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**Essays on the
House Money Effect**

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Declaration

I declare that this thesis was composed by myself and that the work contained therein is my own, except where explicitly stated otherwise in the text.

(Athanasia Arnokourou)

Στους γονείς μου, Αναστασία και Διονύση
και στη θεία μου, Φωτεινή
που με ανέθρεψαν να 'στέκομαι' στα πόδια μου.
Και στην αδερφή μου, Μαρία
που με τη δύναμή της μου έδειξε
πώς να 'ξαναστέκομαι όρθια' και να συνεχίζω.

*To my parents, Anastasia and Dionysis
and my aunt, Foteini who taught me
to stand on my own feet.
And to my sister, Maria
who showed me with her strength
how to stand up and keep going after every fall.*

Abstract

This thesis provides a detailed analysis of the so-called ‘house money’ or windfall endowment effect and its main determinants.

Chapter 1 provides a detailed survey on the literature related to the house money effect. This effect according to Thaler and Johnson (1990) - refers to the situation where prior gains mitigate the influence of loss aversion and facilitate risk-seeking. The concept borrows its name from the expression employed in the gambling parlance of “playing with the house money”, which is used when people gamble while ahead. As the literature has used a variety of concepts and ideas to describe the house money effect, this chapter presents and discusses them within the environment and the related literature that they have emerged. This is done in order to highlight the predominant answers to the main research questions raised in the various strands of the literature, namely: (i) whether people treat money differently depending on its origin; and (ii) the implications of the house money effect for the experimental methodology in economics.

The literature is organised and presented according to the context in which the above two research questions have been examined. By presenting results in each particular context, we pin down the contextual differences that might be responsible for the presence (or absence) of the house money effect, and lay the initial ground work to answer a third research question: What drives the house money effect? In this regard, after we demonstrate the context-dependency of the house money effect we present the two main interpretations that it has received, namely that the house money effect is a result of different mental accounting over windfall gains (‘windfall effect’) or a result of fairness or deservingness concerns (‘Lockean desert effect’).

Chapter 2 re-examines the house money effect and explores its main driving forces. For that, we employ a novel experimental design utilising a within-subject approach, coupled with the use of three different contexts of economic decisions (a trust game, a set of lotteries and a public good game). Both the within-subject experimental design and the three contexts of economic decisions allow us to better test the two main interpretations of the house money effect.

Our experimental data confirm the presence of the house money effect both in the decision to trust (but not in the decision of trustworthiness) in the trust game and in the decision to contribute in the public account of the public good game. However, our findings do not support the hypothesis that changes in risk behaviour of participants are due to different sources of money, suggesting that risk attitudes are robust and independent of the origin of money along the experiment. Therefore, our findings seem to favour interpretations of the house

money effect as a result of ‘just desert’ or fairness preferences rather than the result of different mental accounting over windfall gains.

Chapter 3 combines two branches of experimental literature, namely the house money effect and the literature on individual differences in social preferences. Both the house money effect and individual differences have been used extensively to explain cooperation in social dilemmas (and its decline over time). Here, we test the implications of house money on reciprocal behaviour, that is, whether participants in economic experiments are less likely to reciprocate when earned money rather than windfall money is at stake. Using the innovative experimental design of Fischbacher et al. (2001) with strategy method, we classify participants according to their behaviour in a linear public good game, and by adding the within-subject element in our experimental design we test the robustness of this classification across the different origin of endowments. Our results indicate that the types’ classification is robust across the origin of money. Contrary to Harrison (2007), we find that participants’ decision to free ride or not (contribute or not) is independent of the origin of money, but given that the decision to contribute has been made, contribution levels may vary -actually be lower- when money is earned rather than windfall endowed. We also elicit beliefs about others’ contributions and test how these beliefs affected by the ‘house money’ and in turn how they affect the decision to contribute. This discussion relates to what the literature has characterised so far as “anticipatory reciprocity”.

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Along these lines, this thesis is also dedicated to the memory of my friend Alexandra and my student Stefan who I met during the first steps of my academic life, as a postgraduate student at the University of Warwick and later as a teaching fellow at the University of Edinburgh, respectively. Their loss, as well as the birth of my goddaughter Chryssa, reminded me to focus on the important things and motivated me to complete this thesis.

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Chapter 1

A Survey on the House Money Effect

1.1 Introduction

This chapter provides a detailed survey on the literature related to the house money effect. The house money effect borrows its name from the expression used in the gambling parlance of “*playing with the house money*”, which is used when people gamble while ahead. An example might make things clearer: imagine that you have just turned 21 and you are finally allowed to visit a casino with friends. You have saved £100 to gamble on your special night. As you enter the casino you see a slot machine and, feeling lucky, you give it a go. Chance seems to smile and you win £100. The question now is: Are you going to spend the £100 you won on the slot machine by a lucky hand the same way you are going to spend the £100 you have been saving the last two months?

The house money effect refers to the situation where “*after a gain, subsequent losses -smaller than the original gain- can be integrated with the prior gain, mitigating the influence of loss aversion and facilitating risk-seeking*” (Thaler and Johnson, 1990, p. 657). So if the house money effect holds, the answer to the previous question is obvious: you will be keener to spend the £100 you have won rather than the £100 you brought from home.

The question that then arises is the following: Do people treat money differently depending on its origin, meaning how the money (or the asset, in general) ended up in their hands? This research question has long triggered the interest of economists. Friedman (1957), in his seminal work on the permanent income hypothesis, discusses how people treat ‘windfall (unexpected) money’ differently than their regular income. Since Friedman (1957) many have looked into this question, as well as a host of related issues. Different approaches, sometimes from differing perspectives, have produced a variety of terms and concepts describing what we have earlier defined as the house money effect. In the rest of this chapter, we will present and discuss these concepts and ideas related to the house money effect, as well as the environment and the related literature within which they have emerged.

There is a second question, though, which is also of prime interest. Assum-

ing that people do treat the money differently depending on its source, what are the implications of this house money effect for the experimental methodology in economics? How can the house money effect influence decisions of participants in economic experiments where the common practice is to endow participants with an initial amount of money? What are the consequences for the external validity of economic experiments and how should the discipline take these considerations into account in order to advance the methodology used? Quoting Levitt and List (2007, p.170): *“By anticipating the types of biases common to the lab, experiments can be designed to minimize such biases. Further, knowing the sign and plausible magnitude of any biases induced by the lab, one can extract useful information from a study, even if the results cannot be seamlessly extrapolated outside the lab. In this sense, even in cases where lab results are believed to have little generalizability, some number from a laboratory estimate is better than no number, provided that a theoretical model is used to make appropriate inference.”*

Apart from tackling the two questions discussed above, this chapter also organises the related literature in such a way as to answer a third question: What drives the house money effect? This is actually the central question in Chapter 2 of this thesis, but here in Chapter 1 we trace the development of these important issues and draw out the tensions in prior research. It is hoped that this approach will prepare the reader and open the path for demonstrating how our subsequent research questions, findings and results fit into previous scholarly works.

The main contribution in this chapter is that the literature is organised and presented according to the context that the above mentioned research questions have been examined in. The obvious question arising at this point is why we should use this organisation criterion. Although the evidence on the existence of the house money effect is mixed, there is a general consensus that this effect is context-dependent¹, implying that different frameworks of examination might imply different results. By presenting results in each particular context, we will pin down the contextual differences that might be responsible for the presence or absence of the house money effect. In that way, we will facilitate our analysis and discussion in Chapter 2 where we explore what drives the house money effect. Facilitating our analysis in Chapter 2 further, we will also discuss here the literature behind the two commonly used interpretations of the house money effect, namely a different mental accounting over windfall gains (‘windfall effect’) or a result of fairness or deservingness concerns (‘Lockean desert effect’).

The rest of this chapter is organised as follows: We first define the house money effect and discuss its contextual dependency by presenting the related literature according to the context the house money effect has been examined in. We divide that literature between those scholarly works that examine the house money effect in the context of individual decisions, and those that do the same but within the environment of a game, where social preference may emerge. We then take a step back to briefly present the theoretical background that has been used to interpret the house money effect in the literature presented here.

¹See for example Thaler and Johnson (1990), Cherry et al. (2005), Kroll et al. (2007) etc.

1.2 The House Money Effect (HME)

The idea that people treat small amounts of ‘windfall’ or unexpected money differently than their regular income is quite old, dating back to Friedman (1957) and his work on permanent income hypothesis, where he presented evidence of the ‘windfall effect’. According to this hypothesis, assuming that people have preferences for smoothing consumption over their lifetime, their marginal propensity to consume a temporary (one-time) windfall increase in income will be smaller than if this increase in income was permanent²

The house money effect though was first formally discussed and defined by Thaler and Johnson (1990). In their paper, they examine how prior gains and losses affect risk-taking behaviour. Their work is a continuation of previous research³ that clearly established the idea that real decision makers do not only consider marginal costs -as economic theory suggests- but are often influenced by historical or sunk costs. Starting by recognising that prior outcomes (gains or losses) affect most decision makers, they explore how this takes place and they find evidence that prior gains and losses can influence choices in systematic ways. An example of these systematic ways is their finding that under some circumstances⁴ a prior gain can increase subjects’ willingness to accept gambles. They named this finding the house money effect. The intuition behind this - which also gave the name - is captured in the gambling parlance of “playing with the house money” which expresses the feeling of gambling while ahead.

1.3 The Context-Dependence of the HME

As we have underlined in the introduction earlier the context dependence of the house money effect has a general consensus in the literature. Cherry et al. (2005) conclude that “[...]although our earnings protocol replicated the Cherry et al. (2002)’s design, the results differ, suggesting that the influence of asset origin could be context dependent. Relative to anonymous dictators, subjects acting in the public goods game faced a more complex task that demanded greater cognitive effort and involved simultaneous decisions by other contributors.” Kroll et al. (2007) find that the origin of wealth seems to matter in asymmetric situations (such as our best-shot public good game with heterogeneous-wealth groups) and also conclude that the impact of endowment origin in the lab appears context-dependent and they suggest that future research should draw more attention on understanding how these contextual cues affect how people weight the different elements of preference. Throughout this thesis we use the term ‘context’ along the line of Cherry et al. (2005) and when we talk about ‘different contexts’ we refer to different decision tasks.

Here in this Section 1, we make a preliminary distinction between the studies that they examine the house money effect in the context of individual decisions

²There is an extensive early literature testing the permanent income hypothesis that yielded mixed only results (see Bodkin (1959); Bird and Bodkin (1965); Lee (1975); Keeler et al. (1985)).

³See Arkes and Blumer (1985), Thaler (1980).

⁴Those ‘circumstances’ will be described later in this chapter.

and those which focus on games, where social preference may appear because of the interaction with other counterparts. That is, we first consider studies that examine the house money effect in the contexts such as decisions over lotteries or investment decisions, while in the second part, we present relevant results for the house money effect in situations where people facing decisions in the context of a dictator, ultimatum, trust and public good game or in a market environment.

As we explain in more detail at the section regarding the interpretation of the house money effect, the idea behind this division in the literature is that contextual differences might favour the mental accounting interpretation in some cases and the ‘Lockean desert’ interpretation in some others.

1.3.1 The HME in Individual Decisions

We start here by presenting the house money effect in the context of individual decisions. In these studies presented here, participants in the relative experiments face an individual decision either in a form of deciding upon a set of lotteries or making an investment decision. We first continue from before our discussion on the original work of Thaler and Johnson (1990) and then we demonstrate results from earlier or subsequent work on the topic. This branch of the literature usually interprets the house money effect as treating ‘windfall gains’ within a different mental account than that they evaluate regular income.

1.3.1.1 Lotteries

Thaler and Johnson (1990) extended the previous work of work of Kahneman and Tversky (1979) on prospect theory by focusing on prior gains and losses and how these affect risky choices. Citing Kahneman and Tversky (1979, p.286) “... a person who has not made peace with his losses is likely to accept gambles that would be unacceptable to him otherwise”, Thaler and Johnson (1990) discuss the conditions under which prior outcomes might influence the reference point. Within the prospect theory framework, and specifically with regards to the editing phase, they suggest alternative editing rules and design surveys and experiments to test which of these best explains the phenomena in discussion.

Considering these five editing rules, namely *Prospect Theory with Memory*, *Prospect Theory with No Memory*, *Concreteness*, *Hedonic Editing* and *Quasi-Hedonic Editing*, as well as the findings from four experiments that they run, the authors conclude that the *Quasi-Hedonic* rule seems to do best relative to the others. *Quasi-Hedonic Editing* rule resulted as a modification of the *Hedonic Editing*⁵ rule to make it closer to the *Concreteness* editing rule in response of experimental data that refuted *Hedonic Editing*. *Quasi-Hedonic Editing* assumes that when subjects are presented choices in the one-stage format they do not actively segregate the sure gain and that when faced with a two-stage gamble involving a prior loss, subjects will not integrate subsequent losses with the initial loss. However, after prior gains, subsequent losses will be integrated with/cancelled against the prior gain. They run an experiment with actual choices for real money, which

⁵See section on mental accounting for description of *Hedonic Editing* rule.

