. These results suggest that several Receivers did not anticipate an increase in the number of truthful messages in the treatments with ex post information.

Figure 5. Fraction of Receivers who expect the truth and Senders who actually send Message 1



Finding 8: *The frequency of Receivers learning about the payoffs changed across treatments.*

Row (7) Table 3 indicates the relative frequency of Receivers choosing to learn about the payoffs. More Receivers are willing to learn about the payoffs in EndoInfo-Free (90% of the Receivers), and EndoInfo-Gain (86% of the Receivers) than in EndoInfo-Costly (20% of the Receivers) where ex post information was costly

Considering all treatments, the frequency of Receivers learning about the payoffs was significantly different across treatments (Fisher's exact test, $p=0.000$). Hence, the Receiver's decision to learn about the payoffs depended on how costly it was to be informed. Behnk et al (2014) studied the role of ex-post information about the payoffs in a Sender-Receiver game, but the Receiver did not choose whether to get ex-post information about the payoffs, hence they are not able to study the demand for ex post information about the payoffs.

To test whether the proportion of Receivers choosing to get ex-post information about the payoffs in EndoInfo-Costly was significantly different from zero, I performed a one-sample t-test of proportion. I found that the proportion is statistically significantly different from zero at 5% of significance level (two tailed t-test (19) = 2.1794, p-value=0.042). This finding provided evidence that a significant minority of subjects chose to learn about the payoffs, even though this information was costly to the Receivers.

Finding 9: *Senders are less likely to expect the Receivers will learn about the payoffs in EndoInfo-Costly than in EndoInfo-Free and EndoInfo-Gain.*

Row (8) of Table 3 shows the relative frequency of Senders expecting the Receiver to get ex-post information about the payoffs. Overall, Senders expect the Receiver to learn about the payoffs in 65% of cases. Considering all treatments, there are significant differences in these expectations across treatments (Fisher's exact test, p-value=0.338). Therefore, Sender's expectations are affected by the experimental treatment.

Finding 10: *Senders have correct beliefs about how likely the Receivers will learn about the payoffs*

It is also noteworthy that the relative frequency of Senders expecting the Receiver to learn about the payoffs was very similar to the relative frequency of Receivers actually learning the payoffs. This can be seen in Figure 6. Both relative frequencies were very close in each of the three treatments. Moreover, Senders expected on average 64% of the Receiver population to learn about the payoffs (see Row (10) of Table 3).

Figure 6. Relative frequency of Senders who expect the Receiver to learn about the payoffs and Receivers who actually learn about the payoffs

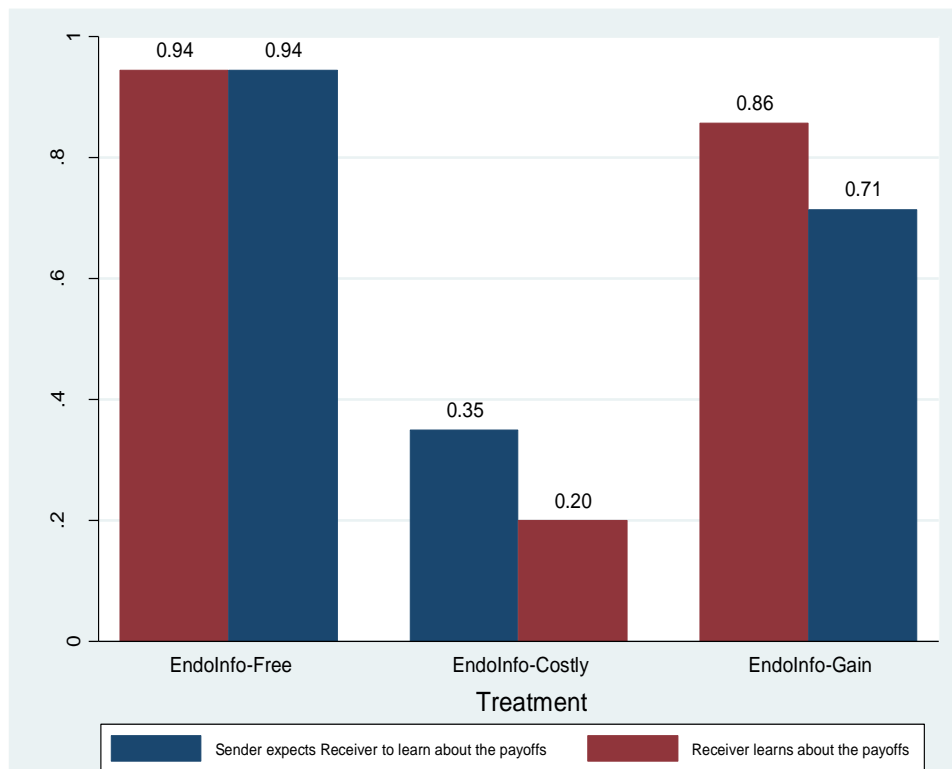


Table 3. Descriptive statistics (relative frequencies)

	Treatment					Total
	No-Info	Exo-Info	EndoInfo-Costly	EndoInfo-Free	EndoInfo-Gain	
Number of observations	44 (22 pairs)	40 (20 pairs)	40 (20 pairs)	36 (18 pairs)	28 (14 pairs)	188 (94 pairs)
(1) P1 sends Message 2	0.41	0.20	0.30	0.28	0.36	0.31
(2) P2 chooses Option B	0.55	0.20	0.40	0.33	0.50	0.39
(3) P2 follows P1's message	0.86	0.90	0.90	0.83	0.86	0.87
(4) P1 expects P2 to follow chosen message	0.73	0.80	0.60	0.83	0.57	0.71
(5) P1 expects P2 to choose Option B	0.41	0.30	0.20	0.22	0.36	0.30
(6) P2 expects P1 to tell the truth	0.59	0.85	0.85	0.78	0.86	0.78
(7) P2 gets ex-post information about the payoffs	-	-	0.20	0.94	0.86	0.63
(8) P1 expects P2 to get ex-post information about the payoffs	-	-	0.35	0.94	0.71	0.65
(9) Receivers population expected to follow the Sender's message	0.59	0.68	0.59	0.62	0.50	0.60
(10) Receivers population expected to learn about the payoffs	-	-	0.40	0.86	0.71	0.64

Note: P1 refers to the Sender and P2 refers to the Receiver.

5.3. Regression analysis

In this section, I further estimate several probit models to test the main experimental hypotheses.¹¹ Table 25 in the Appendix H. **Regression variables** shows the definitions of all regression variables used in the analysis.

5.3.1. Sender's message decision

To investigate the factors that result in the Sender choosing Message 2, I estimated two probit models. The dependent variable was a dummy variable ($p1m2$), which takes the value of 1 if Sender sends Message 2, and 0 otherwise.

In Model I, the independent variables included a dummy variable for each experimental treatment (NoInfo, ExoInfo, EndoInfo-Costly, EndoInfo-Free, EndoInfo-Gain) and a dummy variable ($p1expfoll$) which takes the value of 1 if the Sender expects the Receiver to follow the Sender's message. I also included age and a dummy variable (female) that equals 1 if Sender is female and 0 otherwise to control for individual's characteristics.

Model II excluded the dummy variables NoInfo and ExoInfo, but included a dummy variable ($P1exinfo$) which takes the value of 1 if Sender expects the Receiver to get information, and 0 otherwise. Table 4 reports the results of the estimation for the two models.

Finding 11: *The Sender's honesty depends on the Sender's belief the Receiver is sufficiently likely to follow the chosen message.*

The coefficient for $p1expfoll$ in Model I and in Model II is negative and strongly significant. In other words, Senders are less likely to send Message 2 when they expect the Receiver will follow the Sender's message. The marginal effect indicates that the

¹¹ I also ran the regressions without considering the variables age and gender. The results do not change qualitatively if age and gender were not included among the explanatory variables.

probability to lie decreased by 30.1% in Model I and by 49.1% in Model II, while the other variables are at their mean. This finding supports Hypothesis 4.

A self-interest Sender will lie if he/she believes the Receiver will follow the message. However, a Sender with inequity aversion preferences will tell the truth if he/she believes the Receiver will follow the message. This behaviour is consistent with any theory of fairness (Fehr and Schmidt 1999, and Bolton and Ockenfels 2000).

Finding 12: *An automatically disclosure of information affects the Sender's decision to lie.*

The fact that the Receiver learns automatically about the payoff has a negative and significant impact on the decision to send Message 2. Senders are more likely to tell the truth when they know that the Receiver will learn automatically about the payoffs (ExoInfo) than in NoInfo. The marginal effect indicates that the probability to tell the truth increases by 20.4%, while the other variables are at their mean. This finding supports Hypothesis 6. Note that the probability to lie is significantly lower in ExoInfo relative to NoInfo, even though the non-parametric test was not significant.

An explanation for this result is that Sender's behaviour is driven by guilt and shame aversion. Previous studies (López-Pérez 2010, Charness and Dufwenberg 2006) have shown that people feel guilty and shame, if they let others down. If subjects were guided by lying aversion, regardless of whether the Receiver learn about the payoffs or not, Senders will tell the truth. This finding suggests that aversion to lying cannot explain the decrease in the relative frequency of lying. This finding also corroborates previous findings in Behnk et al (2014)

Finding 13: *In the EndoInfo treatments, the Sender's honesty depends on the Sender's belief that the Receiver is sufficiently likely to learn about all the payoffs.*

The Sender's belief about whether the Receiver will learn the payoffs has a negative and significant impact on the decision to send Message 2. The coefficient for P1exinfo was negative and statistically significant. The more likely the Senders believes the Receiver will follow the Message, the more likely the Sender will tell the truth. The marginal effect

indicates that the probability to lie decreases by 34.4%, while the other variables are kept at their means. This finding also supports Hypothesis 6.

Finding 14: *More Senders send Message 1 in EndoInfo-Costly than in EndoInfo-Free*

The cost to be informed has an impact on the decision to send Message 2. The coefficient for EndoInfo-Free in Model II was positive and significant, meaning that Senders in EndoInfo-Free are more likely to tell send Message 1 than in EndoInfo-Costly. The marginal effect indicates that the probability to send Message 1 increases by 35.4%, while the other variables are at their mean. This result supports Hypothesis 7.

Table 4. Determinants of sending Message 2

Independent variables	Model I		Model II	
	Sender sends Message 2 (P1m2)		Sender sends Message 2 (P1m2)	
	Coefficient	Marginal effects	Coefficient	Marginal effects
ExoInfo	-0.685 (0.438)	-0.204*		
EndoInfo-Costly	-0.568 (0.428)	-0.174		
EndoInfo-Free	-0.346 (0.421)	-0.111	1.014** (0.578)	0.354**
EndoInfo-Gain	-0.354 (0.420)	-0.112	0.660 (0.536)	0.234
Sender expects the Receiver to learn about the payoffs (P1expinfo)			-0.987 * (0.505)	-0.344*
Sender expects the Receiver to follow the message (P1expfol)	-0.829*** (0.317)	-0.301***	-1.409 *** (0.462)	-0.491***
Female	-0.042 (0.286)	-0.014	0.286 (0.438)	0.095
Age	0.150 ** (0.074)	0.051**	0.111 (0.088)	0.037
Constant	-2.576 * (1.501)		-1.922 (1.941)	
N° Observations	94		52	
Prob > χ^2	0.0630		0.0237	
Pseudo R-Square	0.1181		0.2393	
Log-likelihood	-51.2249		-24.4147	

Note: The dependent variable was a dummy variable (P1m2 = 1 if the Sender sends Message 2, P1m2 = 0 otherwise). Robust standard errors in parentheses. Marginal effects evaluated at the mean values for the independent variables. *** p < 0.01, ** p < 0.05, * p < 0.1.

5.3.2. Receiver's option decision

To investigate the factors that result in the Receiver to follow the Sender's message I estimated a probit model, where the dependent variable was a dummy variable ($P2_{\text{follow}}$) which takes the value of 1 if the Receiver followed the Sender's message and 0 otherwise. The independent variables included a dummy variable for each experimental treatment (NoInfo, ExoInfo, EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Gain) and a dummy variable ($P2_{\text{expecttruth}}$) which takes the value of 1 if the Receiver expects the Sender message true. Moreover, I included age and a dummy variable for gender. Table 5 reports the results.

Finding 15: *The Receiver's decision to follow the message depends on the Receiver's belief that the Sender's message is sufficiently likely to be true.*

The coefficient for the $p2_{\text{expecttruth}}$ was positive and significant, meaning that Receivers are more likely to follow the message when they expect the Sender to tell the truth. The marginal effect indicates that the probability to follow increases by 50.3%, while other variables are at their mean. A probit model with only endogenous treatments confirms the positive relationship between the decision to follow the chosen message and the Receiver's expectations about the truthfulness of the Sender's message. This evidence provides support for the Hypothesis 2.

Table 5. Determinants of following the Sender's message

Independent variables	Model III		Model IV	
	Receiver follows the message (P2foll)		Receiver follows the message (P2foll)	
	Coefficient	Marginal effects	Coefficient	Marginal effects
ExoInfo	-0.221 (0.653)	-0.024		
EndoInfo-Costly	-0.477 (0.559)	-0.059		
EndoInfo-Free	-0.806 (0.681)	-0.119	-0.320 (0.584)	-0.039
EndoInfo-Gain	-0.708 (0.710)	-0.104	-0.133 (0.565)	-0.016
Receiver expects the truth (P2exptruth)	2.217*** (0.497)	0.503***	1.926*** (0.611)	0.472**
Female	0.234 (0.445)	0.023	-0.125 (0.495)	-0.014
Age	0.205** (0.088)	0.020*	0.372** (0.177)	0.042**
Constant	-3.877** (1.881)		-7.572** (3.585)	
N° Observations	94		52	
Prob > χ^2	0.0001		0.0069	
Pseudo R-Square	0.4276		0.3936	
Log-likelihood	-20.5507		-12.4581	

Note: The dependent variable was a dummy variable (P2foll = 1 if the Receiver follows Sender's message, P2foll = 0 otherwise). Robust Standard errors in parentheses. Marginal effects evaluated at the mean values for the independent variables. *** p < 0.01, ** p < 0.05, * p < 0.1.