Initial Outcome	Two-Stage Gambles Choices		Percent Risk Seeking		One-Stage Gambles Choices	
	Payoff	Prob.	N = 95	N = 111	Payoff	Prob.
1. +\$15	\$0	1	77	44	\$15	1
	\$4.50	0.5			\$19.50	0.5
	-\$4.50	0.5			\$10.50	0.5
5. +\$15	\$5	1	72	68	\$20	1
	\$15	0.33			\$30	0.33
	\$0	0.67			\$15	0.67

Table 1.1: Results from Experiment Using Real Money

largely replicated the results that they had previously obtained in experiments with hypothetical questions. Results on the question with prior gains in both kinds of experiments showed evidence that is consistent with the house money effect.

Since we discuss the importance of context for the house money effect, Thaler and Johnson (1990) themselves underlined the importance of framing⁶. They find consistent results across their experiments indicating that the house money effect is more evident when the situation is presented to participants in a two-stage rather than in a one-stage format⁷, suggesting that the presence of house money effect is context-dependent. The tables below show how questions were presented to participants and the percentage of those who chose the risk-seeking option. Kahneman and Tversky (1981) had already underlined the dependence of preferences on the formulation of decision problems and recognised that their theory was developed for one-shot gambles.

At around the same time, Battalio et al. (1990) conducted an experiment with four series of questions, where asking participants to make a choice between gambles. In the two series *gambles involved gains* and in other two they *involved losses*. Participants were first asked a set of hypothetical questions and later on they were invited to answer a second set of questions that involved real payments. Although there were systematic and significant quantitative differences over real and hypothetical sets of questions, the qualitative conclusions did not differ. Among the behavioural issues observed, they noted risk-loving behaviour over a number of prospects with all-positive payoffs, in cases where prospect theory would have predicted risk aversion. Therefore, as they claim, *“this calls into question prospect theory’s arguments regarding the extent to which the gains’ function is concave and/or the probability weighting function exhibits subcertainty”*.

In the same vein, Cardenas et al. (2014) examines the house money effect following the Binswanger (1980)’s and Attanasio et al. (2012)’s Ordered Lottery Selection design which consists of two sets of lotteries, one involving losses and

⁶Kahneman and Tversky (1979, 1981) have discussed the importance of framing in earlier work.

⁷This distinction between two-stage and one-stage framing of the question posed to participants will draw our attention again later on in Chapter 2.

Problem number	Format type	Initial Outcome	Sure Outcome	Gamble	Sample	% Risk-seeking
1.	2-stage	\$30.00	\$0.00	(\$9, 0.5; -\$9, 0.5)	C-MBA N = 44	70
		30.00	0.00	(\$9, 0.5; -\$9, 0.5)	C-MBA N = 75	82
		7.50	0.00	(\$2.25, 5, -\$2.25, 5)	C-BA N = 117	81
		7.50	0.00	(\$2.25, 5, -\$2.25, 5)	UBC-BA N = 68	72
	1-stage	—	30.00	(\$39, 0.5; \$21, 0.5)	C-MBA N = 46	43
		—	30.00	(\$39, 0.5; \$21, 0.5)	C-MBA N = 46	44

Table 1.2: Results from Experiment Using Hypothetical Questions

gains and another only involving gains. Their innovation lies in giving the cash⁸ to the treatment group three weeks in advance so that the respective participants feel as close to playing with their own money as possible. With their preferred specification they find a CRRA⁹ coefficient of 0.34 with a standard deviation of 0.09, and they observe that if participants in the treatment group spent 35% of their endowment their CRRA coefficient is higher than that of the control group by approximately 0.3 standard deviations. They interpret this as a small and indirect house money effect operating through the amount of the cash in advance that was actually spent.

1.3.1.2 Investment Decisions

Keasey and Moon (1996) extend the work of Thaler and Johnson (1990) on prior gains and losses by examining how these affect choices in a *business context*. Rather than making choices over lotteries participants were asked to make capital expenditure decisions. Their results found additional support for the house money effect as participants shifted behaviour towards risk seeking in situations involving prior gains.

Weber and Zuchel (2005) examine contradictory evidence from the empirical literature on how prior outcomes affect decision makers' risk attitudes. They examine both the house money effect and the escalation of commitment effect (that is, prior losses rather than prior gains inducing more risk-taking behaviour). Their experimental design involved both a portfolio decision and a two-stage betting game and helped them show that framing does matter. They actually found that decisions in the portfolio decision context are consistent with escalation of commitment whereas in the two-stage betting game setting decisions seem to follow predictions consistent with the house money effect.

⁸The cash was enough to cover the potential losses.

⁹CRRA refers to constant relative risk aversion.

The studies that have been discussed so far support the existence of the house money effect in the context of individual decisions involving risk. In all these experiments the objective was to examine how prior gains affect decisions under risk and evidence suggests that participants involved in those experiments exhibited more risk-taking behaviour after having experienced gains.

1.3.2 The HME in Games

There is also another branch of literature that focuses more on what we discuss in the introduction as the relevance of the house money effect to experimental economics, namely the changes in behaviour both in the lab and the field when ‘earned’ rather than ‘windfall endowments’ are used. Some of these studies within this branch of literature refer to the house money effect but some others rather talk about ‘endowment (or asset) heterogeneity’. This additional terminology is discussed here in this section, where we present research on the house money effect that has been done within the environment of bargaining games, charitable giving and social dilemmas. These studies focus on decisions that either do not involve risk at all (e.g. dictator game, charitable giving) or the risk involved is related to strategic uncertainty (e.g. trust game)¹⁰. The interesting bit of this branch of literature is that usually interprets the house money effect as a result of Lockean desert effects or fairness concerns.

1.3.2.1 Dictator (& Ultimatum) Games

Cherry et al. (2002) are among the first to “*argue that just as rewards must be salient (Hwan Baik et al. (1999)), the assets in a bargain must be legitimate to produce rational behaviour*”, and although claiming that this proposition had not been explicitly examined previously in bargaining behaviour, they specifically and clearly make the connection between their research on bargaining over earned wealth and the pre-existing and closely related work within economics and psychology on found-money effect and mental accounting¹¹.

Cherry et al. (2002) consider a dictator game where dictators bargain over their earned wealth rather than unearned windfall endowments. They show that dictators bargaining over their earned wealth were more self-interested than what was shown in previous studies. Moreover, dictators were eventually hardened; that is, they became even more selfish when complete anonymity was induced; more precisely, the other-regarding behaviour was essentially eliminated. They conclude that controlling for asset origin is no less important than controlling reciprocity in explaining other-regarding behaviour in simple (one-shot) bargaining games and that when assets are legitimised with effort and strategic concerns are controlled with isolation -meaning complete anonymity- other-regarding behaviour was the exception. This shows that the motivation behind the non-selfish

¹⁰Strategic uncertainty refers to situations where imperfect information over the likelihood of another person’s decisions is involved, as opposed to state uncertainty which is associated with perfect information over the likelihood of outcomes that often do not involve another person (Houser et al. (2010, p.73)). See also discussion in Chapter 2 for more on that.

¹¹See (Cherry et al., 2002, p.1218 footnote 1)

behaviour were strategic considerations rather than fairness and/or altruism.

Cherry et al. (2002) extend the work of Hoffman et al. (1996) who focus on how social distance affects other-regarding behaviour in dictator games and they show that as anonymity or social isolation are relaxed the offer distributions decrease as predicted. Hoffman et al. (1994) in an earlier work conducted dictator and ultimatum double blind experiments as well and they observed that if the right to be the first mover is “earned” (rather than the wealth) by scoring high in a general knowledge quiz, then first movers exhibit a more self-regarding behaviour opening the path for further research on the topic. Interestingly, the lower offers in the ultimatum games were not accompanied by higher rejection rates.

Cherry (2001) himself had earlier commented on mental accounting as a key determinant of other-regarding behaviour observed in bargaining experiments. He showed that dictators bargaining over earned money exhibited self-interest behaviour in 76% of bargains as opposed to 26% of bargains over allocated money and underlined that the earnings protocol may be an important option for future laboratory research as it can contribute to increase the external validity of such experiments by providing a closer correspondence between the laboratory and the actual (over personal assets) individuals’ decisions.

A few years later, Oxoby and Spraggon (2008) continued the above-discussed work on legitimising wealth in dictator games. They started with the conjecture that legitimising assets creates property rights which participants observe, regardless of who accumulates those rights. They tested this conjecture by running a series of dictator games - a baseline treatment where there were only wind-fall endowments; a dictator earnings treatment replicating Cherry et al. (2002); and a receiver earnings treatment¹² where receiver had earned the wealth and the matched dictator was called to decide on its allocation. They found that property rights, generated by legitimising assets with effort, play a crucial role in the revealed preferences of participants, as dictators who earned wealth made zero offers consistent with game theoretic predictions, while dictators facing receivers who earned the wealth allocate significantly more. They basically observe a dichotomous effect of earned endowments in the dictator game: an individual’s own entitlements to assets seem to dominate the fairness concerns characterised in outcome based models of other-regarding preferences (Bolton and Ockenfels (2000); Fehr and Schmidt (1999)), and others’ entitlements to assets seem to intensify each individual’s observed positive reciprocity characterised in intention-based models of fairness (Rabin (1993); Dufwenberg and Kirchsteiger (2004))¹³.

Cherry and Shogren (2008) confirm the results of both Oxoby and Spraggon (2008) and Cherry et al. (2002) but their main contribution is that they try to disentangle the two factors that might be driving the observed earned endowment effect, that is, the changes in relative deservingness and the legitimising of the endowment with effort. Their results provide evidence that the deservingness of

¹²This treatment - according to Oxoby and Spraggon (2008) - mirrors the Berg et al. (1995) trust game as the receiver’s exertion of effort indicates trust in the dictator not making a zero offer. More on trust games are discussed, though, in the next section.

¹³For a more detailed discussion on other-regarding preferences see also following section on interpretations of the house money effect and Chapter 3.

the recipient does matter regardless of the origin of the endowment and in those cases dictators made a greater offer to the respective recipient, but they also find that dictators that earned endowments, although showing some degree of sympathy to recipients who had not been afforded the same opportunity, offered significantly less than those with allocated endowments.

Bradley (1998) and Eckel and Grossman (1996) discuss the concept of deservingness. Bradley (1998) shows that in dictator games allocators (dictators) reward skilful recipients but punish unskilful ones only modestly though since they appear to consider effort as an appropriate measure of deservingness. The author also presents evidence on ultimatum games where offerers seem to adapt to the strategic environment, but yet when faced with skilful recipients they seem to be motivated by fairness concerns rather than by strategic considerations. Eckel and Grossman (1996) find a significant increase in offers when the recipient was generally acknowledged to be ‘deserving’. Their experiment consists of two treatments - in one of them the recipient is an anonymous student subject while in the other an established charity. In their discussion they argue in favour of the moral motivation theory of Hoffman et al. (1994) and they suggest its modification to include altruism. They also underline the importance of abstraction in the conduction of experimental procedures when theory is tested, but at the same time they recognise that since social and psychological factors are relevant in determining economic decisions, this abstraction might need to be abandoned to some extent in the investigation of particular topics such as other regarding behaviour.

On the same direction Rutstrom and Williams (2000) conduct an experimental study on distributive preferences. They examine whether individuals have non self-interested preferences over income distributions, and if so how these preferences depend on their perception of worthiness of compensation as indicated either by the amount of effort exerted or by productivity level achieved. They employ a modification of the standard dictator game. Subjects first participated in an assigned (individual) task that determined their initial income entitlement and were then brought together into groups of 12, where they were asked to choose their preferred final distribution of income within the group. A random dictator rule was applied to give equal chance to everyone involved to dictate the outcome and to eliminate any strategic considerations¹⁴. Their results strikingly favour the model of self-interest in individual decision-making since 99% of subjects choose the income distribution that maximised their own payoff. As an additional robustness check of their results they implement a random entitlement mechanism. They observe a statistically significant increase in the non-self-interested behaviour that is though not entirely consistent with earnings-based distributive preferences and in absolute magnitude is small.

In the literature the dictator game is often presented and examined within the environment of charitable giving¹⁵. Reinstein and Riener (2009) attempt to decompose the house money effect into two components: the tangibility and the

¹⁴The choice of the quasi-dictator game by Rutstrom and Williams (2000) was intentionally made to abstract from and control for strategic considerations in order to examine individual preferences directly.

¹⁵See also earlier discussion on work of Eckel and Grossman (1996).

desert effect. The former refers to the situation where people treat the money they are promised differently than cash that they actually hold and the latter to the situation where people may treat money that they earn differently than random windfall gains. In line with Eckel and Grossman (1996), they conduct their experiment in the context of charitable giving and the treatments vary according to the extent to which money is perceived as earned and secondly according to the tangibility of payment¹⁶. Their results support both the tangibility and the desert effect with the magnitude of the former appearing to be at least as strong as the latter.