5.3.3. Receiver's information decision

To examine the factors that result in the Receiver learning about the payoffs, I estimated a probit model Model V. The dependent variable was the Receiver's information decision (p2info) which takes value 1 when the Receiver learns about the payoffs, and 0 otherwise.

The independent variables included a dummy variable for each experimental treatment (EndoInfo-Free and EndoInfo-Gain), a dummy variable for the Receiver's belief (P1expfoll) which takes the value of 1 if the Sender expects the Receiver to follow the Sender's message, and a dummy variable for gender (Female) that equals 1 if Sender is female and 0 otherwise. I also included age. The results of the estimation are reported in Table 6.

Finding 16: *Receivers are more likely to learn about the payoffs in In EndoInfo-Free and EndoInfo-Gain than in EndoInfo-Costly.*

The coefficients for EndoInfo-Free and EndoInfo-Gain were positive and significant. Receivers in EndoInfo-Costly are less likely to learn about the payoffs than in EndoInfo-Free and EndoInfo-Gain. The probability to learn about the payoffs increased by 61.5% and 47% in EndoInfo-Free and EndoInfo-Gain, respectively, while the other variables are at their means. This is partially consistent with Hypothesis 3. Therefore, the cost of information affects the decision to learn the payoffs.

In a post-experimental questionnaire, I asked the Receivers to explain why they had chosen to learn or not about the payoffs. According to the post-experimental questionnaire, there were three potential reasons for why subjects did not want to learn about the payoffs: fear to find out that he/she had been cheated, trust, and the cost of information.¹² For instance, one subject wrote: 'the information might make me feel robbed if it turns out that the distribution of money wasn't equal'; and another subject wrote: 'If I'd have found out another choice would have given me more money, it would have been upsetting.'

In addition, one subject also stated 'I'm giving player 1 the benefit of the doubt. The money that we earn is not our money to start with. I have nothing to lose by trusting the other person in the room.' Receivers also wrote: 'To find out how player 1 had chosen'; another subject wrote: 'I was curious to see if I was ripped off or not. I wanted to see how honest my partner was'. Finally, one subject said: 'I was intrigued, can't trust anyone!'

¹² Subject's statements were not edited. Additional answers are available upon request.

Table 6. Determinants of learning about the payoffs

Independent variables	Model V	
	Receiver learns about the payoffs (P2info)	
	Coefficient	Marginal effects
EndoInfo-Free	2.472*** (0.528)	0.615***
EndoInfo-Gain	1.910** (0.565)	0.470***
Receiver expects the truth (P2exptruth)	0.078 (0.408)	0.028
Receiver follows message (P2foll)	-0.165 (0.487)	-0.056
Female	-0.165 (0.431)	-0.058
Age	0.011 (0.131)	0.004
Constant	-0.906 (2.520)	
N° Observations		52
Prob > χ^2		0.0001
Pseudo R-Square		0.4278
Log-likelihood		-19.5334

Note: The dependent variable was a dummy variable (P2info = 1 if the Receiver gets ex-post information about the payoffs, P2info = 0 otherwise). Robust standard errors in parentheses. Marginal effects evaluated at the mean values for the independent variables. *** p < 0.01, ** p < 0.05, * and p < 0.1.

5.3.4. Receiver's belief about the truthfulness of the Sender's message

To investigate further the factors affecting the Receiver's beliefs about the truthfulness of the Sender's message, I estimated Model VI. The dependent variable is a dummy variable (P2exptruth) which takes the value 1 if the Receiver believes the Sender tells the truth, 0 otherwise. The independent variables are the dummies treatment variables, gender, and age. The results are reported in Table 7.

Finding 17: *Receivers are more likely to expect the Sender to tell the true in ExoInfo than in NoInfo.*

The table shows that Receiver was more likely to expect the Sender to tell the truth in ExoInfo than in NoInfo. This finding suggests that the Receiver anticipates a decrease in deceptive messages due to ex-post information. Receivers are 18% more likely to expect a true message.

Table 7. Determinants of Receiver's beliefs about the Sender's message

Independent variables	Model VI	
	Receiver expects the truth (P2exptruth)	
	Coefficient	Marginal effects
ExoInfo	0.768* (0.443)	0.180**
EndoInfo-Costly	0.818* (0.441)	0.189**
EndoInfo-Free	0.563 (0.430)	0.138
EndoInfo-Gain	0.773 (0.512)	0.172**
Female	-0.342 (0.209)	-0.099
Age	-0.043 (0.067)	-0.012
Constant	1.284 (1.438)	
N° Observations		94
Prob > χ^2		0.3044
Pseudo R-Square		0.0746
Log-likelihood		-46.2060

Note: The dependent variable was a dummy variable Receiver's belief about message (P2exptr = 1 if the Receiver expects Sender's message will be true, P2exptr = 0 otherwise). Robust standard errors in parentheses. Marginal effects evaluated at the mean values for the independent variables. *significant at 1%, ** significant at 5%, and *** significant at 10%.

5.3.5. Sender's belief about Receiver's decision to learn about the payoffs

To investigate the factors that affect Sender's beliefs about whether or not the Receiver will learn about the payoffs, I estimated Model VII. The dependent variable was P1exinfo, which takes the value of 1 if player 1 expects the Receiver to learn about the payoffs.

The independent variables included the treatment variables EndoInfo-Free and EndoInfo-Gain, and Sender's characteristics: a gender dummy variable and age. The sample consisted of 52 observations. Table 8 reports the coefficients, standard errors, and the marginal effects.

Finding 18: *Senders are more likely to believe the Receiver will learn about the payoffs in EndoInfo-Free and EndoInfo-Gain than in EndoInfo-Costly.*

The coefficients for EndoInfo-Free and EndoInfo-Gain were positive and significant. Thus, Senders are more likely to expect the Receiver to learn about the payoffs in

EndoInfo-Free than in EndoInfo-Costly, and similarly in EndoInfo-Gain than in EndoInfo-Costly. The probability increased by 51.5% in EndoInfo-Free and 27.3% in EndoInfo-Gain. This result confirms the Hypothesis 7.

Table 8. Determinants of Sender's belief about the Receiver's information decision

Independent variables	Model VII Sender expects the Receiver to learn about the payoffs (P1expinfo)	
	Coefficient	Marginal effects
EndoInfo-Free	1.953*** (0.571)	0.515***
EndoInfo-Gain	0.935** (0.471)	0.273**
Female	0.043 (0.400)	0.015
Age	-0.060 (0.086)	-0.021
Constant	0.828 (1.797)	
N° Observations		52
Prob > χ^2		0.0072
Pseudo R-Square		0.2550
Log-likelihood		-24.9878

Note: The dependent variable was a dummy variable (P1expinfo = 1 if the Sender expects Receiver gets information about the payoffs, P1expinf = 0, otherwise). robust standard errors in parentheses. Marginal effects evaluated at the mean values for the independent variables. *** p < 0.01, ** p < 0.05, * p < 0.1.

5.3.6. Correlation between the Receiver's decision to get ex-post information about the payoffs and the Receiver's decision to follow the Sender's message

It is possible that the Receiver's decision to follow the message or not may be influenced by his/her planned decision to be informed ex post or not. If the Receiver has already decided to be informed (say because it is free or because he/she actually be paid), then this could impact on her decision to follow the Sender's message or not.

If the Receiver has decided to be informed, and he/she assigns substantial weight to not finding out she was lied to, then she may prefer not to follow the message. If so, then the decision to follow is influenced by the information decision. Both the information and the decision to follow are then simultaneously determined. To address this potential endogeneity problem, I ran a bivariate probit model (Model VIII) and I estimated the joint probability of these two binary variables.

In Model VIII, the dependent variables were the Receiver follows the Sender's message (p2foll) and the Receiver gets ex-post information (p2info). The independent variables were treatment dummies, gender dummy, and a p2exptruth dummy, and age. The analysis is based on a sample of 52 observations. Table 9 shows the results of the estimated model.

Finding 19: *The Receiver's decision to follow the Sender's message is not correlated to the Receiver's decision to get ex-post information about the payoffs.*

The estimated correlation coefficient between the bivariate outcomes was negative, but not statistically significantly different from zero ($\rho = -0.145$, $p\text{-value}=0.802$), indicating that the unobserved factors affecting the Receiver's decision to follow the Sender's message and the decision to get information about the payoffs are not correlated. This result confirms that the two choices, the Receiver's decision to follow the Sender's message and the Receiver's decision to get ex post information about the payoffs are not jointly determined.

Furthermore, the Receiver's expectations about the truthfulness of the Sender's message does not have a statistically significant effect on the probability that the Receiver learns about the payoffs, but it does have a statistically significant and positive effect on the probability that the Receiver follows the Sender's message. The marginal effect of the joint probabilities indicates that, *ceteris paribus* and evaluated at mean values for the independent variables, the probability that Receiver expects the Sender's message is true and the probability that the Receiver follow the Sender's message increases by 34.4% if the Receiver expects the Sender's message is true.

In contrast, EndoInfo-Costly does not have a statistically significant effect on the probability that the Receiver follows the Sender's message, but it does have a statistically significant and negative effect on the probability that the Receiver gets ex post information about the payoffs. The marginal effect of the joint probabilities indicates that, *ceteris paribus* and evaluated at mean values for the independent variables, the joint probability that the Receiver follows the Sender's message and that the Receiver expects the Sender's message is true decreases by 71.3% in the EndoInfo-Costly treatment.

Table 9. Bivariate probit model

Independent Variables	Model VIII		
	Receiver's information decision	Receiver's option decision	Marginal effect on joint probabilities
EndoInfo-Free	2.475*** (0.529)	-0.324 (0.595)	0.555***
EndoInfo-Gain	1.907*** (0.562)	-0.119 (0.563)	0.437***
P2 expects P1 to tell the truth	0.004 (0.374)	1.916*** (0.614)	0.344**
Female	-0.160 (0.432)	-0.142 (0.508)	-0.065
Age	0.003 (0.121)	0.381** (0.187)	0.034
Constant	-0.843 (2.427)	-7.410** (3.711)	
ρ (1,2)	-0.145 (0.286)		
N° Observations	52		
Log-likelihood	-31.9748		
Likelihood-ratio test of ρ	0		
$\chi^2(1)$	0.261		
p-value	0.610		

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * and $p < 0.1$. Marginal effects evaluated at the mean values for the independent variables.

5.4. Receivers' messages

There is a large body of experimental evidence, which indicates that reciprocity is a powerful determinant of human behaviour. For instance, Fehr and Gächter (1998), Brandts and Charness (2000), and Falk and Fischbacher (2006) showed that people are

willing to sacrifice their own payoff to increase/decrease the other's payoff for the kind/unkind actions.¹³

In this section, I investigate whether Receivers are willing to punish or reward the Sender's actions by sending a harsh/kind message back to the Sender. Looking at the content of the messages, I identified up to twenty different categories: 1 = *Thankful*, 2 = *Acknowledge*, 3 = *Fairness*, 4 = *Trust*, 5 = *Sarcasm*, 6 = *Anger*, 7 = *Apology*, 8 = *Humor*, 9 = *Confused*, 10 = *Hope*, 11 = *Warning*, 12 = *In doubt*, 13 = *Off-topic*, 14 = *Slang*, 15 = *You'd do the same*, and 16 = *No message*. Table 10 contains a list of these categories, a description of each category, and an example of each category.¹⁴ Table 26 in Appendix I. Messages provides all unedited messages from ExoInfo treatment (session 1), alongside the Receivers' chosen option and the messages classification.

A message can fall into one or more than one category. Notice that when classifying the Receiver's messages; I took into account not only the content of the Receiver's message, but also the message sent by the Sender, and the Receiver's chosen option. For instance, if the Receiver received message 2 and chose option B and the message say 'enjoy the money', I classified this message as a sarcasm.

¹³ Anderson and Putterman (2006) found significant levels of non-strategic sanctions in public goods.

¹⁴ The complete Receivers' messages unedited for each treatment along with the classification of the messages are available from the author upon request. Note that NoInfo did not allow the Receiver to send a message back to the Sender.

Table 10. Messages classification

Category	Label	Description	Example
1	Thankful	The message expressed gratitude or joy	'thanks'
2	Acknowledge	The message recognised the fact that the Sender has told the truth or has lied	'well done', 'no cool man'
3	Fairness	The message referred to fairness	'thank you for choosing the fair option'
4	Trust	The message referred to trust	'I'm obviously not a very trusting person'
5	Sarcasm	The message was sent to mean the opposite	'Enjoy your £14'
6	Anger	The Receiver was angry or upset	'so annoying feel like an idiot now'
7	Apologised	The Receiver is sorry for not following the chosen message	'apologies again for not trusting your original'
8	Humour	The Receiver maked jokes	'faith in humanity restored :)'
9	Confused	The Receiver may expect more rounds	'i dont know if theres anymore rounds'
10	Hope	The Receiver expects the her/his choice be good for both players	'hope it works out in both of our favour'
11	Warning	The Receivers warned the Sender if he decides to lie	'you might get more on one turn, but that would force me to change my decision'
12	In doubt	The Receiver doubts about the Sender's intention	'Thank you for being honest! At least I think you were'
13	Off-topic	The message was related to weather, food, etc.	'have a lovely day!'
14	Slang	The messages included slangs	'If you pick option A I will send you some dank memes'
15	You'd do the same	The Receiver thought he/she would choose the same message as the Sender if he/she were the Sender	'I will be honest too'
16	No message	Empty message	

Note: All messages were unedited

Figure 7 shows the distribution of messages by category. The more frequent category was thankful (20.61% of the cases), whereas the less frequent category was an apology (1.53%). Receiver apologised for not trusting or not following the chosen message. Only in 2.29% of the cases, Receivers thought that they would send the same message as the Sender did, and in 8.40% of the cases, Receivers did not send any message. Likewise, in 2.29% of the cases, Receivers sent a message expressing anger, and in 8.40% of the cases, Receivers wrote a message indicating sarcasm.