Carlsson et al. (2009) take the discussion on the role of windfall endowments beyond the confines of the laboratory to the field experiment setting, in attempt to examine the limitations of lab experiments' external validity. Following Eckel and Grossman (1996), they use a dictator game where the recipient is a charity organisation. The charitable giving context allows them to keep all other factors such as stakes, subjects' pool, choice sets and time horizon constant whilst only varying the asset origin (windfall or earned endowments) and the environment (lab or field). Their results confirm previous research on asset origin in dictator games as they find a significant and substantial difference in donations when using windfall rather than earned endowments both in the lab and in the field. However, they also find a sizeable and significant difference in choices made in the lab and in the field and in particular with regards to windfall endowments¹⁷.

Mittone and Ploner (2006) continue the work of Cherry et al. (2002) by slightly modifying their experimental design. They focus on the *symmetry in effort* factor as they argue that not controlling for the impact of symmetry in effort might lead to a biased assessment of the relevance of anonymity and asset legitimacy in decision making. For this reason, they ask both the dictator and the recipient to participate in a real effort task as part of the treatment condition. In order to provide adequate incentives to exert effort, participants only received information about their type (either being a dictator or a recipient after they exerted effort in the task) after completing a quiz of 17 GMAT-type questions in the first stage of the game. Their evidence suggests that symmetry in effort induces higher offers than when only the dictator is involved in the real effort task. This result strengthens their argument that although asset legitimacy may play an important role in bargaining behaviour, its impact might be overestimated in some cases where the experimental design does not account for another implicit relevant issue, namely the "social reference point" that the dictators may assume as a benchmark for evaluating the degree of fairness in their decisions.

Surveys on dictator games and their modifications (e.g. charitable giving) seem to reach a consensus on the existence of what they call "found money effect" with dictator offers to be significant lower in all the related experiments when endowments (or the right to be a dictator) were earned rather than windfall endowed (randomly assigned) and it is very obvious from the discussion above

¹⁶That is, subjects were given the cash before they decided how much to donate; or they were allocated an endowment on screen and made their donation on a computer screen. They only received the cash at the end of the experiment.

¹⁷More precisely, lab experiments are upward biased but the difference is much smaller when the endowment is earned.

that the research has focused on issues of fairness and deservingness, on property rights and entitlements to explain this effect.

1.3.2.2 Trust Games

In contrast to the long literature on asset origin and property rights in dictator games, little has been done regarding trust games. To our knowledge, Fahr and Irlenbusch (2000) are the first to consider earned property rights in the context of a trust game. The main goal of their research is to examine whether behaviour of both the trustor and the trustee alters in the trust game when property rights are explicitly introduced by asking trustors and/or trustees to participate in a real effort task. They also test whether this behaviour is predicted better by the mindreading concept (Smith (1998)) or by the equity principle. They run three treatments: trustor who exerts effort matched with a trustee who does not; trustor who does not exert any effort matched with a trustee who does; and finally where both trustor and trustee exert effort and are matched together. They set out five hypotheses to be tested: (i) the stronger the property rights of the trustors, the higher the returns; (ii) the stronger the property rights of the trustors, the higher the investments; (iii) the stronger the property rights of the trustees, the higher the investments; (iv) the higher the property rights of the trustors, the higher their payoffs; (v) the stronger the property rights of the trustees, the higher their payoffs. Their results confirm the last two hypotheses implied by the equity principle and underline the robustness of similar results in previous experimental work. They also observe that trustees return significantly more money the stronger the property rights of the trustors, but they fail to reject the null hypothesis in favour of the hypothesis implied by mindreading that trustors with stronger property rights invest more in anticipation of the trustees' expected behaviour. Instead they observe that trustors unilaterally implement a fair outcome by investing more in the cases where trustees have stronger property rights.

Cox and Hall (2010) continue the work of Cox et al. (2009) on cooperation in private and common property trust games. They assign stronger property right entitlements by requiring participants to meet a performance target in the real effort task in order to earn their endowment. They find new evidence that under stronger property entitlements common property and private property trust games are not isomorphic. More precisely they observe that cooperation is lower in common property compared to private property trust games. This new evidence supports predictions of revealed altruism theory¹⁸.

Apart from the literature on trust games there is also some closely related literature that examines the 'mirror' image game of trust game, that is, the power-to-take game, also known as the taking game. In a two-player taking game the First Mover (FM) decides whether or not to take an amount of money from the Second Mover's (SM) endowment. If the FM decides not to take, the game ends and both players keep their endowments. If the FM takes money from the SM, the SM can retaliate in return.

¹⁸See Cox et al. (2008)

Bosman et al. (2005), extending previous work on emotional hazard in a power-to-take experiment (Bosman and Winden (2002)), examine if agents behave differently if their own earnings are at stake ('effort'), or if a budget is allocated to them by the experimenter ('no-effort'). Measuring emotions and focusing on their behavioural significance, they find that: (i) responders destroy more on aggregate, and more regularly, with no-effort; (ii) responders frequently choose an intermediate amount of destruction with no-effort, in contrast with the all-or-nothing finding for effort; (iii) takers' behaviour does not depend on effort; (iv) responders expect substantially lower take rates with no-effort; (v) both actual and expected take rates have a significant effect on the probability of destruction, both in case of effort and no-effort; (vi) and finally they underline that emotional factors explain these results.

In a much more recent study Danková and Servátka (2013) examine the house money effect and its impact on negative reciprocity. They explore the implications of windfall endowments on observed reciprocal behaviour, focussing specifically on negative reciprocity. They implement three treatments where in the first treatment SM's endowment is entirely 'house money'; in the second and third it consists of both 'house' and earned money. In one case the FM can take from the 'house money' part of the endowment and the SM can retaliate using his/her earned money part. In the other case, the FM can take the earned part and the SM can retaliate using 'house money'. They conjecture two reasons why the origin of endowment might matter for negative reciprocity: first, using earned money as opposed to house money might increase the costs of negative reciprocity due to this money being in a different mental account and thus lead to less retaliation, and secondly decreasing an endowment consisting of earned money might be considered a stronger violation of property rights and lead to more retaliation. Their results suggest that participants retaliate more in both cases, supporting the latter conjecture.

Closely related to this literature is an extensive literature on retaliation and punishment, mainly suggesting that an increase in the perceived cost of reciprocation/retaliation may diminish its frequency and/or extent. Although of some relevance to our discussion, due to spatial considerations we shall omit further details. Readers interested in this field may consult the following key papers within the literature: Ostrom et al. (1992), Fehr and Gächter (2000), Anderson and Puterman (2006), Carpenter (2007), Nikiforakis and Normann (2008), Nikiforakis (2008), Herrmann et al. (2008), Nikiforakis and Engelmann (2011).

Experimental evidence on both trust and take games, despite the many different incentives involved in their more complicated set up, also support the existence of the house money effect and the discussion on these studies again refers to entitlements and property rights. Danková and Servátka (2013) actually in their conclusion claim that despite the auxiliary evidence on their results that participants seem to distinguish between earned and 'house' money, this might not necessarily mean that they were using different mental accounts for each type of money.

1.3.2.3 Public Good Games

At the same time that the literature on asset origins in dictator games was emerging, Clark (2002) conducted research on -what he calls- an objection to experimental methodology that has received surprising little empirical attention: “. . . the disparate evidence that people treat small amounts of windfall unexpected money differently than they do their regular income.” Building on the literature of permanent income hypothesis and on that of mental accounting he underlines the relevance of “house money effects” for experimental economics’ methodologies not only in individual decisions but also in group decisions. He argues that house money effects are relevant in public goods settings in two ways. Firstly, in such group experiments subjects with windfall endowments may be more willing to spend them on non-pecuniary goods like altruism, fairness, revenge, etc., and secondly, subjects may choose unusually risky strategies. Although in the context of linear public good games risk preference seems irrelevant because of the existence of a dominant strategy, work by Kreps et al. (1982) and Andreoni (1988) suggests that subjects may strategically make positive contributions in early rounds of public good games in case of incomplete information regarding the types of other subjects, who might hold reciprocal norms and are uncertain about others’ types as well.

Following Isaac et al. (1984) and Andreoni (1995), Clark (2002) employs an experimental design consisting of two treatments of a Voluntary Contribution Mechanism¹⁹ (VCM). The VCM is repeated in each treatment for 10 times but the composition of groups of 5 changes across rounds. In one of the treatments, half of the subjects participated in the traditional way where their initial endowments for decisions were windfalls given by the experimenters (house money), while in the other treatment subjects had to bring their own money. Final wealth distribution was maintained identical across the two treatments by an additional payment ex post at the end of the experiment that had not been announced in advanced²⁰. Findings suggest a familiar VCM pattern where contributions start from below 50% and gradually decline over the rounds. Although mean contributions in the first round are slightly higher with house money rather than with own money, these differences in contributions are not statistically significant.

Harrison (2007) commented on Clark (2002)’s work, criticizing the statistical methods used in analysing the data and describing them as inappropriate. More precisely, with regards to both the first analysis with the session-specific means and the second analysis, which focuses on the first round contributions only, Harrison (2007) claims that problems derive from inappropriate metrics of evaluation which disregard information rather than the actual use of nonparametric tests. Harrison (2007) takes a closer look at the raw data, separates the subjects into pure free riders and those making positive contributions in both cases over each round. This reveals that the house money had a major effect on the fraction of free riders but no clear effect on the levels of positive contributions. For a

¹⁹The Voluntary Contribution Mechanism is another name of the well-known linear public good game.

²⁰Subjects had only been promised ex ante an identical distribution of earnings but were not explicitly aware of the additional payment.

more systematic examination, Harrison (2007) conducts a panel analysis of data by employing a population-averaged estimation method known as Generalised Estimating Equations. This method confirms what had already been shown by indicating a significant effect of the house money treatment on the propensity to completely free ride, but no effect on the level of contributions once the decision to contribute has been made. The house money effect is estimated to reduce the probability of positive contribution by 8.2 percentage points.

Cherry et al. (2002) had already set the agenda for further research with regards to asset origin affecting to some degree the anomalous behaviour in experimental settings by suggesting in their conclusion that, “*windfall wealth [...] might explain the lack of free-riding in the provision of public goods in the laboratory*” (p.1220). In later work of their own, Cherry et al. (2005) examine the impact of endowment origin and heterogeneity on public good contributions in the lab. Their results reject the hypothesis that positive contributions in linear public goods games are due to asset origin as subjects’ contributions were approximately equivalent regardless of whether their wealth was windfall or earned. They do however find a significant difference between contributions within groups of heterogeneous wealth and contributions within groups of homogeneous wealth, with the former being lower than the latter; this finding is independent of the origin of assets²¹. They find evidence of -what they call- an ‘anticipated reciprocity effect’ not present in the dictator games; according to which subjects with high endowments contribute less in heterogeneous groups. Their experimental design suggests two treatments: one with windfall wealth and one with earned wealth. In both treatments subjects are grouped into collectives of four and play a linear public good game only once. The earned wealth is induced at an earlier stage prior to the public good game in the sessions with earned wealth following the protocol of Cherry et al. (2002) and subjects are matched into either group where all members hold the same amount of initial wealth or each member holds each endowment level (10 – 40, heterogeneous case). Discussing their results and how they differ from Cherry et al. (2002)’s in the context of dictator games, Cherry et al. (2005) argue that public good games are more complex tasks for participants in experiments and that they require greater cognitive effort, concluding that the influence of asset origin could be context-dependent. These claims form both the motivation for and the core of Chapter 2 of this thesis.

Kroll et al. (2007) continue the earlier work of Cherry et al. (2005) on the impact of asset origin and heterogeneity. Arguing that the ‘anticipatory reciprocity’ effect might overshadow the endowment origin effect, they suggest examining the origin and heterogeneity effects in the context of a best-shot public good game²². In such games the behavioural motives of anticipatory reciprocity should be eliminated as only the highest contribution sets the level of provision of the public good, whereas the lower contributions are for nought since they only serve to decrease the payoffs of the contributors. Their experimental design is identical to the one in Cherry et al. (2005) with the only exception being that the linear

²¹This result, although still apparent, weakens when contributions as percentage of endowments were considered.