It is noteworthy that in 4.58% of the time, Receivers were not sure about the Sender's intention. In order words, they were aware of the possibility of meeting a 'sophisticated

liar' (Sutter, 2009). This finding suggest that some Receivers believed that the Sender behaved strategically. For instance, one subject wrote:

'I'm in an overly cynical mood today so I thought you'd be lying in order to get more money, but it turns out you were giving me the better option. Or maybe you were double-bluffing? Either way, enjoy your money, I'm off to the shop to buy a sandwich.'

Figure 8 shows the distribution of messages across treatments, regardless of whether the Receiver was informed about the payoffs or not. Receivers were more thankful in ExoInfo (23.53% of the time) and EndoInfo-Free (27.27% of the time), than in EndoInfo-Costly (17.5% of the time) and EndoInfo-Gain (12.5% of the time).

Receivers acknowledged the Sender's action, either by praising the Sender for telling the truth or reproaching the Sender for telling a lie in ExoInfo, 11.76% of the time. Receivers also hoped that the chosen option benefited both in 17.5% of the time in EndoInfo-Costly. A non-parametric test showed that there were not statistically significant differences between treatments, $\chi^2(45) = 54.07$, $p = 0.167$. However, there was significant difference between EndoInfo-Costly and EndoInfo-Free (Fisher's exact test, $p = 0.086$).

Figure 7. Distribution of Receivers' messages

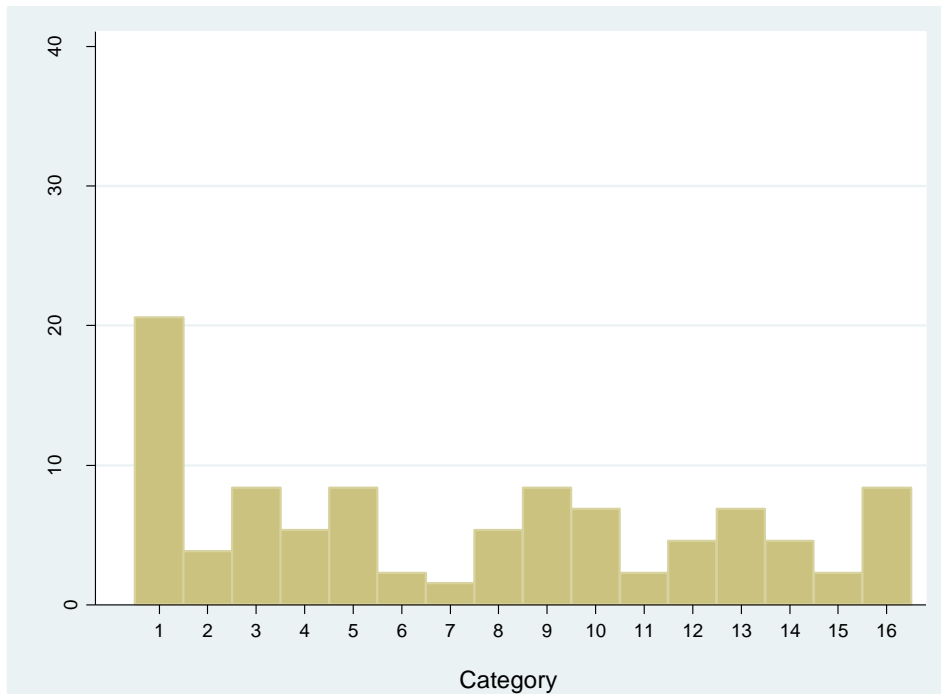


Figure 8. Distribution of Receivers' messages across treatments

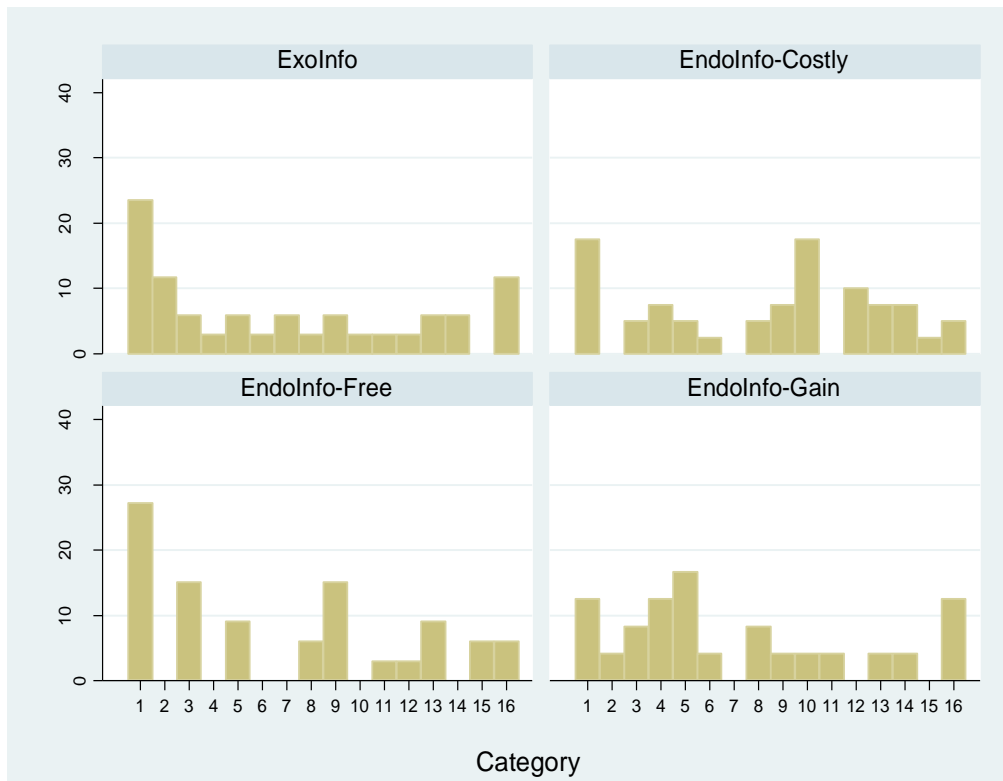


Figure 9 shows the distribution of messages by whether the Receiver learned information about the payoffs. When the Receiver learnt about the payoffs, the more frequent category was thankful (22.41% of the time), whereas it was hope (20.51% of the time) when the Receiver did not learn the payoffs. In 1.53% of the cases, Receivers apologised for not trusting the Sender or not following the chosen message. A non-parametric test showed that there were statistically significant differences across distributions (Fisher's exact test, $p = 0.032$).

Figure 9. Distribution of Receivers' messages across information decision



I also investigated whether Receivers who learnt the payoffs reacted differently if they knew they have been told a lie or not. Figure 10 shows the distribution of messages across the decision to learn about the payoffs and by message. When the Receiver learnt about the payoffs and the Sender sent Message 1, more messages were thankful (33.3% of the time).

However, when the Receiver learnt about the payoffs and the Sender sent Message 2, more messages were sarcasm (31.6% of the time). When the Receiver did not learn about

the payoffs, they sent more often hope messages. A non-parametric test showed that there were statistically significant differences across distributions when the Receiver learnt about the payoffs (Fisher's exact test, $p = 0.000$), but there were not when the Receiver did not learn about the payoffs (Fisher's exact test, $p = 0.320$).

Figure 11 compares the distributions of Receivers' messages by Sender's message. The figure shows an interesting contrast. When Receivers received Message 1, they are more likely to send a thankful message, and when Receivers received Message 2 they are more likely to send a sarcastic message. A non-parametric test showed that there was not a significant difference between distributions (Fisher's exact test, $p = 0.001$). Therefore, emotions were related to the type of messages.

Figure 12 shows the distributions of Receivers' messages across Senders' message and Receivers' chosen option. When Receivers received Message 1 and chose option A, they sent back more often a thankful message. However, when Receivers received Message 1 and chose option B, they sent back more often a sarcastic or off-topic message. A non-parametric test showed that there was a significant difference between distributions (Fisher's exact test, $p = 0.005$).

On the other hand, when Receivers received Message 2 and chose option A, they more often sent a message referring to fairness. Likewise, when they received Message 2 and chose option B, Receivers more often sent a message referring to fairness or inequality (33.3% of the time). A non-parametric test showed that there was not a significant difference between distributions (Fisher's exact test, $p = 0.481$)

Figure 10. Distribution of Receivers' messages across information decision and Sender's message

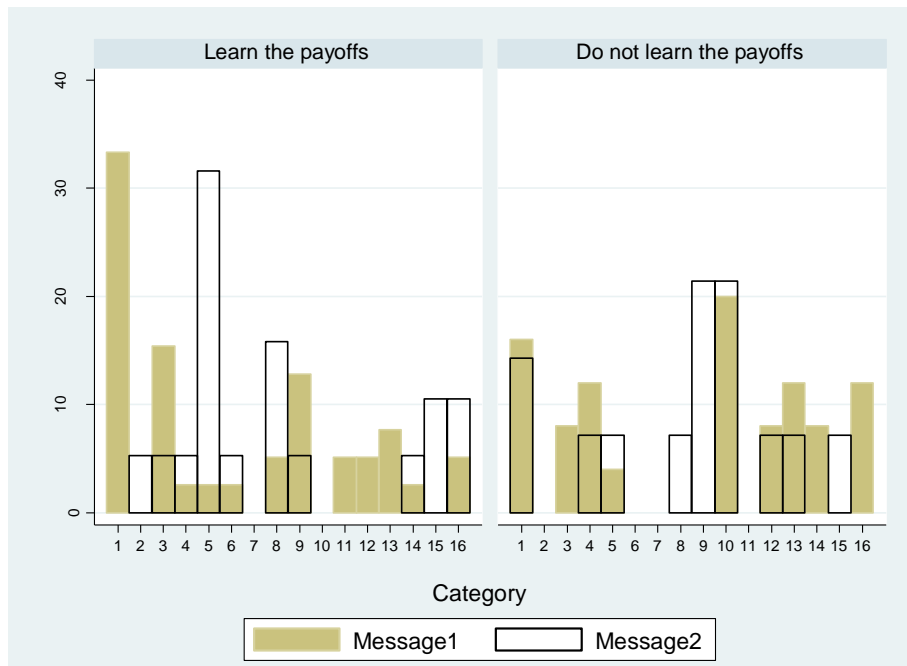


Figure 11. Distribution of Receivers' message across Sender's message

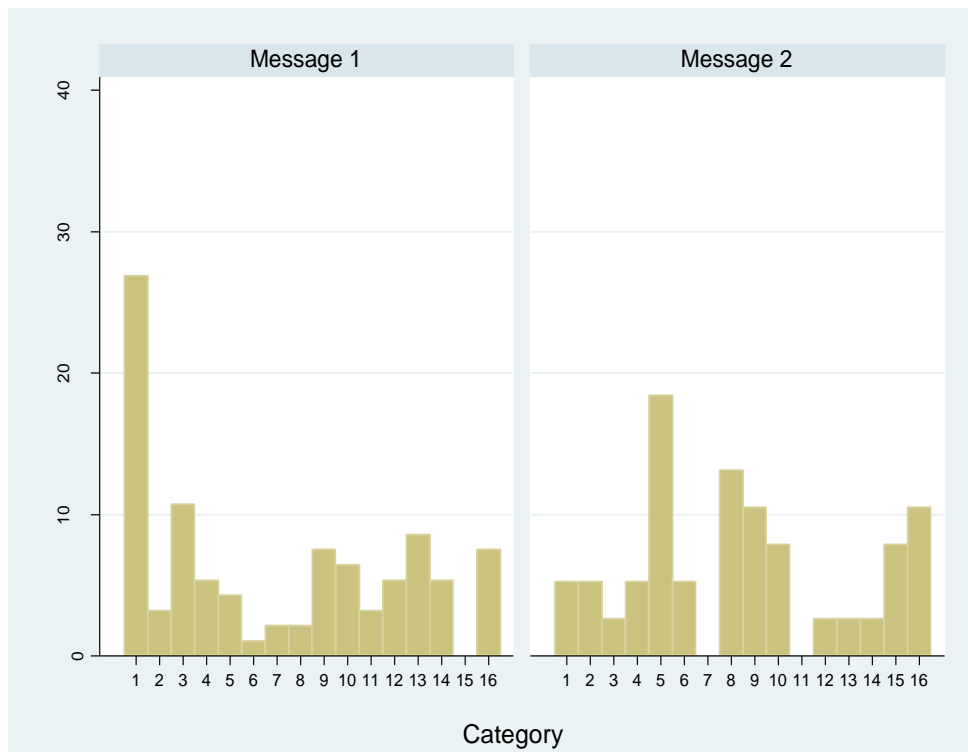
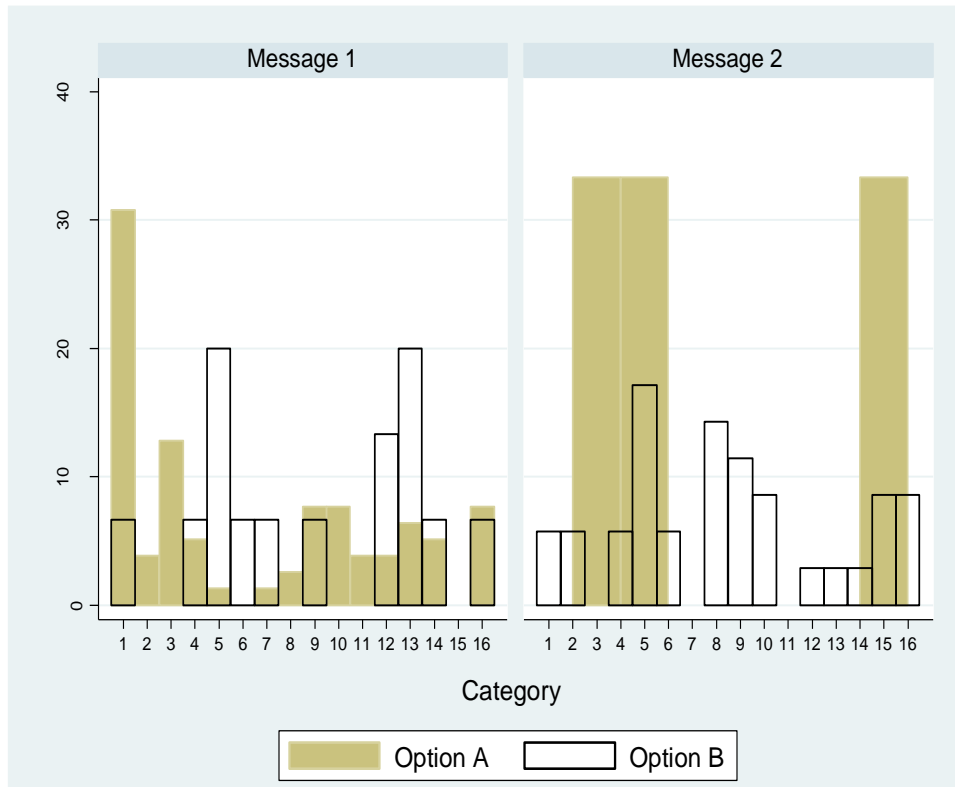