²²This game was first introduced by Hirshleifer (1983).

public good game is now replaced by the best-shot public good game. Their results indicate that in agreement with Cherry et al. (2005) endowment origin does not have an effect on (either relative or absolute) contributions in the symmetric setting of homogeneous endowments. In the heterogeneous endowments treatments, although overall mean contribution levels show no statistically significant difference between earned and windfall endowments, a closer look reveals what aggregation hides: it appears that groups receiving windfall endowments behave as predicted by the focal-point Nash equilibrium, that is, the member with the highest endowment contributes almost everything while the other three members contribute little to nothing. On the other hand, in groups where endowments are earned subjects with the highest endowments seem to contribute less while the other group members seem to contribute more. Further comparisons with symmetric cases lead the authors to conclude that “...any endowment-origin effect depends on asymmetric issues such as heterogeneous endowments and best-shot production technologies(p. 425). Interpreting their results, Kroll et al. (2007) comment on the trade-off that exists in a best shot game between preferences for fairness and preferences for efficiency, in addition to preferences for self-interest; and observe in their findings that with windfall endowments, there is no specific information about what is more ‘fair’ among Nash equilibria and for that efficiency concerns seem to prevail while with earned endowments, fairness consideration rise and the efficient outcome might not be considered fair. They also in the same vein with our work underline how context can affect competing preferences.

Oxoby and Spraggon (2009) consider a two-person public good game with heterogeneity in the source of participants’ endowments. More precisely, they match participants into pairs in three ways: (i) both with earned endowments; (ii) both with endowed endowments by the experimenters; and (iii) one with earned and one with endowed. The public good game is the typical one shot linear public good game but participants have to make an unconditional and a conditional on what the other group member will do- contribution. Following Fischbacher et al. (2001) and Keser and Winden (2000)²³, they make the conditional contribution incentive compatible by making it relevant for one of the participants in each pair after both participants have made both types of decisions²⁴. They find an ‘inverse found money effect’ in which participants who earn their endowments and are matched with someone who does not earn their endowments are more unconditionally and conditionally cooperative. They attribute this result to ‘anticipatory reciprocity’ as introduced and discussed by Cherry et al. (2005) and Kroll et al. (2007). According to ‘anticipatory reciprocity’, subjects who earn their endowment seem to expect those who receive windfall endowments to contribute more and as a result they contribute more themselves. Similarly, subjects with windfall endowments expect those with earned endowments to contribute less and thus, they do so themselves.

In a more recent paper, Muehlbacher and Kirchler (2009) motivated by the inconclusive evidence of the effect of earned (by effort) endowments in public

²³We also follow the experimental design of Fischbacher et al. (2001) and Keser and Winden (2000). See more on that in Chapters 2 and 3.

²⁴For more on the strategy method see Experimental design section in Chapter 2.

good games, conduct an additional experiment where, in contrast to the majority of previous studies, the participants are not informed about the asymmetry of the origin of the endowments within their groups. The idea behind this experimental design is to test the “pure” effect of effort on cooperation. Their results indicate that participants who earn their endowments by exerting a high level of effort contribute less to the public good than those participants who receive their endowments more easily in the low effort condition of the first stage of the experiment.

Although at first sight evidence regarding the house money effect in public good games seems mixed and sometimes contradicting, reading the related literature closer reveals that results are in accordance with those we have seen so far in bargaining games. The house money effect is context-dependent and more evident in asymmetric situations. At this point it is interesting to make two comments: first, earlier games (both dictator and trust games) that we have discussed involve asymmetry in decisions of the counterparts as well and second, as opposed to dictator games where the house money effect intensifies self-interest preferences (in accordance with game-theoretical predictions), in the case of public good games, the house money effect seems to favour fairness preferences, suggesting behaviour that diverge from predictions of Nash equilibrium.

1.3.2.4 Markets (& Auctions)

In the last part of this section, we present studies that examined the house money effect in the context of markets or some of them more specifically in the context of an auction. We chose to present this literature under the general category of games that we have set up as strategic considerations are involved, but as (both the findings that we will further discuss in Chapter 2 and) the competitive nature of these settings -which seems to rule out social preferences- brings them close to that literature on house money effect in individual decisions.

Following the theoretical work of Barberis et al. (2001), Ackert et al. (2006) attempt an experimental investigation of the house money effect in a multi-period (dynamic) financial market setting. Barberis et al. (2001) show that investors who derive utility from consumption and changes in financial wealth and are loss averse, are less risk averse after an increase in stock prices because gains cushion subsequent losses whereas after a fall in stock prices, investors become more risk averse as they are concerned about further losses.

Ackert et al. (2006) conduct nine experimental sessions, each consisting of a series of markets where eight participants in each market compete via a sealed-bid Vickrey auction to acquire an asset of one-period life that pays a dividend of \$40 or nothing with equal probability. Participants are endowed either with low endowments (\$60) or high (\$75). Their experimental hypotheses are that the market price is higher when traders’ endowments are larger, and that the average price is increasing in the wealth of the market. Their empirical results indicate that the house money effect persists across trading periods. They find a significant difference between the low and the high endowments groups and show that market prices are higher when traders have more found money.

As with the prices, bids also appear higher in the high endowment sessions and

this result is also persistent across trading sessions. As they ask the participants-traders to predict the average price in each market, they present also the predicted price in the low endowment sessions; the results show the predicted price to be lower than the high endowment sessions in all trading periods. This result indicates that traders expect lower bids when endowments are lower. They also report that although they were able to document a significant house money effect in their setting, they are not able to show that subsequent changes in wealth had an effect on market pricing. They actually find that changes in average wealth lasts for more than one period in the low endowment sessions but they have no significant effect in the high endowment sessions. Results are similar when they consider the traders' bids.

In an attempt to explain this inconsequentiality of the wealth changes, they cite Knetsch et al. (2001) who argue that traders in repeated trials may adjust their bids toward a margin following a kind of peer pressure and lack of confidence in their own valuation. Ackert et al. (2006) claim that the house money effect in their context might have been concealed by these effects.

In order to eliminate these peer pressure effects, Chakravarty and Ma (2009) consider a BBecker et al. (1964) bidding mechanism instead. They run four distinct trading sessions with 28 subjects. Each subject participates in one session consisting of six treatments. The treatments vary in terms of initial endowments (\$60, \$75 and \$120) and the number of bidding periods (3 and then 6). Their experimental design results in poor evidence of the house money effect as they only find the existence of a short-term house money effect that is related only to the initial endowments. They also report that changes in wealth had no effect on risk-taking behaviour and for that they argue that expected utility theory seems to survive in the laboratory and people's risk-taking behaviour is driven by rational inference rather than irrational exuberance.

Returning to the discussion on the equity principle, according to which a person's earnings should be in proportion to his/her inputs, Cason et al. (2011) examine whether real-effort investments made by sellers that determine buyers' values or sellers' costs can affect prices in a competitive experimental market. Starting from the experimental result (Smith (2012)) that subjects in the laboratory appear to trade at prices remarkably close to those theory predicts for the competitive case, they differentiate their design by introducing real effort made by sellers rather than exogenous costs and values, thus ensuring that equity concerns are now relevant to the determination of prices in the competitive experimental market examined.

The experiment consists of three treatments: the baseline (replicating earlier research); the costs treatment, where sellers' production depends on their relative performance in a tournament and on a random productivity shock; and the values treatment, where sellers' effort can increase the values of buyers, generating potential efficiency gains. These investments in effort do not affect the competitive equilibrium resulting from own-payoff maximisation, but prices are expected to be higher in the costs and values treatments than in the baseline if equity considerations are present. Previous experimental research (Franciosi et al. (1995)) has shown that fairness concerns have an effect on market prices but 'fairness' in those studies refers to preferences for equality not equity. Although they include

certain features²⁵ in their design that increase the likelihood that real effort will affect market outcome, they find no significant difference between the three treatments. They argue that their results suggest that competition seems to eliminate any equity concerns that traders might have.

Rosenboim and Shavit (2012), using a “prepaid mechanism” to overcome ethical problems and selection biases for participants using their “own” money, run three different experiments. In the first one, participants are asked to make choices between lotteries that involve positive and negative outcomes. In the second one, they participate in a second price auction and in the third one they face a decision problem similar to the one a first mover faces in a trust game. In all cases they observe that participants who use prepaid money exert greater effort to reduce any possible losses than those participants who receive their endowments on the spot. They do observe though that when losses are not involved or were not possible, prepayment had no effect and results were similar to those with the on-the-spot mechanism.

All the above studies suggest that house money effect is not (strongly) evident in the contexts of markets. These findings help us to take our discussion on the interpretation of the house money effect a step further in the next section and also motivate our work in Chapter 2.

1.4 The Interpretations of the HME

In this section, we present a short summary of the literature behind the two most prevailing interpretations of the house money effect. Some scholars consider the house money effect to be a result of mental accounting. Particularly, as we see in the next subsection Thaler and Johnson (1990), using the concept of “mental accounting” as used by Thaler (1999), that is, the process of coding, categorising and evaluating events, find in their experiments that following a gain, losses (smaller than the original gain) can be integrated with that, moderating the influence of loss aversion and facilitating risk-seeking. That implies that people will tend to risk more when making decisions with windfall endowments (as they perceive these as a gain and they tend to integrate smaller subsequent losses) rather than with earned endowments, which are not perceived as a prior gain.

The other interpretation of the house money effect ascribes it to fairness or ‘just desert’ concerns. More precisely, other scholarly studies suggest that people treat windfall endowments differently than earned endowments, as the later involve entitlements earned by exerting effort and for that fairness and deservingness concerns arise. In this case differences in behaviour with windfall and earned endowments will be driven by how people perceive entitlements over their endowments and -as we have already seen in the previous section- also over the endowments of others in the context of games.

These two interpretations of the house money effect are not necessarily competing. In some cases they complement each other on explaining changes in

²⁵Posted-offer trading institutions, market supply and demand that increases considerably earnings’ inequality between sellers and buyers, public information, etc. (for further details see Cason et al. (2011), p.2 3).

behaviour when earned assets are used rather than windfall endowed. In fact, in Chapter 2 we suggest an experimental design that aspires to disentangle these two factors driving the house money effect, namely changes in risk behaviour and deservingness and fairness concerns.

Here, we first briefly present the concept of mental accounting and how that relates to the house money effect and in the second part, we shortly discuss the literature on other-regarding preferences that involve desert and fairness considerations.

1.4.1 Mental Accounting

Mental accounting emerged as a concept as part of the broader discussion regarding expected utility theory's perceived failure as a descriptive model of decision making. Thaler (1999) who first named mental accounting summarises all the work related to that and concludes that "mental accounting matters". He defines it as a set of cognitive operations that individuals (and households) use to organise, evaluate and track their financial activities. There are three important components of mental accounting: (i) how outcomes are perceived and experienced and how decisions are made and evaluated subsequently; (ii) the assignment of financial activities to specific accounts; and (iii) the frequency with which these accounts are evaluated; and each of them violates the economic principle of fungibility, that is, money in one mental account is not a perfect substitute for money in another mental account. For instance, the house money effect is interpreted in this framework as following: windfall endowments belong in one mental account and earned endowments in another one.

Regarding the first important component of mental accounting, Thaler (1980, 1985) assumes that people perceive outcomes in terms of the value function of Kahneman and Tversky (1979) prospect theory, which has three important features: (i) the value function is defined over gains and losses relative to a reference point. The focus is on changes rather than in final states as in expected utility theory; (ii) the gain function is concave and the loss function is convex suggesting diminishing sensitivity; and (iii) losing hurts more, implying loss aversion. Kahneman and Tversky (1981) defined a mental account as a frame for evaluation, "an outcome frame which specifies (i) the set of elementary outcomes that are evaluated jointly and the manner in which they are combined and (ii) a reference outcome that is considered neutral or normal". Thaler (1999), when using the term 'mental accounting', differs somewhat by referring to the entire process of coding, categorising and evaluating events. Kahneman and Tversky (1984, p. 347), propose three ways that outcomes might be framed: in terms of a *minimal account*, a *topical account*, or a *comprehensive account*. Considering two alternatives, using the *minimal account* implies examining only the differences between the two choices, disregarding all their common features. A *topical account* relates the consequences of possible options to a reference level that is determined by the context within which the decision arises. A *comprehensive account* takes into account all other factors including current wealth, future earnings, possible outcomes of other probabilistic holdings, and so on. Framing has an impact on choices in the real world as people make decisions piecemeal, influenced by the

context of the choice.

Thaler (1985) suggested an editing rule on how people combine financial outcomes (within a single mental account). Assuming that people do so to make themselves as happy as possible and given the shape of the value function, he proposed the **hedonic framing** which entails the following principles:

- i) Segregate gains (concave gain function).
- ii) Integrate losses (convex loss function).
- iii) Integrate smaller losses with larger gains (to offset loss aversion).
- iv) Segregate small gains (silver linings) from larger losses (because the gain function is steepest at the origin, the utility of a small gain can exceed the utility of slightly reducing a large loss).

More formally, the hedonic editing hypothesis is summarised in the following formula:

$$x \& y = \max[\nu(x + y), \nu(x) + \nu(y)]$$

where $\&$ denotes the cognitive combination of the two outcomes x and y .