Figure 12. Distribution of Receivers' messages across message and chosen option



In summary, there are four important findings from the analysis of the Receivers' messages in this section. First, Receivers are more likely to send a thankful message than to get angry with the Sender. Second, there is significant difference if the Receiver learnt the payoffs and realised that he/she received a false message than if he/she received an honest message. When the message was false, Receivers are more likely to send a sarcastic message. However, when the message was true, Receivers are more likely to say thank you. These findings appear to be consistent with the idea that people care about intentions. This analysis is new, but the evidence is consistent with the evidence in Xiao and Houser (2009). The authors showed that people are willing to make non-monetary punishment (Xiao and Houser, 2009).

6. Free-form messages

Previous experimental research has given considerable attention to deception using fixed messages where the Sender chooses a message from a pre-established menu of

messages (Gneezy, 2005; Sutter, 2009; Erat and Gneezy, 2012). Thus, the Sender has only fixed options, for example, to lie or tell the truth. These papers observed a substantial fraction of true messages.

In many real world situations, however, communication is not typically restricted to pre-established menu of messages, people can freely decide what to write or say. Consider for example the expansion of internet that has increased the amount of textual communication received by individuals and organizations in the real world. People receive messages by Facebook, e-mail, and mobile phone.

In this section, I move beyond the fixed messages and experimentally explore preferences for truth telling in a more natural environment. I ask the following questions: How many Senders lie in this case? Will they tell the truth or a straight lie? Will they send ambiguous messages that are not lies, but still intended to make the other person make the choice that the message Senders would like? How does the possibility of sending free text messages affect the Receiver's behaviour compared with her behaviour in the fixed messages?

Some laboratory experiments have allowed pre play communication, but they do not examine what people actually write. For example, Lundquist et al (2009) studied free form communication in a bargaining game.¹⁵ They showed that pre-play communication lead to the few lies and the most efficient outcomes. Charness and Dufwenberg (2006) studied the effects of communication with a hidden-action in a trust game. They also found that communication led to better social outcomes.¹⁶

In this section, I ran two additional treatments (FreeMess-NoInfo and FreeMess-EndoInfo-Costly) where, as in the previous fixed message treatments, subjects play a

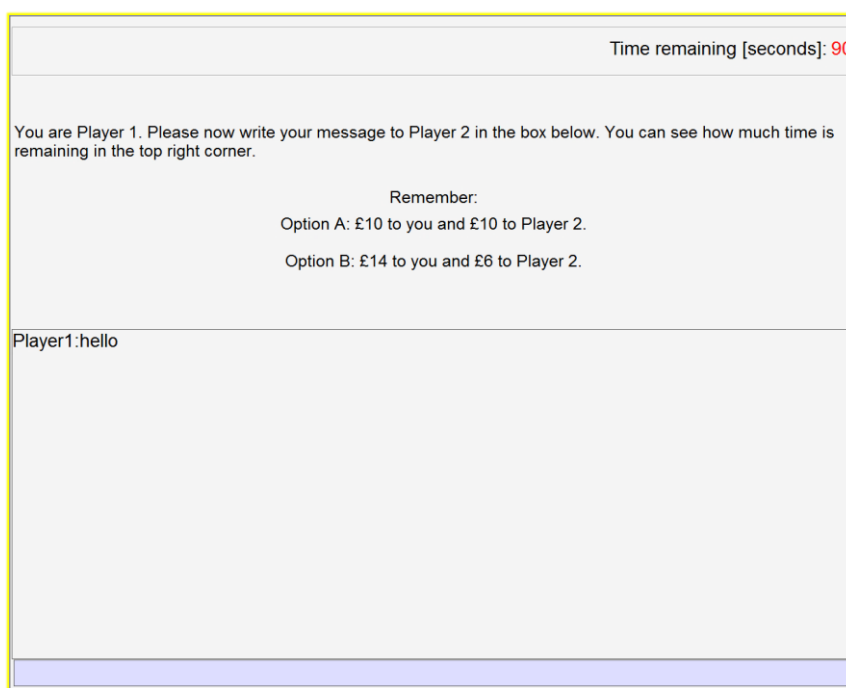
¹⁵ For instance, it is well established that non-binding pre-play verbal communication raises cooperation in public good-type settings (Ledyard 1995; Sally, 1995)

¹⁶ Wang et al (2010) used eye tracking on a Sender-Receiver game to understand deception. The authors found that Senders do not pay much attention to Receiver's payoffs compared to their own payoffs, but the Sender's pupils dilated when they send deceptive messages, and dilated more when the deception is larger in magnitude.

Sender Receiver game, but instead of restricting Senders to choose from a given set of messages, the Sender had a maximum of 120 seconds to write a free-text message and send it to the Receiver. She can also write nothing. However, she is not allowed to reveal her identity nor threaten the Receiver with anything that could occur after the experiment.

Figure 13 shows a screenshot of the task faced by the Sender for FreeMess-NoInfo and FreeMess-EndoInfo-Costly. The Sender wrote their message in the text box at the bottom of the screen. Once the Sender sent his message, the message was recorded. When the Sender finished writing the message, they appeared on the Sender's screen. Thus, the Sender can read the messages until the time runs out. A scrollbar emerges if the messages were very long.

Figure 13. Sender's decision screen for free-text message treatments

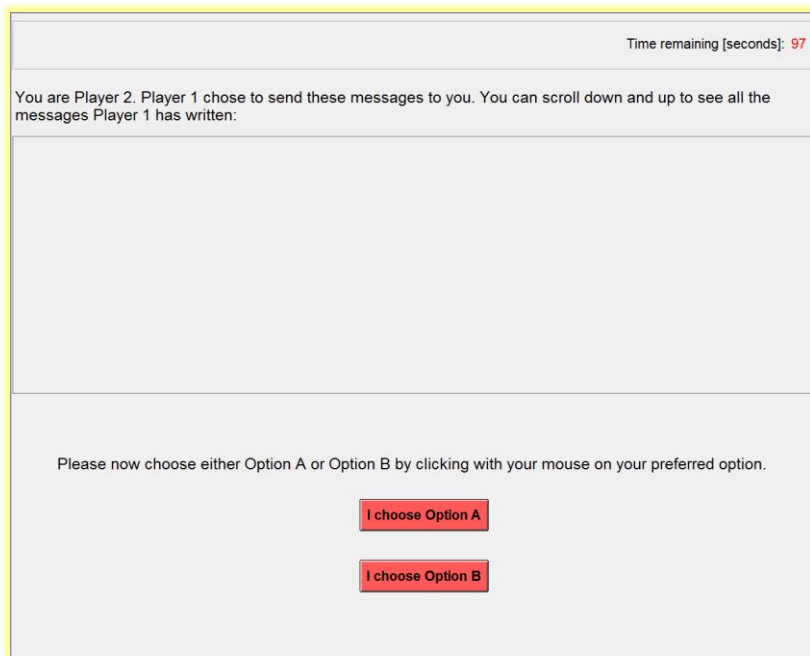


In FreeMess-NoInfo, the Receiver after choosing between option A and B, the Receiver learns only about his own payoff and terminates the game. In FreeMess-EndoInfo-Costly, the Receiver, after choosing between options A and B, decides whether she wants to learn about the payoffs. If she decides to learn, she pays 60 pence. Note that the structure of the games and the payoffs remained essentially the same as for the fixed

message treatments. In this sense, the present experiment also provides a test of the robustness of the results presented in EndoInfo-Costly.

Figure 14 shows a screenshot of the task faced by the Receiver for FreeMess-NoInfo and FreeMess-EndoInfo-Costly. There were two buttons, one button reading, “I choose option A” and another button reading, “I choose option B.” The Receiver made her decision by clicking on one of the two buttons. Note that the Receiver can read the messages sent by the Sender before making her decision.

Figure 14. Receiver’s decision screen for free-text message treatments



A sample of the experimental instructions for FreeMess-NoInfo and FreeMess-EndoInfo-Costly is included in Appendix B. Experimental instructions for free messages of this chapter, respectively. I ran 2 sessions for each treatment. The experiment was conducted between May and June 2017 at the Centre for Behavioural and Experimental Social Science (CBESS) computerized laboratory at the University of East. A total of 24 sender-receiver pairs participated in treatment 1 and 18 sender-receiver pairs participated in treatment 2. None of the subjects participated in more than one session.

The experimental protocol in each session was exactly the same as for NoInfo and EndoInfo-Costly, respectively. Sessions were also run at the Arts Laboratory of the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia (UEA) in order to guarantee the same local environment for the individuals. Sessions were conducted and programmed using z-TREE (Fischbacher, 2007).

The average age was approximately 20.14 years (S.D. 2.03, min. 18, max. 31), 48.40% of the subjects were female, 87.77% were from United Kingdom, and 25% studied Economics and Business management. Details of the participants' socio-economic background are available in Appendix D. Background of the participants in free messages.

6.1. Classification

When Sender can freely write a message, there is something of a spectrum of messages. Messages are classified in thirteen groups: 1 = *All payoffs true*, 2 = *One payoff true*, 3 = *All payoffs false*, 4 = *One payoff false*, 5 = *Partial true*, 6 = *Ambiguous message*, 7 = *Direct recommendation*, 8 = *Indirect recommendation*, 9 = *Confused*, 10 = *Off-topic*, 11 = *It's up to you*, 12 = *Fairness*, and 13 = *No message*.¹⁷ Table 11 lists all the categories and provide a definition of each category and an example. All messages were classified as belonging to one or several categories.¹⁸

¹⁷ In the analysis of written messages, some studies use a computer-based text analysis program to classify liars and truth-tellers. For instance, Gneezy (2005) and Erat, and Gneezy (2012) only classified lies.

¹⁸ Table 27 in Appendix I. **Messages** shows all the unedited messages from FreeMessage-NoInfo treatment, session 1, along with the chosen option and the messages classification. The complete Senders' messages unedited for each treatment are available from the author on request.

Table 11. Message classification

Category	Label	Description	Example
1	All payoffs true	The Sender deliberately makes a true statement about all the payoffs.	'Option A = £10 each, option B = £14 to me and £6 to you'
2	One payoff true	The Sender makes a true statement about one of the payoffs.	'Option A is £10 each and option B is £14 to player 2 and £6 to player 1'
3	All payoffs false	The Sender deliberately makes a false statement about all the payoffs.	'Option A I get £14 and you get £6, Option B I both get £10.'
4	One payoff false	The Sender makes a false statement about one of the payoffs.	'option B will be the option that is fairer to the both of us, we will equally benefit! choose wisely '
5	Partial true	The Sender makes a true statement about all the payoffs, but deliberately omits some information.	' if you choose option A we will get an equal amount, and if you choose option B we get different amounts!'
6	Ambiguous message	The Sender makes a subjective statement about the payoffs.	'option b may be slightly better'
7	Direct recommendation	The Sender makes a clear recommendation.	' go for option A
8	Indirect recommendation	The Sender makes an ambiguous recommendation.	'choose wisely'
9	Confused	The Sender thinks that he/she will choose the option	'I am going to choose the option that gives us the same amount of money'
10	Off-topic	The Sender talks about holidays, weather, mood, etc	'Have a lovely day :)'
11	It's up to you	The Sender leaves the decision to the Receiver.	'option A is fair but its up to you'
12	Fairness	The Sender makes a statement based on arguments about fairness, equality, efficiency	'go for option A i think it would be fairer'
13	No message	No written message	

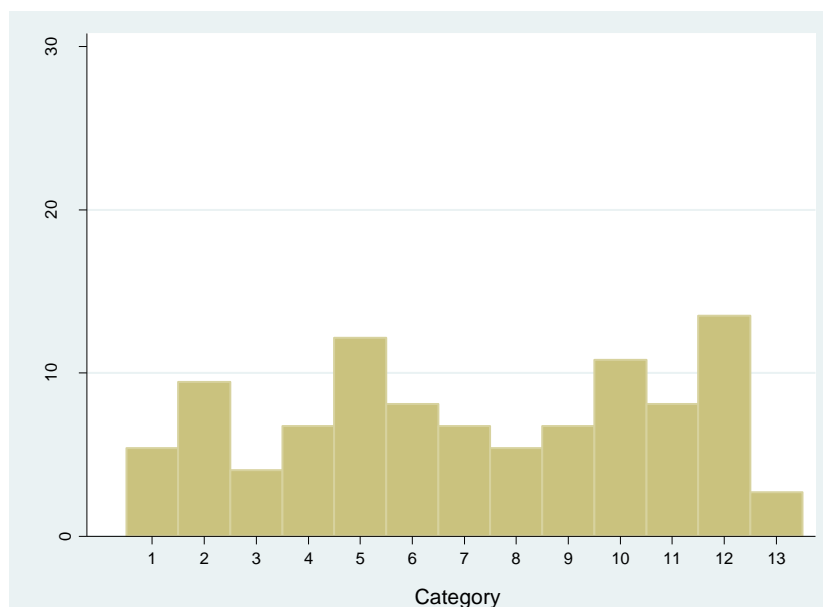
Note: All messages were unedited

6.2. Senders' messages

Figure 15 shows a histogram of the Senders' messages. More messages were partial truth (where relevant information was omitted) and made statements about fairness. Certainly, Senders do not tell very often outright lies about all the payoffs. These messages were only 4% of the total messages.

The figure also reveals that Senders do not make a statement only about the Receiver's payoff: 11% of the messages avoided sending a message about the payoff and talk about weather or mood, and 8% of the messages directly delegated the responsibility of the final payoff on the Receiver (category 11). I also found that 8% of the times, Senders sent an ambiguous message (category 6). Differently from previous Sender-Receiver games with fixed messages, the Sender usually makes statement where she compares her own payoff and the Receiver's payoff. For example, one subject wrote: "so option B is the worse of one for you that makes me better off."

Figure 15. Distribution of Senders' messages by category (n=74)



An exploratory analysis of the content of the Sender's message for treatments shows that Senders are more likely to send a truth message about the payoffs in FreeMess-EndoInfo-

Costly (9.4% of the time) than in FreeMess-NoInfo (2.4% of the time). Figure 16 shows the distribution of messages across treatments. It shows that Senders avoid telling the truth about all the payoffs in FreeMess-NoInfo, when the payoffs cannot be disclosed.

Few Senders tell a direct lie in FreeMess-EndoInfo-Costly, when information can be verified. Instead, they tell a partial truth or send a message that avoids telling about the payoffs, an off-topic message. Additionally, in the FreeMess-EndoInfo-Costly, Senders never send a direct recommendation message. The difference in the distribution of messages is not statistically significant. (Fisher's exact test, $p = 0.377$). Therefore, Senders' messages are not affected by whether the Receiver can learn about the payoffs.

I also investigated whether the Sender's message differed by the Receiver's option. Figure 17 shows the distribution of messages across treatments by chosen option. I found that the distribution of Sender's messages was also similar in FreeMess-NoInfo and FreeMess-EndoInfo-Costly if the Receiver chose Option A (Fisher exact test, p -value = 0.595). Likewise, the distribution of Sender's messages was also similar in FreeMess-NoInfo and FreeMess-EndoInfo-Costly if the Receiver chose Option B (Fisher exact test, p -value = 0.598).

Figure 16. Distribution of Senders' messages for FreeMess-NoInfo (n=41) and FreeMess-EndoInfo-Costly (n=33)

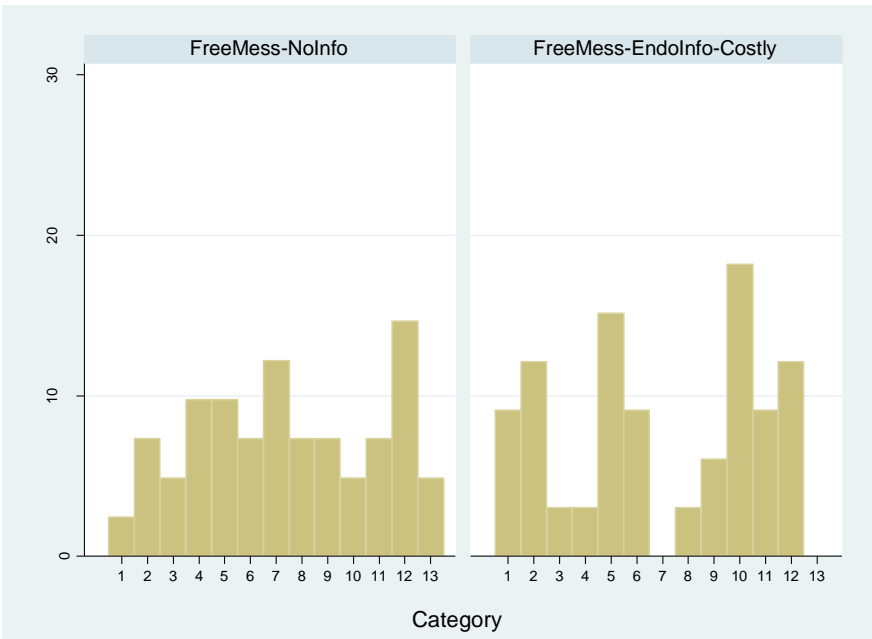
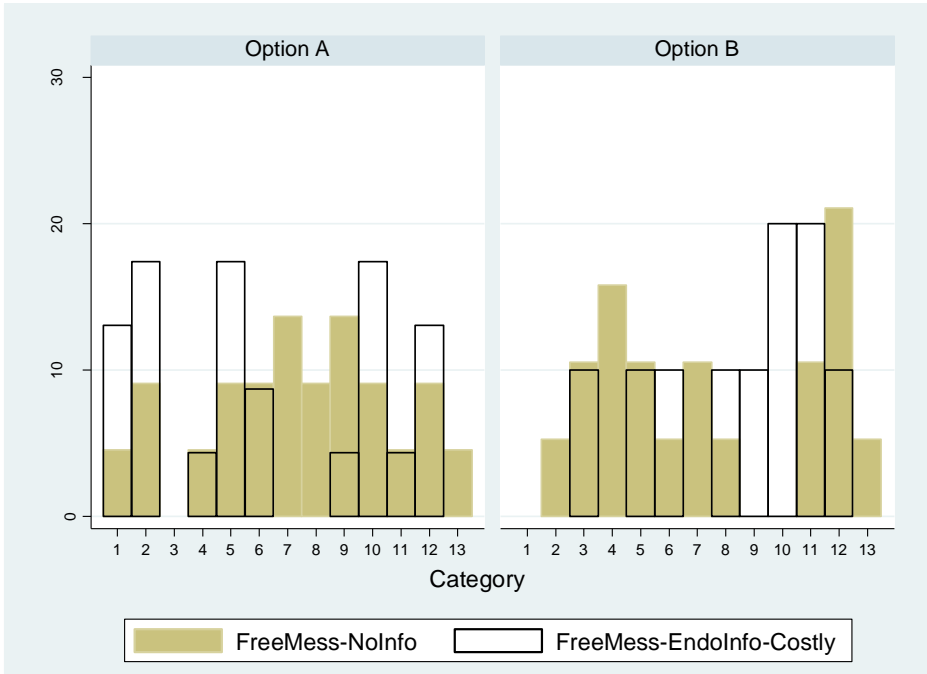


Figure 17. Distribution of Senders' messages across treatments in Option A (n=45) and option B (n=29)



6.3. Receivers' behaviour

Table 12 compares Receivers' behaviour in the fixed and free message treatments. It shows that 55% of Receivers chose option B in NoInfo and 42% in FreeMess-NoInfo. The difference in these proportions is not statistically significant (1-sided Fisher's exact, $p = 0.282$). The table also reveals that 40% of Receivers chose option B in EndoInfo-Costly and 28% in FreeMess-EndoInfo-Costly. The difference in these proportions is not statistically significant (1-sided Fisher's exact, $p=0.327$).

Moreover, 20% of Receivers learnt about the payoffs in EndoInfo-Costly and 6% learnt about the payoffs in FreeMess-EndoInfo-Costly. However, the difference in these proportions was not statistically significant (1-sided Fisher's exact, $p=0.205$). Therefore, the data suggests that receiving a free text message does not affect the Receiver's behaviour.

6.4. Senders' expectations

Table 12 also compares Sender's behaviour in the fixed and free message treatments. It shows that 41% of Senders expect the Receiver to choose option B in NoInfo and 46% in FreeMess-NoInfo. The difference in these proportions is not statistically significant (1-sided Fisher's exact, $p = 0.485$).

The table also reveals that 40% of Senders expect the Receiver to choose option B in EndoInfo-Costly and 22% in FreeMess-EndoInfo-Costly. The difference in these proportions is not statistically significant (1-sided Fisher's exact, $p=0.859$). In addition, 35% of Senders expect the Receiver to learn about the payoffs in EndoInfo-Costly and 33% in FreeMess-EndoInfo-Costly. However, the difference in these proportions was not statistically significant (1-sided Fisher's exact, $p=0.593$). Therefore, the data suggests that allowing Senders to write a free text message does not affect the Sender's behaviour.

Table 12. Comparison of fixed and free message treatments

Treatment	Proportion of Receivers choosing option B	Proportion of Receivers learning about the payoffs	Proportion of Senders expecting the Receiver to choose option B	Proportion of Senders expecting the Receiver to learn about the payoffs
NoInfo	12/22 (55%)		9/22 (41%)	
FreeMess-NoInfo	10/24 (42%)		11/24 (46%)	
EndoInfo-Costly	8/20 (40%)	4/20 (20%)	4/20 (40%)	7/20 (35%)
FreeMess-EndoInfo-Costly	5/18 (28%)	1/18 (6%)	4/18 (22%)	6/18 (33%)

7. Discussion

The experimental results in this chapter confirm that more Senders are willing to tell the truth. This is consistent with previous Sender-Receiver games who report more truth telling than predicted by standard Bayesian Nash equilibrium (Gneezy 2005, Cai and Wang, 2006; Sánchez-Pagés and Vorsatz, 2007).

Moreover, the data show that deception decreases automatic ex-post information about the payoffs decreased frequency of lying relative to the Baseline. A regression analysis confirms that the probability to lie decrease by 20% in ExoInfo (automatic disclose of information) compared to NoInfo (no information about the payoffs).

Clearly, ex-post information about the payoffs is of great importance for deceptive behaviour. The evidence from this study is not all consistent with lying aversion. Subjects who tell the truth cannot be explained with reference to lying aversion alone. However, the decrease in lying in this experiment may be explained in terms of guilt and shame aversion. The evidence here is actually consistent with that presented by Behn et al (2014). The authors showed that costless ex post information about the payoffs decreases lying.

Another important finding is that Receivers are willing to learn about the payoffs. Moreover, a significant minority learns about the payoffs even though it is costly to themselves. This finding is not consistent with the usual standard economic theory, which predicts that a rational subject should never pay a positive sum of money for non-instrumental information. The standard economic theory is that information is deemed

valuable if, and only if, it is instrumental for decision-maker. The data is not consistent with this view.

There are several reasons why Receivers get ex-post information about the payoffs. One might be curiosity. Golman and Loewenstein (2015) define curiosity as the 'desire for knowledge for its own sake, even in the absence of material benefits' The Receiver would like to find out what he could have gotten if he had made a different choice, and what the other player got. The psychology literature has identified that higher levels of curiosity are associated with higher demand for information (see Loewenstein (1994) for a review of the literature). Another explanation might be that the Receiver wants to reciprocate or has a particular desire to verbally punish the Sender by sending a harsh message if the latter has lied. In an interesting study, Fels (2015) showed how the anticipation of the emotional impact of information seems to be an important factor determining attitudes toward information.

An analysis of the Receiver' messages show that most Receivers do not send a harsh message. However, Receivers sent a sarcastic message when they learn the payoffs and know the Sender has lied. Future research may wish to explore the extent to which demand for non-instrumental information holds across other different settings. In addition, one might also want to collect data to distinguish between curiosity and punishment hypotheses. For instance, the Receiver can get information, but cannot send a message back to the Sender. According to the curiosity hypothesis, as much information will be collected in this treatment, while the punishment/reward hypothesis predicts that no information will be collected. These can be treatments for further empirical work.

I also extend the Sender-Receiver game beyond the fixed message and run two additional treatments where the Senders can write a free message to the Receiver. I found that Senders are not willing to tell straight lies; instead, they are willing to tell partial truths. These results suggest that fixed message treatments fail to capture the large range of preferences for truth telling. An interesting question for future research would be what will happen if people engage in a conversation (bilateral communication) before the Receiver make his/her decision. How does this affect the propensity to lie?

8. Conclusion

In previous Sender-Receiver games, deception is totally disclosure to the experimenter, but the Receiver does not know about the payoffs. The Receiver never learns about the truth. The principal objective of the research reported in this chapter is to examine experimentally the role of ex-post information about the payoffs in Sender-Receiver games: the Receiver can ex-post, and at a cost, find out if the Sender's messages were true or not.