As we have already seen when discussing the seminal paper of Thaler and Johnson (1990) on the house money effect, the *Hedonic* editing hypothesis has been contradicted by the findings in their experimental data. More precisely, the *Hedonic* editing rule was supported by experimental evidence in the domain of gains but in the case of losses evidence suggested that separation of losses was preferred to integration. A modification of that rule named *Quasi-Hedonic* editing rule was then suggested as an alternative. The intuition behind the *Quasi-Hedonic* editing hypothesis was that risk aversion can be observed after prior losses because subsequent losses are not integrated with the prior ones, while in the presence of prior gains, the opposite expected, that is, following a gain, losses which are smaller than the original gain can be integrated with that, moderating the influence of loss aversion and facilitating risk-seeking. The second part regarding prior gains was named house money effect.

1.4.2 Entitlements, Fairness and Desert

The other possible explanation suggested for the house money effect was that of fairness and deservingness concerns. Experimental research has very early underlined the relevance of ‘fairness’ or ‘justice’ for economic decisions. The literature on other-regarding preferences is very extensive²⁶ and it is not our purpose here to present an exhaustive review of that literature. We would rather focus on discussing studies that favour and support an interpretation of the house

²⁶We advise the reader interested in these topics to refer to Konow (2003)’s exceptional review on justice theories, including those that have been presented and tested experimentally not only in economics but also in psychology, political science, sociology, philosophy and other related fields.

money effect as the result of fairness and deservingness concerns, which is related to what is known as the ‘Lockean desert effect’.

Hoffman and Spitzer (1985) conducted an experiment regarding distributive justice, that is, how participants perceive fairness, entitlements and rights. They make the distinction between entitlements and rights, referring to the former as legally enforceable claims while the latter as morally justifiable entitlements. Their study involves bargains between two participants with opposing payoff functions and full information regarding the other’s payoffs. Participants choose between a non-cooperative (keep \$12 and give nothing to the other) and a cooperative outcome (get \$14 from the experimenter and split it according to a mutually agreed-on manner). The participant who makes the decision in each bargain is determined by a coin flip. The results show that all of the subjects choose the efficient outcome of joint-profit maximisation; but the striking result is that all of them divided the \$14 equally. Hoffman and Spitzer (1985) rationalise these results by suggesting that participants behaved ‘as if’ they chose a payoff distribution according to their own perception of fairness.

In this particular case, distributive preferences imply that participants seem to find luck (the flipping of a coin) not a ‘just’ way of allocating unequal property entitlements. By varying the experimental institutions they test three major families of theories of justice: utilitarian, egalitarian and natural law or desert. The evidence they collected supports the Lockean theory of earned desert. Despite maximising joint profits across all treatments, participants seemed to acknowledge ‘earned’ entitlements and for that reason the frequency of self-regarding versus equal payoff divisions varied depending on the methods used for assigning entitlements.

Kahneman et al. (1986) discuss entitlements in the market and ‘fairness as a constraint on profit seeking’ in what Konow (2003) describes as ‘the most widely cited descriptive study of justice in economics’. They use a household survey of telephone interviews in Canada to identify standards of fairness regarding prices, rents and wages and to consider the possible implications of those standards to market outcomes. They consider three determinants of fairness: the reference transaction which is “*a relevant precedent that is characterized by a reference price or wage, and by a positive reference profit to the firm*”(p.729); the outcomes for the firm and the transactors; and the occasion for the action of the firm. Their results are summarised in the principle of dual entitlement that they propose, which states that: “*Transactors have an entitlement to the terms of the reference transaction and firms are entitled to their reference profit. A firm is not allowed to increase its profits by arbitrarily violating the entitlement of its transactors to the reference price, rent or wage. When the reference profit of a firm is threatened, however, it may set new terms that protect its profit at transactors’ expense*”(pp. 729–730).

Burrows and Loomes (1994) also argue in favour of the ‘Lockean desert’ in the two-person trading environment that they examine. They consider their results as an updated version of Lockean desert theory since it refers to a ‘two-part desert’, that is desert that derives not only from the effort that produces the initial entitlements but also from the effort exerted in the bargaining process itself.

Apart from the experimental also the theoretical literature has responded and tried to incorporate fairness in the analysis of economic decisions. Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) assume that people care about ‘fairness’ and they define this ‘fairness’ concern as inequity-aversion, that is, the dislike for unequal payoff distributions.

Besides these outcome-based models of other-regarding behaviour, other approaches that focus on intentions of economic agents have been developed. The most influential paper in this regard is by Rabin (1993) which incorporates the concept of reciprocity and conditional other-regarding behaviour. Dufwenberg and Kirchsteiger (2004) and Falk and Fischbacher (2006) have extended Rabin’s work to extensive form games. Charness and Rabin (2002), motivated by the confoundedness in explanations that models of other-regarding preferences had suggested, present a model that embeds difference aversion, social-welfare preferences and other preferences (such as reciprocity).

Frohlich et al. (2004) extend the Fehr and Schmidt (1999) model to incorporate ‘the just deserts’ for those cases where individuals were involved in production or work. Fehr and Schmidt (1999) in their original work focus on explaining equity concerns in a public good setting but Frohlich et al. (2004) focus on dictator games. Let d represent the dictator and r the recipient, and let the total money received by the dictator for distribution be \mathbf{x} . Then in any distribution, $\mathbf{x} = x_d + x_r$: the sum of the amount kept and the amount redistributed. In the experiments where production takes place, the amount produced \mathbf{p} (in monetary terms) is the amount to be distributed, being the summed production of the paired individuals: $\mathbf{p} = p_d + p_r$. Hence, $p_d + p_r = x_d + x_r$. In a typical dictator game without production, the Fehr-Schmidt model characterizes the dictator’s utility of allocating x_d to herself as follows:

$$U(x_d) = x_d - \alpha \max(x_r - x_d, 0) - \beta \max(x_d - x_r, 0)$$

Given that this environment involves a unilateral decision by the dictator and that $\beta < \alpha$, either β is sufficiently large to motivate a 50/50 split, or it doesn’t affect behaviour at all.

The critical value is $\beta > 0.5$. Introducing ‘just deserts’ and continuing to assume linearity the dictator’s utility will now look like:

$$U(x_d) = x_d - \alpha \max(x_r - x_d, 0) - \beta \max(x_d - x_r, 0) - \gamma \max(p_d - x_d, 0) - \psi \max(p_r - x_r, 0)$$

where the two additional terms represent the cost to the dictator of not taking his/her own ‘just deserts’ and the cost to the dictator of not giving to the recipient his/her ‘just deserts’. The coefficients for the ‘just dessert’ arguments, γ and ψ , differ to those of equity in the sense that they do not involve the element of reciprocity that leads to the 2 for 1 multiplier found in β . In the cases with production, the other-regarding behaviour will emerge when both β and ψ are below 0.5. Similarly with equity concerns, they assume that the individual is more concerned about fairness to self than to others. Thus, apart from $\alpha > \beta$ they also assume $\gamma\psi > 0$. Figure 1.1 summarises the relationship between decisions and norms.

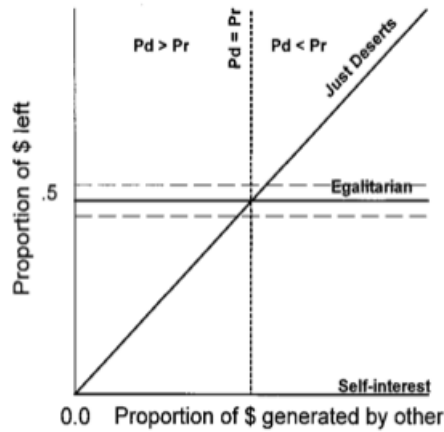


Figure 1.1: The Relationship between Behaviours and Norms

Moreno Garrido and Rodriguez Lara (2012) extend the work of Frohlich et al. (2004) to capture the accountability principle, which claims that subjects should only be rewarded for factors under their control. This fairness concept does not hold subjects responsible for factors beyond their control in the production of the surplus.

The studies discussed in this section relate to the house money effect in two ways: first experimental literature has documented considerations for property rights and entitlements while theoretical literature has suggested ways to model these preferences of fairness and desert. In that way, the house money effect can be explained as a result of this kind of preferences. The intuition behind this explanation would be that the “social reference point” that people may assume as a benchmark for evaluating the degree of fairness in their decisions might be different depending on how entitlements on endowments were induced (earned endowments vs. windfall endowments).

1.5 Conclusion

This chapter organises and provides a survey on the literature on the house money effect. Apart from the profound two questions of whether people treat money differently depending on its origin and the implications of the house money effect for the experimental methodology in economics, this review tackles two more research questions: first, what drives the house money effect and second, what its context-dependency can say regarding this first question.

Bearing these questions in mind, the literature on the house money effect has been divided into two main categories: papers that examine the house money effect in the context of individual decisions, and those that do the same but within the environment of a game, where social preferences may emerge. This is our first step towards our two goals: to document how contextual differences can amplify (or weaken) the house money effect and how different contexts might favour different interpretations of the house money effect.

Regarding the first goal, we hope that our work has contributed not only to present an extensive review of related studies on the house money effect but also with the help of our organisation criterion to highlight important features related to the house money effect across experimental settings. We believe that our second goal regarding contextual differences and how these can favour different interpretations of the house money effect has also been achieved. Although we aspire to give a more complete answer to the question of what drives the house money effect in the following chapter we believe that it is already obvious from our earlier discussion here that the house money effect seems to be explained better as the result of a different mental accounting over windfall gains in situations where people face individual decisions²⁷ whereas in situations where social preference may emerge the house money effect seems to appear as a result of fairness or ‘just desert’ preferences.

²⁷That also involves competitive market environment.

Chapter 2

Re-examining the House Money Effect

2.1 Introduction

A cursory glance at the literature on the house money effect typically leaves a somewhat muddled impression of conflicting findings. This seemingly convoluted state of affairs in the literature is what motivated the idea for this study. In Chapter 1 we have already pinned down two important features of the house money effect: its context-dependency and the different interpretations it has received. This in turn has enabled us to systematically document the differences across contexts, thus providing a useful first step towards uncovering what drives the house money effect.

This question of what drives the house money effect is at the core of our analysis in this chapter. We employ a novel experimental design to examine this research question that employs a within-subject approach, in tandem with the use of three different contexts of economic decisions. Both the within-subject experimental design and the three contexts of economic decisions allows to better test the two most prevailing interpretations of the house money effect; namely that it is the result of different mental accounting over windfall gains, and the idea that it is a result of fairness or ‘just desert’ preferences. To our knowledge, this is the first study on the house money effect using a within-subject design, and although many scholars have argued in favour of the idea that the house money effect is context-dependent, there is only one study, that of Rosenboim and Shavit (2012), which examines and compares the findings on the house money effect across different contexts¹.

Regarding the different contexts we employ in our experiment, we opt for based on our analysis in chapter 1- an individual decision setting and two group decision-like games. Our individual decision setting involves choices between a certain amount and a series of all-or-nothing lotteries, while the group decision-like games consist of a trust game and a public good game. The choice of these three contexts, apart from the apparent purpose to examine context-dependency of the house money effect, also facilitates the examination of more specific ques-

¹We refer to their experiments and findings and compare them with ours later in this chapter.

tions.

Firstly, our individual decision setting allows us to measure the degree of risk aversion of our participants using a Holt and Laury (2002)-like scale and test directly the hypothesis that different sources of endowments (house money vs. earned money², thereon HM vs. EM) change the risk behaviour of participants, as this stems from the definition³ of the house money effect by Thaler and Johnson (1990). We also do the same, namely testing for the presence of the house money effect, within the trust game and the public good game environments.

This risk aversion measure has also a second role as the way it was designed allows us to directly compare the individual decision over lotteries with the decision of the first mover in the trust game. We are able to do so show as we have designed the choices in the individual decision to map exactly the decision that the first mover in the trust game faces. The only difference between the two decisions lies in the fact that in the decision over lotteries participants face state uncertainty as nature (luck) determines the outcome and their payoff while in the trust game first movers face strategic uncertainty as their outcome and payoff will be determined by the decision of the respective second mover.

The public good game enters the discussion - and facilitates our analysis - as a game where although social preferences may emerge as in the trust game, the crucial difference lies in the existence of a dominant strategy, namely free-riding, which renders risk preferences irrelevant to the decision⁴. In this way, we have three different types of decision: one of them (decision over lotteries) involves only risk; the other one (decision in the public good game) involves only social preferences; and the last one (the first mover's decision in the trust game) involves both risk and social preferences. By examining the presence (or not) of the house money effect across these three contexts, we try to disentangle the two factors that might be driving the house money effect, namely changes in risk behaviour and deservingness or fairness concerns.

Our experimental data confirm the presence of the house money effect both in the decision to trust (but not in the decision of trustworthiness⁵) in the trust

²As we have already seen in Chapter 1 terms are used interchangeably throughout the literature. For instance, 'house' money, found money or 'windfall' endowments and respectively, earned money or endowments.

³See Chapter 1 for the exact definition.