Previous research have showed that *ex ante* disclosure of conflicts of interest fails to reduce deception (Koch and Schmidt, 2010). I used a simple Sender-Receiver game and conducted five different treatments. In treatment 1, the Receiver never learns about the payoffs. In treatment 2, the Receiver, after choosing between option A and B, automatically learns about the payoffs.

In treatment 3, the Receiver, after choosing between option A and B, decides whether she wants to learn about the payoffs. If the Receiver chooses to learn about the payoff, he/she decreases his/her payoffs by £1. Treatment 4 is identical to treatment 3, but now information is free. Finally, treatment 5 is identical to treatment 3, except that if the Receiver chooses to learn about the payoff, he/she increases his/her payoff by £0.10.

The main results emerging from this study is that providing ex post information, compared to non-information, can significantly reduce the probability to lie. This result is hard to reconcile with Senders being exclusively motivated by lying aversion. The effect of ex post information about the payoffs on deception is consistent with models of guilt and shame aversion.

An interesting finding is that a significant minority of Receivers get ex post information about the payoffs, even though it is costly to themselves. In the current design, there is not strategic reason to get information about the payoffs. Hence, the evidence contradicts the standard assumption that Receivers view information as merely a means for making better decisions, in which case no one should get information about the payoffs.

References

Agne Kajackaite and Uri Gneezy. (2017) Incentives and cheating. *Games and Economic Behavior* 102, 433-444.

Austen-Smith, D. and Riker, W.H., 1987. Asymmetric information and the coherence of legislation. *American Political Science Review*, 81(3), pp.897-918.

Balafoutas, L. and Sutter, M., 2017. On the nature of guilt aversion: Insights from a new methodology in the dictator game. *Journal of Behavioral and Experimental Finance*, 13, pp.9-15.

Bastardi, A. and Shafir, E., 1998. On the pursuit and misuse of useless information. *Journal of personality and social psychology*, 75, pp.19-32.

Battigalli, P., Charness, G. and Dufwenberg, M., 2013. Deception: The role of guilt. *Journal of Economic Behavior & Organization*, 93, pp.227-232.

Behnk, S., Barreda-Tarrazona, I. and García-Gallego, A., 2014. The role of ex post transparency in information transmission – An experiment. *Journal of Economic Behavior & Organization*, 101, pp.45-64.

Brady, M., 2009. Curiosity and the Value of Truth. *Epistemic value*, pp.265-283.

Brandts, J. and Charness, G., 2000. Hot vs. cold: Sequential responses and preference stability in experimental games. *Experimental Economics*, 2(3), pp.227-238.

Brown, J.D. and Dutton, K.A., 1995. Truth and consequences: The costs and benefits of accurate self-knowledge. *Personality and Social Psychology Bulletin*, 21(12), pp.1288-1296.

- Bolton, G.E. and Ockenfels, A., 2000. ERC: A theory of equity, reciprocity, and competition. *American economic review*, 90(1), pp.166-193.
- Brandts, J. and Charness, G., 2003. Truth or consequences: An experiment. *Management Science*, 49(1), pp.116-130.
- Buccioli, A. and Piovesan, M., 2011. Luck or cheating? A field experiment on honesty with children. *Journal of Economic Psychology*, 32(1), pp.73-78.
- Buller, D.B. and Burgoon, J.K., 1994. Deception: Strategic and nonstrategic communication. *Strategic interpersonal communication*, pp.191-223.
- Burgoon, J., Blair, J., Qin, T. and Nunamaker, J., 2003. Detecting deception through linguistic analysis. *Intelligence and Security Informatics*, pp.958-958.
- Cantalalops, A.S. and Salvi, F., 2014. New consumer behavior: A review of research on eWOM and hotels. *International Journal of Hospitality Management*, 36, pp.41-51.
- Cai, H. and Wang, J.T.Y., 2006. Overcommunication in strategic information transmission games. *Games and Economic Behavior*, 56(1), pp.7-36.
- Charness, G. and Dufwenberg, M., 2006. Promises and partnership. *Econometrica*, 74(6), pp.1579-1601.
- Charness, G., Dufwenberg, M., 2010. Bare promises: An experiment. *Economics Letters* 107(2), 281-283.
- Chen, J. and Houser, D., 2017. Promises and lies: can observers detect deception in written messages. *Experimental Economics*, 20(2), pp.396-419.
- Childs, J., 2012. Gender differences in lying. *Economics Letters*, 114(2), pp.147-149.

Church, B.K., Lynn Hannan, R. and Kuang, X.J., 2014. Information acquisition and opportunistic behavior in managerial reporting. *Contemporary accounting research*, 31(2), pp.398-419.

Crawford, V.P. and Sobel, J., 1982. Strategic information transmission. *Econometrica: Journal of the Econometric Society*, pp.1431-1451.

Dana, J.D., Kuang, X.J. and Iber, R.A., 2004. Exploiting moral wriggle room: Behavior inconsistent with a preference for fair outcomes.

Dickhaut, J.W., McCabe, K.A. and Mukherji, A., 1995. An experimental study of strategic information transmission. *Economic Theory*, 6(3), pp.389-403.

Dreber, A., Johannesson, M., 2008. Gender differences in deception. *Economics Letters*, 99(1), 197-199.

Dufwenberg, M. and Gneezy, U., 2000. Measuring beliefs in an experimental lost wallet game. *Games and economic Behavior*, 30(2), pp.163-182.

Dufwenberg, M. and Kirchsteiger, G., 2004. A theory of sequential reciprocity. *Games and economic behavior*, 47(2), pp.268-298.

Dzindolet, M.T. and Pierce, L.G., 2004, September. A computerized text analysis can detect deception. In *Proceedings of the Human Factors and Ergonomics Society Annual Meeting* (Vol. 48, No. 3, pp. 635-639). SAGE Publications.

Eliasz, K. and Schotter, A., 2010. Paying for confidence: An experimental study of the demand for non-instrumental information. *Games and Economic Behavior*, 70(2), pp.304-324.

Ellingsen, T., Johannesson, M., 2004. Promises, threats, and fairness. *Economic Journal* 114, (495), 397-420.

Ellingsen, T., Johannesson, M., 2006. Pride and prejudice: The human side of incentive theory. *American Economic Review*, 98(3), 990-1008.

Ellingsen, T., Johannesson, M., Tjøtta, S., Torsvik, G., 2010. Testing guilt aversion. *Games and Economic Behavior* 68(1), 95-107.

Erat, S. and Gneezy, U., 2012. White lies. *Management Science*, 58(4), pp.723-733.

Falk, A. and Fischbacher, U., 2006. A theory of reciprocity. *Games and economic behavior*, 54(2), pp.293-315.

Fehr, E. and Gächter, S., 1998. Reciprocity and economics: The economic implications of Homo Reciprocans¹. *European economic review*, 42(3-5), pp.845-859.

Fehr, E., Schmidt, K.M., 1999. A theory of fairness, competition, and cooperation. *The Quarterly Journal of Economics* 114(3), 817-868.

Fels, M., 2015. On the value of information: Why people reject medical tests. *Journal of Behavioral and Experimental Economics*, 56, pp.1-12.

Filieri, R., Alguezaui, S. and McLeay, F., 2015. Why do travelers trust TripAdvisor? Antecedents of trust towards consumer-generated media and its influence on recommendation adoption and word of mouth. *Tourism Management*, 51, pp.174-185.

Fischbacher, U., 2007. z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental economics*, 10(2), pp.171-178.

Fischbacher, U. and Föllmi-Heusi, F., 2013. Lies in disguise – an experimental study on cheating. *Journal of the European Economic Association*, 11(3), pp.525-547.

Gibson, R., Tanner, C. and Wagner, A.F., 2013. Preferences for truthfulness: Heterogeneity among and within individuals. *The American Economic Review*, 103(1), pp.532-548.

Gneezy, U., 2005. Deception: The role of consequences. *The American Economic Review*, 95(1), pp.384-394.

Gneezy, U., Rockenbach, B. and Serra-Garcia, M., 2013. Measuring lying aversion. *Journal of Economic Behavior & Organization*, 93, pp.293-300.

Golman, R. and Loewenstein, G., 2015. Curiosity, information gaps, and the utility of knowledge.

Golman, R., Hagmann, D. and Loewenstein, G., 2017. Information avoidance. *Journal of Economic Literature*, 55(1), pp.96-135.

Grazioli, S. and Jarvenpaa, S.L., 2000. Perils of Internet fraud: An empirical investigation of deception and trust with experienced Internet consumers. *IEEE Transactions on Systems, Man, and Cybernetics-Part A: Systems and Humans*, 30(4), pp.395-410.

Greenberg, A.E., Smeets, P. and Zhurakhovska, L., 2015. Promoting truthful communication through ex-post disclosure.

Greiner, B., 2015. Subject pool recruitment procedures: organizing experiments with ORSEE. *Journal of the Economic Science Association*, 1(1), pp.114-125.

Hao, L. and Houser, D., 2012. Belief elicitation in the presence of naïve respondents: An experimental study. *Journal of Risk and Uncertainty*, 44(2), pp.161-180.

Houser, D., Vetter, S., & Winter, J. (2012). Fairness and cheating. *European Economic Review*, 56(8), 1645-1655.

Hurkens, S. and Kartik, N., 2009. Would I lie to you? On social preferences and lying aversion. *Experimental Economics*, 12(2), pp.180-192.

Inderst, R., Rajko, A. and Ockenfels, A., 2010. Transparency and disclosing conflicts of interest: An experimental investigation. In Working paper Beiträge zur Jahrestagung des Vereins für Socialpolitik 2010. Ökonomie der Familie Kiel.

Ismayilov, H. and Potters, J., 2013. Disclosing advisor's interests neither hurts nor helps. *Journal of Economic Behavior & Organization*, 93, pp.314-320.

Kolotilin, A., 2015. Experimental design to persuade. *Games and Economic Behavior*, 90, pp.215-226.

Kübler, D. and Weizsäcker, G., 2004. Limited depth of reasoning and failure of cascade formation in the laboratory. *The Review of Economic Studies*, 71(2), pp.425-441.

Levine, E.E. and Schlitzer, M.E., 2015. Prosocial lies: When deception breeds trust. *Organizational Behavior and Human Decision Processes*, 126, pp.88-106.

Lightle, J.P., 2013. Harmful lie aversion and lie discovery in noisy expert advice games. *Journal of Economic Behavior & Organization*, 93, pp.347-362.

López-Pérez, R., 2010. Guilt and shame: An axiomatic analysis. *Theory and Decision*, 69(4), pp.569-586.

López-Pérez, R. and Spiegelman, E., 2013. Why do people tell the truth? Experimental evidence for pure lie aversion. *Experimental Economics*, 16(3), pp.233-247.

Loewenstein, G., 1994. The psychology of curiosity: A review and reinterpretation. *Psychological bulletin*, 116(1), p.75.

Lundquist, T., Ellingsen, T., Gribbe, E. and Johannesson, M., 2009. The aversion to lying. *Journal of Economic Behavior & Organization*, 70(1), pp.81-92.

Martínez, M., 2015. Deception in Sender-Receiver games. *Erkenntnis*, 80(1), pp.215-227.

Mazar, N. and Ariely, D., 2006. Dishonesty in everyday life and its policy implications. *Journal of public policy & Marketing*, 25(1), pp.117-126

Miké, V., 2000. Seeking the truth in a world of chance. *Technology in Society*, 22(3), pp.353-360.

Miller, G.R. and Stiff, J.B., 1993. Deceptive communication. Sage Publications, Inc.

Newman, M.L., Pennebaker, J.W., Berry, D.S. and Richards, J.M., 2003. Lying words: Predicting deception from linguistic styles. *Personality and social psychology bulletin*, 29(5), pp.665-675.

Pascual-Ezama, D., Fosgaard, T.R., Cardenas, J.C., Kujal, P., Veszteg, R., de Liaño, B.G.G., Gunia, B., Weichselbaumer, D., Hilken, K., Antinyan, A. and Delnoij, J., 2015. Context-dependent cheating: Experimental evidence from 16 countries. *Journal of Economic Behavior & Organization*, 116, pp.379-386.

Peeters, R., Vorsatz, M. and Walzl, M., 2015. Beliefs and truth-telling: A laboratory experiment. *Journal of Economic Behavior & Organization*, 113, pp.1-12.

Phelps, E.A., 2006. Emotion and cognition: insights from studies of the human amygdala. *Annu. Rev. Psychol.*, 57, pp.27-53.

Postlewaite, A., 2011. Social norms and social assets. *Annual. Rev. Econ.*, 3(1), pp.239-259.

Reuben, E. and Stephenson, M., 2013. Nobody likes a rat: On the willingness to report lies and the consequences thereof. *Journal of Economic Behavior & Organization*, 93, pp.384-391.

Rode, J., 2010. Truth and trust in communication: Experiments on the effect of a competitive context. *Games and Economic Behavior* 68(1), 325-338.

Robert, I. and Arnab, M., 2013. Is dishonesty contagious?. *Economic Inquiry*, 51(1), pp.722-734.

Rosaz, J. and Villeval, M.C., 2012. Lies and biased evaluation: A real-effort experiment. *Journal of Economic Behavior & Organization*, 84(2), pp.537-549.

Rubin, P.H., 1990. The economics of regulating deception. *Cato J.*, 10, p.667.

Shani, Y. and Zeelenberg, M., 2007. When and why do I want to know? How experienced regret promotes post-decision information search. *Journal of Behavioral Decision Making*, 20(3), pp.207-222.

Sánchez-Pagés, S. and Vorsatz, M., 2009. Enjoy the silence: an experiment on truth-telling. *Experimental Economics*, 12(2), pp.220-241

Sánchez-Pagés, S. and Vorsatz, M., 2007. An experimental study of truth-telling in a Sender-Receiver game. *Games and Economic Behavior*, 61(1), pp.86-112.

Serra-Garcia, M., van Damme, E. and Potters, J., 2011. Hiding an inconvenient truth: Lies and vagueness. *Games and Economic Behavior*, 73(1), pp.244-261.

Sheremeta, R.M. and Shields, T.W., 2013. Do liars believe? Beliefs and other-regarding preferences in Sender-Receiver games. *Journal of Economic Behavior & Organization*, 94, pp.268-277.

Sobel, J., 2013. Ten possible experiments on communication and deception. *Journal of Economic Behavior & Organization*, 93, pp.408-413.

Sutter, M., 2009. Deception through telling the truth?! Experimental evidence from individuals and teams. *The Economic Journal*, 119(534), pp.47-60.

Swann Jr, W.B., 1992. Seeking "truth," finding despair: Some unhappy consequences of a negative self-concept. *Current Directions in Psychological Science*, 1(1), pp.15-18.

Swann, W.B., Stein-Seroussi, A. and Giesler, R.B., 1992. Why people self-verify. *Journal of personality and social psychology*, 62(3), p.392.

Tadelis, S. and Zettelmeyer, F., 2015. Information disclosure as a matching mechanism: Theory and evidence from a field experiment. *American Economic Review*, 105(2), pp.886-905.

Tonin, M. and Vlassopoulos, M., 2013. Experimental evidence of self-image concerns as motivation for giving. *Journal of Economic Behavior & Organization*, 90, pp.19-27.

Van de Ven, J. and Villeval, M.C., 2015. Dishonesty under scrutiny. *Journal of the Economic Science Association*, 1(1), pp.86-99.

Vanberg, C., 2008. Why do people keep their promises? An experimental test of two explanations. *Econometrica*, 76(6), pp.1467-1480.

Wang, J.T.Y., Spezio, M. and Camerer, C.F., 2010. Pinocchio's pupil: using eyetracking and pupil dilation to understand truth telling and deception in Sender-Receiver games. *The American Economic Review*, 100(3), pp.984-1007.

Xiao, E. and Houser, D., 2009. Avoiding the sharp tongue: Anticipated written messages promote fair economic exchange. *Journal of Economic Psychology*, 30(3), pp.393-404.

Zuckerman, M., DePaulo, B.M. and Rosenthal, R., 1981. Verbal and nonverbal communication of deception. *Advances in experimental social psychology*, 14, pp.1-59.

Appendix to Chapter 3: The Effect of Ex-Post Information Acquisition in Sender-Receiver Games: Experimental Evidence

- A. Experimental instructions for fixed messages
- B. Experimental instructions for free messages
- C. Background of the participants in fixed messages
- D. Background of the participants in free messages
- E. Belief elicitation's questionnaires
- F. Post-experimental questionnaire
- G. Additional analysis: intentional deception
- H. Regression variables
- I. Messages

Appendix A. Experimental instructions for fixed messages

Note: Instructions specific to Player 1 and to Player 2 were shown separately to Player 1 and Player 2, respectively, on their computer screens.

All treatments (beginning of the experiment)

Thank you for participating in this experiment. In this experiment, you can earn money. What you earn will depend upon your decision and on the decision of another participant in the room. No data that you provide can be associated with your person. All data will be treated confidentially.

Please follow the instructions carefully. These instructions explain how the experiment works. If any of the instruction are unclear, or if you have any questions, please raise your hand and I will come and assist you. Please do not talk to any other participant during the experiment.

In this experiment, you will be randomly matched with another participant in this room. I call him/her your co-participant. One of you will be assigned to the role of Player 1 and the other will be assigned to the role of Player 2. You will hold this role throughout

the experiment. Player 1 will not learn the identity of Player 2, and similarly Player 2 will not learn the identity of Player 1.

Your earnings will be paid to you in cash at the end of the experiment.

A.1. Instructions for NoInfo treatment

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will not get any information about these amounts.

Decisions: There are two messages. Both players will know what the messages are. Player 1 first chooses one of the messages and sends it to Player 2. After reading the message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Player 2 will then be informed how much he/she will be paid from the chosen option, but Player 2 will never learn how much Player 1 got, and Player 2 will also never learn how much the option that he or she did not choose gave to Player 1 and 2.

After Player 2 has made his or her decision, no more decisions will be made.

A.2. Instructions for ExoInfo treatment:

This experiment has two stages.

First stage

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will not get any information about these amounts.

Decisions: There are two messages. Both players will know what the messages are. Player 1 first chooses one of the messages and sends it to Player 2. After reading the message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Second stage

Options: Player 2 will be informed how much he/she will be paid from the chosen option, and how much Player 1 got. Player 2 will also learn how much the option that he or she did not choose gave to Player 1 and 2.

Message: Player 2 can then write one or several messages and send it to Player 1. Player 2 is free to write whatever he or she prefers (he/she can choose not to write anything), but I ask Player 2 not to use threatening language or reveal his or her identity. If I see that Player 2 does this, I reserve the right to only pay him or her the participation fee.

After Player 2 has send his message to Player 1, no more decisions will be made.

A.3. Instructions for EndoInfo-Costly treatment:

This experiment has two stages.

First stage

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will initially not get any information about these amounts.

Decisions: There are two messages. Both players will know what the messages are. Player 1 first chooses one of the messages and send it to Player 2. After reading the message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Player 2 will then be informed how much he/she will be paid from the chosen option, but Player 2 will not learn how much Player 1 got, and Player 2 will also not learn how much the option that he or she did not choose gave to Player 1 and 2.

Second stage

Options: Player 2 can pay £1 to find out how much money the chosen option gives to Player 1, and how much the option that Player 2 did not choose would have given to each player. If Player 2 decides to pay £1 to get this information, the £1 will be subtracted from Player 2's overall earnings.

Message: Regardless of whether or not Player 2 has paid for information, Player 2 can write one or several messages and send it to Player 1. Player 2 is free to write whatever he or she prefers (he/she can choose not to write anything), but I ask Player 2 to not use threatening language or reveal his or her identity. If I see that Player 2 does this, I reserve the right to only pay him or her the participation fee.

After Player 2 has send his message to Player 1, no more decisions will be made.

A.4. Instructions for EndoInfo-Free treatment:

This experiment has two stages.

First stage

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will not get any information about these amounts.

Decisions: There are two messages. Both players will know what the messages are. Player 1 first chooses one of the messages and sends it to Player 2. After reading the

message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Second stage

Options: Player 2 will be informed how much he/she will be paid from the chosen option, and how much Player 1 got. Player 2 will also learn how much the option that he or she did not choose gave to Player 1 and 2.

Message: Player 2 can then write one or several messages and send it to Player 1. Player 2 is free to write whatever he or she prefers (he/she can choose not to write anything), but I ask Player 2 not to use threatening language or reveal his or her identity. If I see that Player 2 does this, I reserve the right to only pay him or her the participation fee.

After Player 2 has send his message to Player 1, no more decisions will be made.

A.5. Instructions for EndoInfo-Gain treatment:

This experiment has two stages.

First stage

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will initially not get any information about these amounts.

Decisions: Player 1 is first given 120 seconds to write one or several messages to send to Player 2. Player 1 is free to write whatever he or she prefers (he or she can choose not to write anything), but I ask Player 1 not to reveal his or her identity. When the 120 seconds are gone, Player 1 cannot write anymore. After reading the message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Player 2 will then be informed how much he/she will be paid from the chosen option, but Player 2 will not learn how much Player 1 got, and Player 2 will also not learn how much the option that he or she did not choose gave to Player 1 and 2.

Second stage

Options: Player 2 can pay £1 to find out how much money the chosen option gives to Player 1, and how much the option that Player 2 did not choose would have given to each player. If Player 2 decides to pay £1 to get this information, the £1 will be subtracted from Player 2's overall earnings.

Message: Regardless of whether or not Player 2 has paid for information, Player 2 can write one or several messages and send it to Player 1. Player 2 is free to write whatever he or she prefers (he/she can choose not to write anything), but I ask Player 2 to not use threatening language or reveal his or her identity. If I see that Player 2 does this, I reserve the right to only pay him or her the participation fee.

After Player 2 has send his message to Player 1, no more decisions will be made.

Appendix B. Experimental instructions for free messages

B.1. Instructions for FreeMess-NoInfo treatment

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will not get any information about these amounts.

Decisions: Player 1 is first given 120 seconds to write one or several messages to send to Player 2. Player 1 is free to write whatever he or she prefers (he or she can choose not to write anything), but I ask Player 1 not to reveal his or her identity. When the 120 seconds are gone, Player 1 cannot write anymore. After reading the message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Player 2 will then be informed how much he/she will be paid from the chosen option, but Player 2 will never learn how much Player 1 got, and Player 2 will also never learn how much the option that he or she did not choose gave to Player 1 and 2.

After Player 2 has made his or her decision, no more decisions will be made.

B.2. Instructions for FreeMess-EndoInfo-Costly treatment:

This experiment has two stages.

First stage

Options: There are two options, A and B. Each option specifies an amount of money to Player 1 and to Player 2. Player 1 will be informed about how much each option pays out to each player, but Player 2 will initially not get any information about these amounts.

Decisions: Player 1 is first given 120 seconds to write one or several messages to send to Player 2. Player 1 is free to write whatever he or she prefers (he or she can choose not to write anything), but I ask Player 1 not to reveal his or her identity. When the 120 seconds are gone, Player 1 cannot write anymore. After reading the message, Player 2 then chooses Option A or Option B, and each player receives the payment from the chosen option.

Player 2 will then be informed how much he/she will be paid from the chosen option, but Player 2 will not learn how much Player 1 got, and Player 2 will also not learn how much the option that he or she did not choose gave to Player 1 and 2.

Second stage

Options: Player 2 can pay £1 to find out how much money the chosen option gives to Player 1, and how much the option that Player 2 did not choose would have given to each player. If Player 2 decides to pay £1 to get this information, the £1 will be subtracted from Player 2's overall earnings.

Message: Regardless of whether or not Player 2 has paid for information, Player 2 can write one or several messages and send it to Player 1. Player 2 is free to write whatever he or she prefers (he/she can choose not to write anything), but I ask Player 2 to not use threatening language or reveal his or her identity. If I see that Player 2 does this, I reserve the right to only pay him or her the participation fee.

After Player 2 has send his message to Player 1, no more decisions will be made.

Appendix C. Background of the participants in fixed messages

Table 13. Percentage of male and female subjects by treatment

Treatment	Male		Female	
	Frequency	Percent	Frequency	Percent
NoInfo (T1)	22	50.00	22	50.00
ExoInfo (T2)	22	55.00	18	45.00
EndoInfo-Costly (T3)	20	50.00	20	50.00
EndoInfo-Free (T4)	17	47.22	19	52.78
EndoInfo-Gain (T5)	16	57.14	12	42.86
Total	97	51.60	91	48.40

Table 14. Percentage of subjects per country

Country	Frequency	Percent
Canada	4	2.13
China	1	0.53
France	1	0.53
Germany	1	0.53
Greece	1	0.53
Holland	1	0.53
Hong Kong	1	0.53
Italy	2	1.06
Malaysia	1	0.53
Singapore	1	0.53
South Africa	1	0.53
Sri Lanka	1	0.53
Sudan	1	0.53
Switzerland	1	0.53
Tanzania	1	0.53
USA	4	2.13
United Kingdom	165	87.77
Total	188	100

Table 15. Percentage of subjects by field of study

Field of Study	Frequency	Percent
Accounting and Finance	1	0.53
Actuarial Science	2	1.06
American Studies	1	0.53
Biological Studies	11	5.85
Business and Management	8	4.26
CMP	1	0.53
Chemistry	2	1.06
Cognitive Neuroscience	1	0.53
Computer Science	6	3.19
Drama	4	2.13
Ecology	3	1.6
Economics	35	18.62
Engineering	2	1.06
English Literature	12	6.38
Environmental Sciences	7	3.72
Film and English	2	1.06
Film and History	2	1.06
Film and Television	2	1.06
Film and Television	2	1.06
Forensic And Investigative Chemistry	1	0.53
French with Business	1	0.53
Health sciences	1	0.53
History	8	4.26
History and Politics	1	0.53
International Development	15	7.98
Languages	2	1.06
Law	10	5.32
Marketing	1	0.53
Mathematics	7	3.72
Media	1	0.53
Medicine	4	2.13
Natural Sciences	1	0.53
Nursing	2	1.06
Oceanography	1	0.53
Pharmacy	9	4.79
Philosophy	2	1.06
Politics	5	2.66
Politics, Philosophy and Economics	5	2.66
Psychology	5	2.66
Society, Culture and Media	1	0.53
Speech and Language Therapy	1	0.53
Total	188	100

Table 16. Percentage of subjects by language

Language	Frequency	Percent
Another language	7	3.72
English	181	96.28
Total	188	100

Table 17. Descriptive statistics for age by treatment

Treatment	Number of subjects	Age (years)			
		Mean	S.D	Min	Max
NoInfo (T1)	44	20.23	2.60	18	31
ExoInfo (T2)	40	20.00	1.41	18	24
EndoInfo-Costly (T3)	40	20.50	2.26	18	28
EndoInfo-Free	36	20.19	1.60	18	25
EndoInfo-Gain	28	19.64	1.87	18	27
Total	188	20.14	2.03	18	31

Table 18. Descriptive statistics for experimental earnings by treatment

Treatment	Number of subjects	Experimental Earnings (£)			
		Mean	S.D	Min	Max
NoInfo (T1)	44	10.00	2.99	6.00	14
ExoInfo (T2)	40	10.00	1.81	6.00	14
EndoInfo-Costly (T3)	40	9.90	2.66	5.00	14
EndoInfo-Free (T4)	36	10.00	2.34	6.00	14
EndoInfo-Gain (T5)	28	10.04	2.84	6.10	14
Total	188	9.99	2.53	5.00	14