⁴In a workshop presentation at the School of Economics of the University of Edinburgh, colleagues, among other useful comments, suggested that instead of the public good game we could have used a simpler game that involves only social preferences like the dictator game. We consider this a useful suggestion that might be considered for future research on the topic. In defence of our choice of the public good game, first the experiment discussed here is in this chapter is part of a larger experiment. The other part of the experiment is the central topic of Chapter 3 and for the analysis there the public good game was indispensable. Secondly, given that the trust game is considered as a sequential prisoner's dilemma while the public good game as a generalization of the prisoner's dilemma, similar incentives are in action and social preference that may emerge can be driven in both games by fairness or reciprocity. Note that also preferences for efficiency in these two games as Nash equilibrium is not Pareto efficient in both cases. Finally, we admit that the public good game was in the centre of our experimental design from the very beginning as the motivation for this study was the confoundedness in the experimental findings for the house money effect in the public good games.

⁵The literature usually refers to the decision of the second mover in the trust game as

game and in the decision to contribute in the public account within the public good game. However, our findings do not support the hypothesis of changes in risk behaviour of participants due to different sources of money, suggesting that risk attitudes are robust to different types of money used along the experiment. This result, apart from providing further support for the argument of context-dependency of the house money effect, sheds new light on the relative applicability of the two competing explanations of the house money effect. In fact, our Holt and Laury (2002)-like risk measure only serves to assist in explaining the decision of trust in the trust game (TG, thereon) and the positive contributions in the public good game (PGG)⁶. Therefore, our findings seem to favour interpretations of the house money effect as a result of ‘just desert’ or fairness preferences rather than a consequence of different mental accounting over windfall gains⁷.

The rest of the chapter is organised as follows: we skip any discussion of the related literature given that those most relevant to our work have already been presented in the first chapter⁸; rather, we begin with presenting our experimental design, and in the following section we present and discuss our results while the last section concludes.

2.2 Experimental Design

2.2.1 Experimental Tasks & Games

Each session of the experiment consisted of six economic decisions tasks and one quiz task; all of them referred as ‘situations’ in the instructions⁹. A trust game, a set of lotteries and a public good game were played twice and a real effort task was played once. The within subjects design required two kinds of sessions in ordered to control for order effects. In the one kind of sessions participants were first asked to make decisions in 3 tasks using money endowed by the experimenter (‘house money’, thereon HM), they then participated in the real effort task to earn the money they would use when making decisions in the other 3 tasks (‘earned money’, EM; HMEM session). In the other kind of sessions, they first had to earn the money in the real effort task, which they used in the decisions in the 3 first tasks and for the rest of the tasks they were endowed with HM by the experimental team (EMHM session). The following figure describes the timeline of games and tasks that participants attended in each kind of session respectively.

Participants were informed that they would participate in more than one situation. They only received instructions for each situation by the experiment just

‘trustworthiness’ while ‘trust’ refers to the decision of the first mover.

⁶This result though is consistent with results in existing literature where no predictive relationship found between risk attitudes and decisions in trust. (see Houser et al. (2010) and discussion in Section 5)

⁷See the discussion of Thaler and Johnson (1990)’s Quasi-Hedonic editing rule in Chapter 1.

⁸We do discuss though some additional relevant studies in our discussion section.

⁹More precisely, the six games were referred to as Situation 1, 2, 3, 4, 5 and 6 respectively, whereas the real effort task as Situation *. The reason for this distinction will be made clearer later on in the description of the experiment.

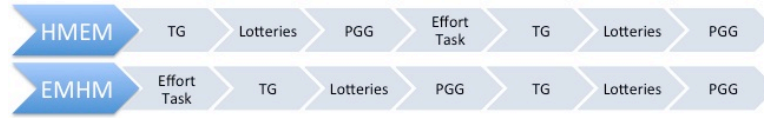


Figure 2.1: Experimental Sessions

before the beginning of the relevant stage. Information about their performance in each stage was only presented to them by the end of the experiment in a summary screen. The matching protocol was strangers and the participants were informed about that. More specifically, participants were paired and grouped with other participants in the room in some of the situations but the composition of groups and pairs was never the same along each session and that was explicitly stated in the instructions. We chose the strangers' matching protocol in an attempt to reduce any reputation, history and implicit punishment spill overs.

We also tried to avoid any income/hedging effects by informing participants about the outcome of their decisions in each situation at the end of the session and by paying them only one of the situations (not the Situation *) they made decisions in. The computer randomly chose one of the situations for each participant. The outcome of that particular situation determined the final payment of each person¹⁰.

2.2.2 Procedure

Participants arrived in the lab, confirmed their registration number, picked a card with a number assigning them to the respective computer and signed a consent form¹¹. They were also asked to answer a short questionnaire -mainly with demographic related questions¹² - in pen and paper. The experiment was fully computerised. It was programmed and conducted with the software z-Tree (Fischbacher (2007)).

The general instructions and the instructions¹³ for each of the tasks were distributed to participants in paper and were also read out loud. The reading

¹⁰Note that the final payment included also the show up and the participation fee.

¹¹This was a general consent form where they were agreeing to be members of the subjects' pool of the BLUE and accepting the general rules of participating in any experiments in the laboratory.

¹²Most of the information collected by this short questionnaire is reported in the following section.

¹³Experimental instructions are included in the Appendix A.1.

of the instructions was accompanied by a power point presentation¹⁴. Some additional instructions were added in each of the screens along the experiment. Participants were welcomed to ask questions for any confusion regarding the instructions.

The experiment lasted approximately one and a half hour and no communication was allowed during the session. When the session finished participants were asked to remain seated up to be called -by the number of their respective computer- to get paid in the next door's room. The participants signed a receipt, which has been kept in records by the lab administration and left the laboratory with their payment in an envelope. Only the lab administrator knew participants' names and exact payment and only for administrative purposes. The experimental team remained in the laboratory while the payment was taking place for each participant, only to invigilate the process of one participant per time going for payment next door according to the number on their computer.

The laboratory has 18 computers separated by partitions that ensure privacy to participants when they make decisions. We could only use up to 16 due to limitations imposed by the experimental design. As it is described below, one of the decisions¹⁵ required groups of 4 participants.

2.2.3 The Origin of the Money - The Real Effort Task

The general idea of the real effort task is to assign stronger property right entitlements on money by requiring participants to meet a performance quota to earn their endowments¹⁶. The real effort task consisted of 25 puzzles (see the appendix for examples of these puzzles) that were taken by an online IQ test¹⁷. Participants were told that they had to solve correctly at least 10 of these puzzles in order to receive £10 that they would be using in subsequent situations. The puzzles were relatively easy (although not trivial) and the quota of 10 out of 25 was purposely set low to ensure that everyone would succeed and earn the £10 of EM. In that way we avoided any sample selection and attrition. We also avoided to award them proportionally for each puzzle solved correctly in order to eliminate heterogeneity in endowments that would complicate our analysis further. None of our participants failed to achieve the quota required in the task. It was very explicit to the participants that the money earned in this task it would not be directly payable to them but rather it would be used in subsequent situations. For that reason, the real effort task was called 'Situation *' throughout the experiment to distinguish it from other situations which were numbered chronologically.

To summarize the process, the three decision tasks were played twice in each session. In each situation, participants were using either £10 that they were credit in their account by the experimental team (HM) or £10 that they had earned by performing the real effort task (EM). In each situation it was made

¹⁴The power point presentation is also available in the Appendix A.2.

¹⁵Recall that decisions were called 'situations' throughout the experiment to avoid any priming

¹⁶Other ways that have been used in the literature to induce 'own' money are discussed in the later discussion section.

¹⁷IQ Test Questions source: <http://www.iqtest-center.com>

explicit to the participants which kind of money they would be using.

2.2.4 The Trust Game¹⁸(Following Berg et al. (1995))

The game in this situation is the well-known trust or investment game. We moderate the original game by Berg et al. (1995) by allowing only for binary choices both for the trustor ('first mover') and the trustee ('second mover'). The trustor has to decide between trusting ('transfer £10') and not trusting ('keep £10') and the trustee between reciprocating¹⁹ ('transfer back £15') and not reciprocating ('keep £30')²⁰.

More precisely in the experiment, participants were paired with other participants in the room without knowing who they are matched with but knowing that they have not been and/or will not be paired with that person before/again. Each pair consisted of a First Mover (FM) and a Second Mover (SM). The allocation of roles was determined randomly by computer tossing a coin but only after the participants had made their decisions both as a FM and as a SM. We are aware of potential problems with using the strategy method but we believe they are likely irrelevant in our context²¹. Participants were informed about the actual allocation of roles and the outcome of their decisions in this situation only by the end of the session.

The FM was either endowed by £10 or had earned £10 and had to decide between keeping the money for him/herself or transfer them to the SM. If the FM kept the £10, the SM earned nothing. If the FM had transferred the £10 to the SM, then experimenters tripled this money before it was passed to the SM. Then, as a SM participants had to decide whether to keep the £30 and do not transfer anything back to the FM or transfer £15 back to the FM paired with. The game tree below summarises the game.

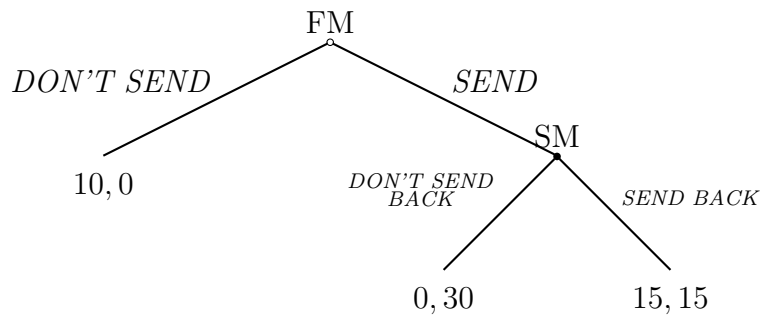


Figure 2.2: Trust Game

Participants were explicitly informed whether their partner -when acting as a FM- had been endowed the £10 or had gained them in the real effort task.

¹⁸The use of a trust game in order to facilitate my analysis was a very useful suggestion by my supervisor Prof. Michele Belot.

¹⁹As we discuss later on in this chapter the literature also refers to the second mover's decision as a choice between to show trustworthiness or not.

²⁰See Experimental Instructions in Appendix A.1. for the terminology used during the experiment.

²¹Strategy method is discussed further in a later section.

We kept homogeneity here as well, that is, we only considered pairs where both members had been endowed or had earned the £10 when deciding as FM.

We also tried to elicit beliefs of participants by incentivizing their predictions. They were asked to predict how many of the rest of the participants in the room they think they will transfer the £10 as a FM and how many they will transfer £15 back as a SM. Precision in predictions was rewarded with additional payments²².

2.2.5 The Set of Lotteries (Following Holt and Laury (2002))

The set of lotteries was designed in such a way to mimic the typical Holt and Laury (2002)’s menu of choices that permits measurement of the degree of risk aversion. Rather than choices between two lotteries with different variability of the potential payoffs we used a menu of nine choices between a lottery and a certain amount. The idea behind this was to map the decision of the FM in the trust game described above onto a risky decision, in order to facilitate direct comparisons between choices in the trust game and the lottery scheme. In that way, the set of lotteries works not only as a measurement of risk aversion but we also have the ability to consider whether decisions with HM and EM differ when framed in a trust environment rather than in a risky environment. Results from the related literature on strategic uncertainty vs. state uncertainty are presented in the following sections where we discuss our results and how these fit into the existing literature.

Case	Choose	
	A	or B
1	(15, 0.1); (0 ,0.9)	10
2	(15, 0.2); (0 ,0.8)	10
3	(15, 0.3); (0 ,0.7)	10
4	(15, 0.4); (0 ,0.6)	10
5	(15, 0.5); (0 ,0.5)	10
6	(15, 0.6); (0 ,0.4)	10
7	(15, 0.7); (0 ,0.3)	10
8	(15, 0.8); (0 ,0.2)	10
9	(15, 0.9); (0 ,0.1)	10

Table 2.1: Set of Lottery Choices

A table like Table 2.1 was given to the participants who were asked to make a decision for each of the cases. In each case they had to choose between the certain amount of £10 or an all-or-nothing lottery that would pay them either £0 or £15 according to the rule stated in the lottery and the result of the roll of a ten-sided die. Participants had to make a choice for all the nine cases but only one of them would be selected randomly by the computer to determine their earnings in this situation. Participants were informed about the outcome of their decisions in this situation only by the end of the session on a summary screen.

²²Strategy method is discussed further in a later section. This is typical practice for elicitation of beliefs in the related literature (see Fischbacher et al. (2001)) and see Experimental Instructions on how precision was rewarded.

2.2.6 The Public Good Game (Following Fischbacher et al. (2001))

In this situation, participants were divided into groups of four. We used random matching and we kept a strangers' protocol across the different situations that participants had to take part²³. The only information the participants received was the number of the group they belong to and their individual number in the group. This information was given to facilitate the understanding of the instructions and both numbers were selected randomly by the computer²⁴.