Appendix D. Background of the participants in free messages

Table 19. Percentage of subjects by gender

Gender	Frequency	Percent
Male	37	44.05
Female	47	55.95
Total	84	100.00

Table 20. Percentage of subjects by country

Country	Frequency	Percent
Cyprus	1	1.19
Hong Kong	1	1.19
USA	2	2.38
United Kingdom	80	95.24
Total	84	100.00

Table 21. Percentage of subjects per language

Language	Frequency	Percent
Another language	3	3.57
English	81	96.43
Total	84	100.00

Table 22. Percentage of subjects by field of study

Field of Study	Frequency	Percent
Accounting and Finance	1	1.19
Actuarial Science	1	1.19
Biological Sciences	1	1.19
Biology	5	5.95
Business	3	3.57
Climate Change	1	1.19
Ecology	1	1.19
Economics	16	19.05
English Literature	8	9.52
English and Film Studies	1	1.19
Environmental Sciences	12	14.29
Film and English Studies	1	1.19
History	6	7.14
History and Film	1	1.19
Humanities	1	1.19
International Development	4	4.76
Languages	1	1.19
Law	4	4.76
Literature and History	1	1.19
Mathematics	1	1.19
Medicine	2	2.38
Occupational Therapy	1	1.19
Pharmacy	5	5.95
Philosophy	1	1.19
Politics	1	1.19
Psychology	2	2.38
Speech and Language Therapy	2	2.38
Total	84	100.00

Table 23. Descriptive statistics for age by treatment

Treatment	Number of subjects	Age (years)			
		Mean	S.D	Min	Max
FreeMess-NoInfo	48	19.83	2.36	18	31
FreeMess-EndoInfo-Costly	36	19.33	1.35	18	24
Total	84	19.62	1.99	18	31

Table 24. Descriptive statistics for experimental earnings by treatment

Treatment	Number of subjects	Experimental earnings (£)			
		Mean	S.D	Min	Max
FreeMess-NoInfo	48	10.00	2.61	6	14
FreeMess-EndoInfo-Costly	36	9.97	2.14	6	14
Total	84	9.99	2.41	6	14

Appendix E. Belief elicitation's questionnaires

Figure 18. Questionnaire screen for elicitation of Sender's beliefs for EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Gain

Time remaining [seconds]: 119

You are Player 1

While Player 2 chooses an option, please answer the following questions:

1. Which option do you expect Player 2 to choose?: A
 B

Remember:

Option A: £10 to you and £10 to Player 2.
Option B: £14 to you and £6 to Player 2.

2. Out of 100 Player 2s, how many do you think follow Player 1's message on average?

3. Do you expect that the Player 2 you are matched with will decide to get information about all the money amounts?: Yes
 No

4. Out of 100 Player 2s, how many do you think decide to get information?:

Please press the OK button to continue

OK

Figure 19. Questionnaire screen for elicitation of Receiver's beliefs about the Sender's message for NoInfo, ExoInfo, EndoInfo-Costly, EndoInfo-Free, and EndoInfo-Gain

You are Player 2

Do you think Player 1's message was true or false? True
 False

Remember: The messages were:
Message 1: "Option A will earn you more money than Option B."
Message 2: "Option B will earn you more money than Option A."

Please press the OK button to continue

OK

Appendix F. Post-experimental questionnaire (all treatments)

Figure 20. Post-experimental questionnaire's screens

Please answer the following questions. Your answers will stay anonymous.

What is your age?

What is your gender?
 Male
 Female

What is your field of study?

What is your native language?
 English
 Another language

What country were you born in?

Please provide feedback about the experiment here:

Appendix G. Additional analysis: intentional deception

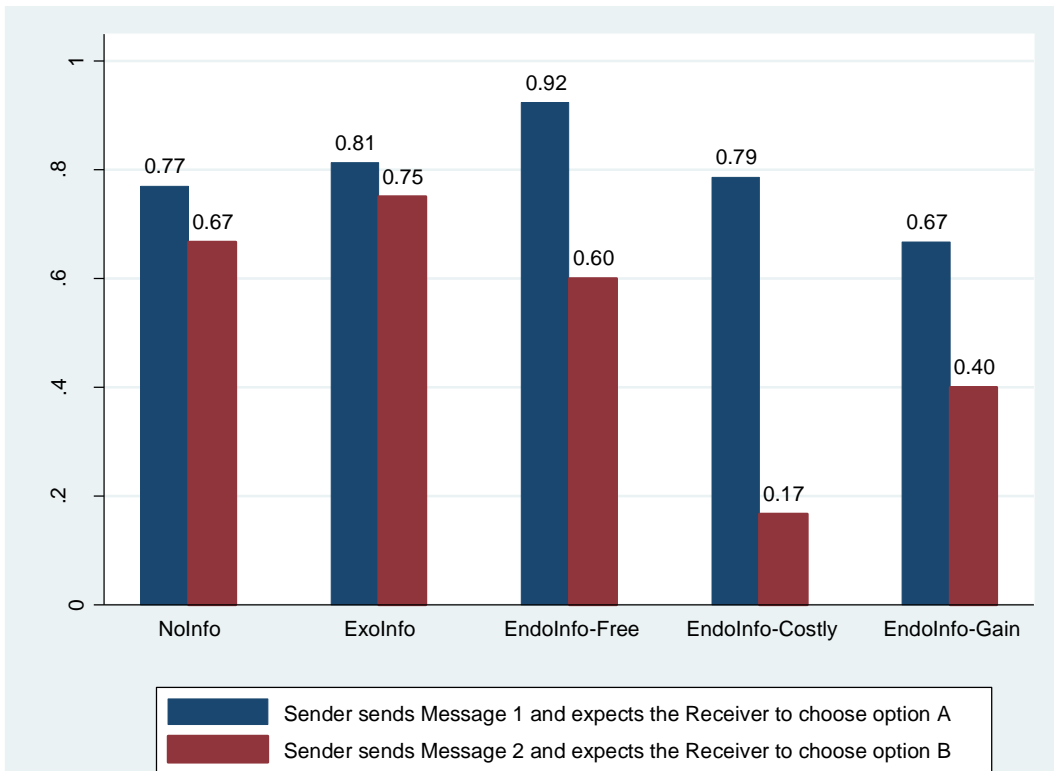
Previous experimental evidence from Sender-Receiver games have suggested that Senders can also tell the truth to deceive the Receiver: the Sender anticipating that the Receiver would reject the message, sends the true message (Sutter 2009, Sánchez-Pagés and Vorsatz 2007). In this section, I examine the Sender's intention to deceive.

Figure 21 shows the relative frequency of Senders expecting the Receiver to implement the chosen message depending upon the chosen message. If the Sender sends Message 1, he/she expects the Receiver to implement the chosen message in 77% of the cases in NoInfo, 81% of the cases in ExoInfo, 79% of the cases in EndoInfo-Costly, 92% of the cases in EndoInfo-Free, and 67% of the cases in EndoInfo-Gain.

On the other hand, if the Sender sends Message 2, he/she expects the Receiver to implement the chosen message in 67% of the cases in NoInfo, 75% of the cases in ExoInfo, 17% of the cases in EndoInfo-Costly, 60% of the cases in EndoInfo-Free and 40% of the cases in EndoInfo-Gain. Therefore, those Senders who sent Message 1 do not actually intend to deceive the Receiver.¹⁹

¹⁹ Sutter (2009) called 'liar', the Senders who send Message 2 and expect the Receiver will choose Option B; 'benevolent liar', the Senders who send Message 2 and expect the Receiver will choose Option A; 'sophisticated truth-teller', the Senders who send Message 1 and expect the Receiver will choose Option B; and 'benevolent truth-teller', the Senders who send Message 1 and expect the Receiver will choose Option A.

Figure 21. Sender's expectations depending upon the chosen message



Appendix H. Regression variables

Table 25. Definition of variables

Variable	Definition
NoInfo	= 1 if treatment is NoInfo; 0 otherwise.
ExoInfo	= 1 if treatment is ExoInfo; 0 otherwise.
EndoInfo-Costly	= 1 if treatment is EndoInfo-Costly; 0 otherwise.
EndoInfo-Free	= 1 if treatment is EndoInfo-Free; 0 otherwise.
EndoInfo-Gain	= 1 if treatment is EndoInfo-Gain; 0 otherwise.
Sender sends Message 2 (P1m2)	= 1 if the Sender sends Message 2; 0 otherwise.
Sender sends Message 1 (P1m1)	= 1 if the Sender sends Message 1; 0 otherwise.
Sender expects Receiver follows the message (P1expfoll)	= 1 if the Sender expects the Receiver to follow the message; 0 otherwise.
Sender expects Receiver gets information (P1expinfo)	= 1 if the Sender expects the Receiver to get ex-post information about the payoffs; 0 otherwise.
Receiver follows the message (P2foll)	= 1 if the Receiver follows the Sender's message; 0 otherwise.
Receiver gets ex-post information about he payoffs (P2info)	= 1 if the Receiver decides to get ex-post information about the payoffs; 0 otherwise.
Receiver expects message to be true (P2exptruth)	= 1 if the Receiver expects the Sender's message to be true; 0 otherwise.
Age	= subject's age in years
Male	= 1 if the subject is male; 0 otherwise
Female	= 1 if the subject is female; 0 otherwise.

Appendix I. Messages

Table 26. Receivers' messages for ExoInfo treatment (only session 1)

Treatment	Session	ID	Message	Message	Receiver's chosen option	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
ExoInfo	1	1	Cheers geeze This is like that tv show golden balls Dont lure me into false hope I Ill set you a beer	1	A	X									X			X			
ExoInfo	1	2		2	A																X
ExoInfo	1	3	thanks	1	A	X															
ExoInfo	1	4	lol I'm obviously not a very trusting person At least you were nice about it haha or were you being strategic because most people would have thought the other was lying? sneaky enjoy your £14 if only I was player one I'm just going to keep sending messages haah i'll trust you next time if we do it again please don't fuck me over :) apologies again for not trusting your original	1	B				X	X		X		X			X		X		
ExoInfo	1	5	Well played	1	A		X														
ExoInfo	1	6	Be truthful everytime If you start lying, then I can find out after and I will be unsure what to pick next time	1	A									X		X					
ExoInfo	1	7	Thank you for telling the truth	1	A	X															
ExoInfo	1	8	Cheers :)	1	A	X															
ExoInfo	1	9	sharing is caring what would jesus do?	2	B								X								
ExoInfo	1	10	cheers, :D lets try and both get the most money out of this i like uea hehe	1	A	X												X			
ExoInfo	1	11	If you pick option A I will send you some dank memes, like A is a pretty swell deal, we both get £10 of dank cush. Option B is pretty alright too I guess, we both gen money for both but you don't get as many memes for it, so in all fairness it's not really a very good deal in the long run due to lack of memes. TLDR pick option A bro	1	A				X												X

Note: All messages are unedited. The complete messages from the Receivers are available from the author upon request. ID indicates the Receiver. 1 = Thankful, 2 = Acknowledge, 3 = Fairness, 4 = Trust, 5 = Sarcasm, 6 = Anger, 7 = Apology, 8 = Humour, 9 = Confused, 10 = Hope, 11 = Warning, 12 = In doubt, 13 = Off-topic, 14 = Slang, 15 = You'd do the same, and 16 = No message.

Table 27. Senders' messages for FreeMess-NoInfo treatment (only session 1)

Treatment	Session	ID	Messages	Receiver's chosen option	Categories													
					1	2	3	4	5	6	7	8	9	10	11	12	13	
FreeMess-NoInfo	1	1	Option A I get £14 and you get £6, Option B we both get £10	B			X											
FreeMess-NoInfo	1	2	I can't remember if player 2 is allowed to reply to this, but either way I'm not really sure what I'm supposed to be saying to you... I guess it's supposed to be a way of me telling you what the options are? In which case, if you choose option A we will get an equal amount, and if you choose option B we get different amounts!	A					X									
FreeMess-NoInfo	1	3	I am going to choice the option that gives us the same amount of money	A									X					
FreeMess-NoInfo	1	4	pick one at random both options are decent for both of us. option b may be slightly better	A						X	X							
FreeMess-NoInfo	1	5	go for option A i think it would be fairer	A							X						X	
FreeMess-NoInfo	1	6	OPTION B	B							X							
FreeMess-NoInfo	1	7	You decide A or B	A													X	
FreeMess-NoInfo	1	8	Hello, not sure what I am supposed to write here I'm going to chose a the fair option Option A?	A								X	X					
FreeMess-NoInfo	1	9	Option A is £10 each and option B is £14 to player 2 and £6 to player 1	A		X		X										
FreeMess-NoInfo	1	10	enjoy the money :)	A													X	
FreeMess-NoInfo	1	11	option a is £14 player 1 and £7 player 2 option b is £10 to both players	B							X							
FreeMess-NoInfo	1	12	Hello, I will be fair with my decision I will give you the same amount as myself :)	A									X				X	

Note: The complete unedited Senders' messages are available from the author upon request. ID indicates the Sender. A indicates the Receiver chose option A, B indicates the Receiver chose option B. 1 = All payoffs true, 2 = One payoff true, 3 = All payoffs false, 4 = One payoff false, 5 = Partial true, 6 = Ambiguous message, 7 = Direct recommendation, 8 = Indirect recommendation, 9 = Confused, 10 = Off-topic, 11 = It's up to you, 12 = Fairness, and 13 = No message.