This situation consisted of two types of decisions: an unconditional contribution and a conditional one. The unconditional decision is similar to the one made in a typical linear public good game. Participants were asked to decide how much²⁵ of the £10 would be willing to keep in a private account and how much to transfer -if any- in a project²⁶. The earnings for each participant would be determined by the following equation²⁷ :

$$\pi_i = (e_i - c_i) + 0.4 \sum_i c_i$$

The second conditional decision²⁸ required from participants to fill in a 'transfer table'. This 'transfer table' looked like table 2.2 and participants had to indicate how much they would be willing to transfer to the project if the average transfer of the others in their group was the respective number.

Your Transfer Table	
Other group members' average transfer	Your transfer
0	
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	

Table 2.2: Transfer Table

²³See discussion earlier on this section.

²⁴The usefulness of the individual numbers will be shown in the next paragraphs.

²⁵They were only allowed to transfer increments of £1.

²⁶We did not use the term 'public account' in order to avoid any priming.

²⁷This equation was not presented to participants. Instructions included in the Appendix A.1 demonstrate how the potential earnings were explained to participants during the experimental session.

²⁸Though data from this second decision are not relevant to our analysis in this chapter we present exactly all the decisions participants faced during the whole experiment.

Following Fischbacher et al. (2001) we made the conditional decision incentive compatible by employing the following rule to determine the earnings in this situation. Participants were told that, after they have all made both types of decisions, a random mechanism would determine which of the two decisions would be relevant for the determination of their actual earnings in this situation. In each group, for one randomly chosen participant the ‘transfer table’ became this participant’s relevant decision. For the other three group members their unconditional ‘transfer’ was their relevant contribution decision. For each member of the group, the probability that the ‘transfer table’ will be the payoff-relevant decision was $1/4$ ²⁹.

We slightly deviate from Fischbacher et al. (2001)’s design by using terms like ‘transfer’ and ‘transfer table’ rather than unconditional and conditional contribution as to avoid any priming towards conditioning decisions since conditional cooperation and reciprocity are central concepts of our analysis -especially in the next chapter.

We also elicited beliefs about average transfers of others. Precision on beliefs was rewarded in this part of the experiment, too. Details on how precision was rewarded follows in the Appendix A.1 with the Experimental Instructions.

²⁹More discussion on this part of the experiment follows in the next chapter where the choice of the particular experimental design is more relevant to the research questions discussed there (see Chapter 3: The House Money Effect and Individual Differences).

2.3 Results

2.3.1 Participants

The experiment included a total of 64 subjects and was conducted in the Behavioural Laboratory at University of Edinburgh (BLUE)³⁰ in the spring of 2013. Five sessions were conducted in total: 3 HMEM (official trial 15/03/2013, session 1 and session 2 18/03/2013) and 2 EMHM (session 3 and session 4 25/03/2013). In each session we needed a number of participants multiple of 4 due to restrictions imposed by our experimental design as described in the previous section. Although we invited more than 16 participants³¹ in each session, there were still sessions where less than 16 participants showed up. Therefore, in some sessions we had 16 participants (official trial & session 1), in some 12 participants (sessions 2 & 4) and in one session 8 participants (session 8).

Participants were recruited using email lists of students of the University of Edinburgh, since the experiment was the first to be run in BLUE and the subjects' pool was not yet organised using ORSEE. We carefully excluded students that were students of the School of Economics to try to eliminate familiarity with game theory among participants. They were all students either on undergraduate (Honours and Non-Honours years) or postgraduate (Masters and PhD) degrees. According to the degrees' classification of the University of Edinburgh there were 18 participants from Humanities, 23 from Social Sciences, 4 from Engineering, 10 from Science, 4 from Medicine and 5 from Art degrees.

The average age of participants was 22 years and varied from 18 to 42 years old. There were 40 female participants and 18 male. Six of them did not report their gender on the Short Questionnaire they were asked to complete. In the same questionnaire they were asked to report their nationality and the country they have spent most years of their life living in. By combining the answers to these to questions we classified the participants as following: 28 were coming from the United Kingdom, 15 from the European Union, 13 from the United States of America and 8 from Asia.

The total cost of the experiment was £1017.4 and it was funded by the School of Economics of the University of Edinburgh. The average earnings were £15.90 and varied from £5 to £36.50. All the participants that arrived in the laboratory were paid a £3 show up fee and those who actually participated in the experiment were paid an additional participation fee of £2 according to the policy of the laboratory³². The payments to participants were only made at the end of the experiment.

³⁰This experiment was the first to be run in the BLUE and at that time the provisional name of the laboratory was CEREP (see Experimental Instructions in the Appendix A.1).

³¹The maximum number of vacancies we could use in the laboratory subject to our experimental design.

³²Sessions were overbooked to ensure enough participation.

2.3.2 Risk

We shall begin our analysis by considering first the individual decisions that participants had to make. In the two lottery situations -as we explain in an earlier subsection- participants face the decision choosing between a certain amount (£10) and an all-or-nothing lottery (£15 or £0). They face this decision for nine times (see 9 cases). The potential payoffs in the lotteries remain the same along the nine cases but we vary the probabilities of low and high payoffs in each lottery. As we go down in the table the probability of winning £15 when choosing the lottery increases by 0.1 and the probability of getting £0 decreases in the same pattern (see Table 2.1). Expected payoffs of the first six lotteries are lower than the certain amount of £10 suggesting that a risk-neutral person would choose the certain amount for 6 times before switching to the risky choice (the all-or-nothing lottery).

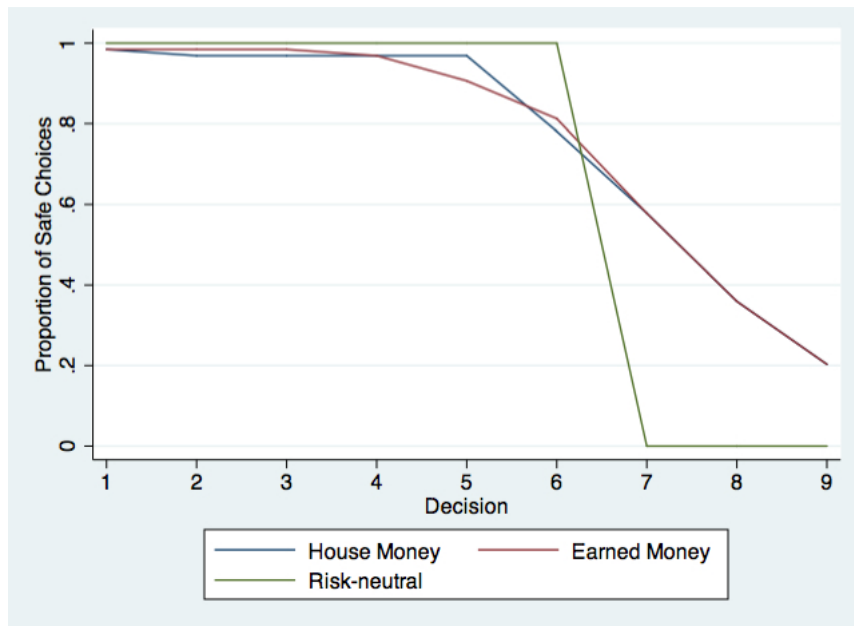


Figure 2.3: Proportion of Safe Choices in Each Decision

Our data as presented in Figure 2.3 suggest a tendency towards risk-averse behaviour among subjects participating. This result is commonly documented in the related literature³³. Figure 2.3 presents the proportion of safe choices made by participants in each of the nine decisions they were asked to make during the experiment. The green line indicates the predictions under the assumption of risk-neutrality, i.e., the probability that the safe option is chosen is 1 for the first six decisions and then the probability drops to 0 for all remaining decisions. The blue line shows the observed frequency of safe choices when participants using HM while the red line indicates the respective one when using EM. The graph itself is pretty indicative that there is no statistical difference between HM and EM. Actually, a sign test³⁴ on number of safe choices with HM vs. EM does not

³³See for instance Holt and Laury (2002)'s results.

³⁴We use here the sign test as it is applicable to the case of two related samples when the

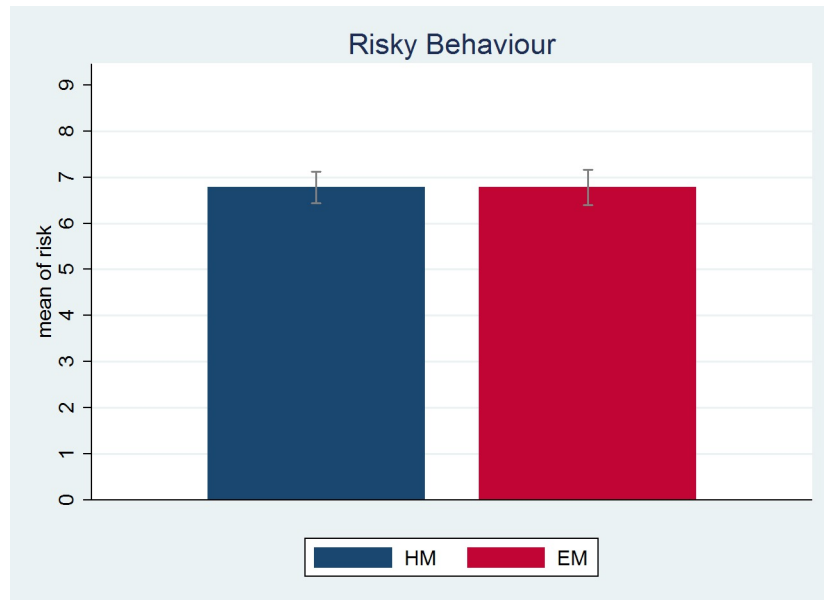


Figure 2.4: Risky Behaviour

allows to reject the null hypotheses of equality of medians as well as a Wilcoxon signed-rank test³⁵ according to which we could not reject the null hypothesis that both distributions are the same. The results of both tests suggest no change in behaviour across the different sources of money (two-sided sign test p -value= 1.0000 and Wilcoxon signed-rank test p -value= 0.8842 respectively). Participants seem not to behave differently when using HM and EM in this individual decision lottery setting. In Figure 2.4 below means of safe choices show that with both HM and EM participants switched to the risky choice later than risk neutrality predicts.

We also examine any potential differences in behaviour between subjects, considering only decisions in the first set of lotteries that each participant faces in each experimental session. That is, for the trial session and sessions 1 and 2 the

experimenter wishes to establish that two conditions are different. Following Siegel (1956) the null hypothesis H_0 tested by the sign test is that $P(X_A > X_B) = P(X_A < X_B) = 1/2$ where X_A and X_B are the two scores for a matched pair. Another way of stating the null is that the median difference is zero. When we apply the sign test, we focus on the direction of the differences between every X_{A_i} and X_{B_i} , noting whether the sign of the difference is plus or minus. Under the H_0 , we would expect the number of pairs which have $X_A > X_B$ to equal the number of pairs which have $X_A < X_B$. That is, if the null hypothesis were true we would expect about half of the differences to be negative and half to be positive. H_0 is rejected if too few differences of one sign occur. Since the test statistic is expected to follow a binomial distribution, the standard binomial test is used to calculate significance. The normal approximation to the binomial distribution can be used for large sample sizes.

³⁵Following Siegel (1956) again, the sign test that we discuss in earlier footnote utilises information simply about the direction of the differences within pairs. If we can also consider the relative magnitude we can use a more powerful test, the Wilcoxon signed-rank test which gives more weight to a pair which shows a larger difference between the two conditions than to a pair which shows small difference. The null hypothesis H_0 states that the difference between the pairs follows a symmetric distribution around zero while the alternative H_1 that the difference between the pairs does not follow a symmetric distribution around zero.

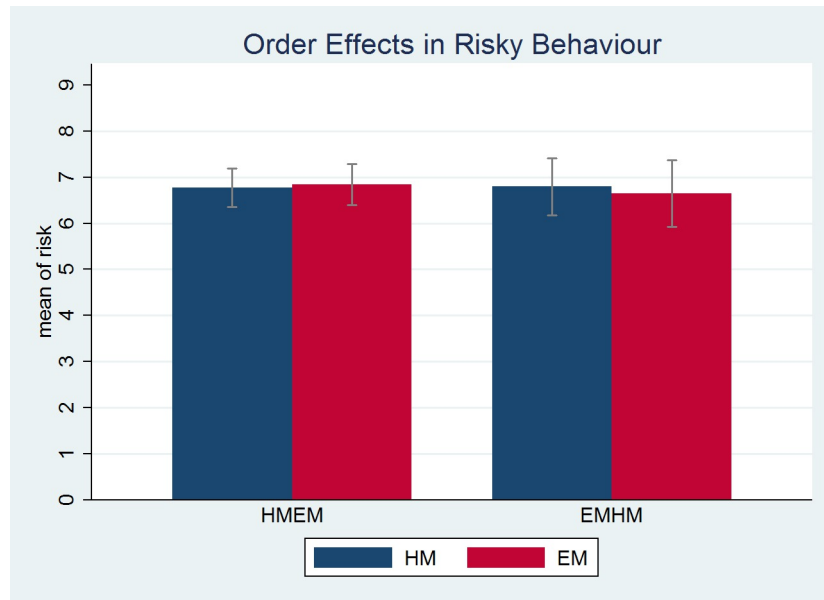


Figure 2.5: Order Effects in Risky Behaviour

choices in lotteries are made using HM and for sessions 3 and 4 using EM. A Mann-Whitney U test³⁶ shows again no statistical difference (p -value= 0.7902) between HM and EM in this setting.

Our findings are similar from hypothesis testing between subjects regarding the second set of lotteries participants face in each experimental session (Mann-Whitney U test p -value= 0.8130; see Figure 2.5). Therefore, our original hypothesis:

³⁶The Mann-Whitney U test (also called Wilcoxon rank-sum test) is not the same as the Wilcoxon signed-rank test, although both are nonparametric and involve summation of ranks. The Wilcoxon rank-sum test is applied to independent (unmatched) samples while the Wilcoxon signed-rank test is applied to matched or dependent samples. Following Siegel (1956), the Mann-Whitney U test is one of the most useful alternative to the parametric t test when the research -like in our case- wishes to avoid the t tests assumptions, or when the measurement in the research is weaker than interval scaling. Assuming two populations A and B , the null hypothesis H_0 is that A and B have the same distribution and the alternative H_1 is that A is stochastically larger than B . We may accept H_1 if the probability that a score from A is larger than a score from B is greater than one half. That is, if a is one observation of population A and b from population B then H_1 is that $p(a > b) > 1/2$. Of course, we might also consider H_1 $p(a > b) < 1/2$ or for the two-tailed test H_1 $p(a > b) \neq 1/2$. The test involves the calculation of a statistic, usually called U , whose distribution under the null hypothesis is known. In the case of small samples, the distribution is tabulated, but for sample sizes above ~ 20 approximation using the normal distribution is fairly good. The method for computing U is the following: We first assign numeric ranks to all the observations, beginning with 1 for the smallest value. Where there are groups of tied values, assign a rank equal to the midpoint of unadjusted rankings. We then add up the ranks for the observations which came from sample 1. The sum of ranks in sample 2 is now determinate, since the sum of all the ranks equals $N(N+1)/2$ where N is the total number of observations. U is then given by: $U_1 = R_1 - \frac{n_1(n_1+1)}{2}$ where n_1 is the sample size for sample 1, and R_1 is the sum of the ranks in sample 1. It doesn't matter which of the two samples is considered sample 1. An equally valid formula for U is $U_2 = R_2 - \frac{n_2(n_2+1)}{2}$. The smaller value of U_1 and U_2 is the one used when consulting significance tables.

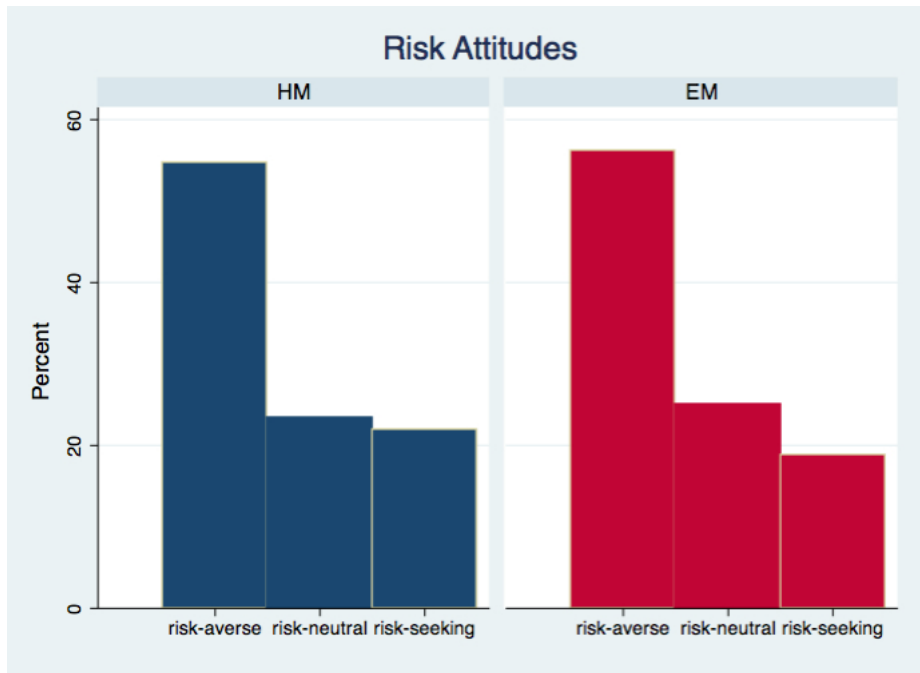


Figure 2.6: Risk Attitude Classification

Hypothesis 2.1. *More safe choices will be made when using EM rather than when using HM.*

is not supported by the experimental evidence presented above. This result is not far from what Cardenas et al. (2014) have documented, who although used a cash in advance method to induce the sense of playing with ‘own’ money, they still find only a small and indirect house money effect in decisions under uncertainty.

Finally in this subsection, we present a risk attitude types’ classification according to the number of safe decisions made by each participant. Classification seems to remain robust across different sources of money as seen in Figure 2.6.

2.3.3 Trust Game

The trust game as it has already been presented earlier is a sequential game with a unique subgame perfect Nash equilibrium³⁷. By backward induction the trustor prefers to keep the £10 as the trustee, if he/she takes the turn, will prefer to keep the £30. Results in related literature³⁸ suggest positive transfers by both the first mover and the second mover. Here, we will examine how both the two decisions in the trust game are affected (or not) by using EM rather than HM.

2.3.3.1 First Movers’ Behavior

We begin by conjecturing that if the house money effect is present in the decision to trust the following hypothesis should hold:

³⁷Similar to a sequential prisoner’s dilemma situation as the equilibrium is not an efficient outcome

³⁸See for instance seminal paper by Berg et al. (1995).

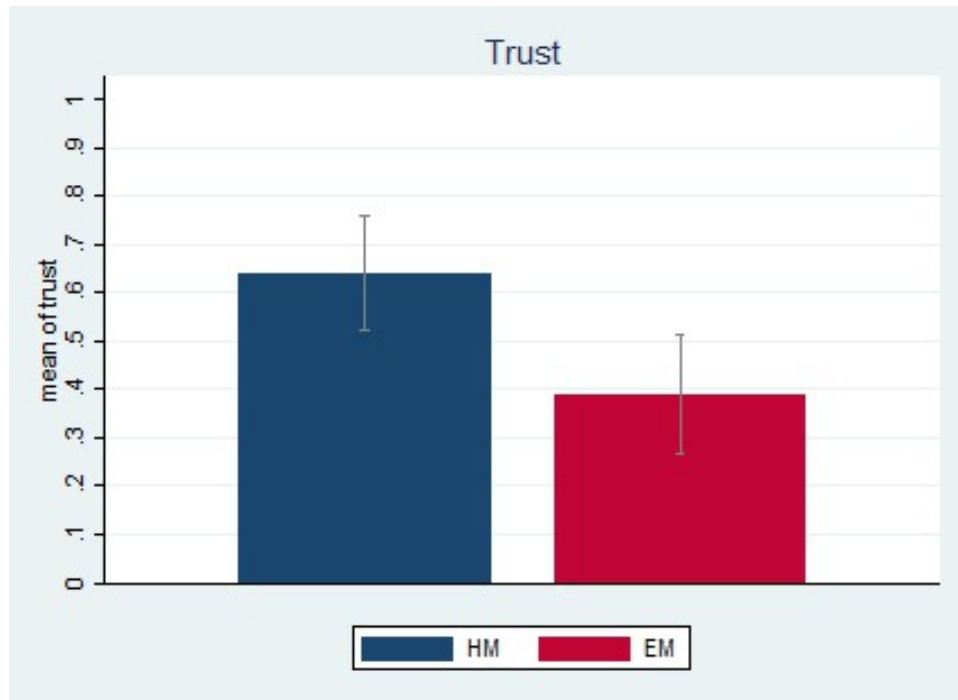


Figure 2.7: Trust

Hypothesis 2.2. *When participants using EM is less probable to trust than when using HM.*

This hypothesis is supported by our experimental data. Both parametric and non-parametric tests suggested significant differences between trust with HM and trust with EM (Wilcoxon signed rank test p -value= 0.0011). As the Figure 2.7 suggests approximately 64% of participants decided to trust when they were endowed with HM while only 39% did the same when earned endowments were at stake. The hypothesis is also supported by results in probit models that are discussed a bit later in this section.

We also consider differences in trust with HM and EM between subjects. We compare across sessions trust with HM and trust with EM when the trust decision is made for the 1st time in the experimental session and also when the trust decision is made for the 2nd time in the session. As Figure 2.8 shows, our results suggest that when decision for trust is made for the 1st time, although the difference between trust with HM and trust with EM is of the same direction like in within-subjects comparisons, it is not statistically significant (Mann Whitney U test p -value= 0.8031). When decision for trust is made for the 2nd time our experimental data show a statistically significant difference, suggesting that trust is more probable when with HM rather than with EM (Mann Whitney U test p -value= 0.0335). We discuss further these particular results when we comment on the advantages and disadvantages of the within-subject experimental design.

Our findings seem to be in the same vein with the related literature as this presented in Chapter 1, supporting the argument that assigning stronger property right entitlements by requiring participants to meet a performance quota in a real effort task in order to earn their endowment has an effect on cooperation in trust

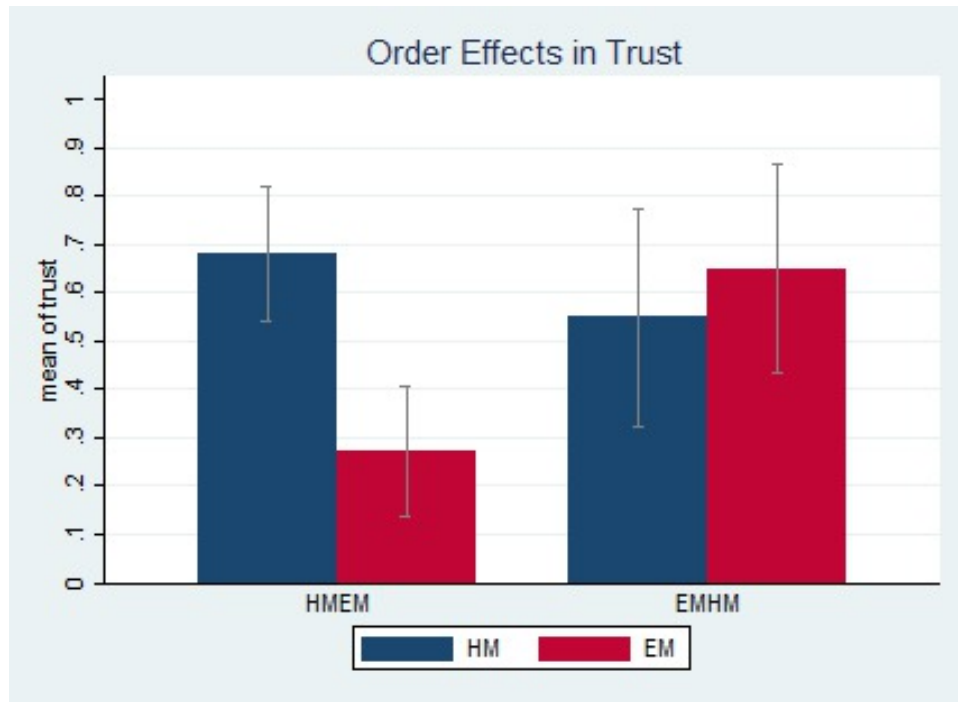


Figure 2.8: Order Effects in Trust

games.

2.3.3.2 Second Movers' Behavior

We treat experimental data on second mover's behaviour in a similar fashion. Given that the money on bargain is entitled to the first mover we expect the behaviour of the second mover in the trust game to be independent of the origin of the money. We test the following conjecture:

Hypothesis 2.3. *Participants' decision to reciprocate (show trustworthiness) is independent of the source of money.*

Our results from non-parametric tests confirm the above hypothesis (Wilcoxon signed rank test p -value= 1.000) while it is also confirmed comparing between subjects both when decision was made for 1st time and when for 2nd time (Mann Whitney U test p -value > 0.01 in both cases).

Our results are not exactly in line with earlier work, for instance by Fahr and Irlenbusch (2000) who observe that trustees return significantly more money the stronger the property rights of the trustors; but they do not contradict our argument for fairness and deservingness concerns, since they seem to respect property rights as the percentage of trustworthiness is always greater than the percentage of trust when earned money is used rather than when 'house' money is used where the opposite holds (compare Figures 2.8 and 2.10).



Figure 2.9: Trustworthiness

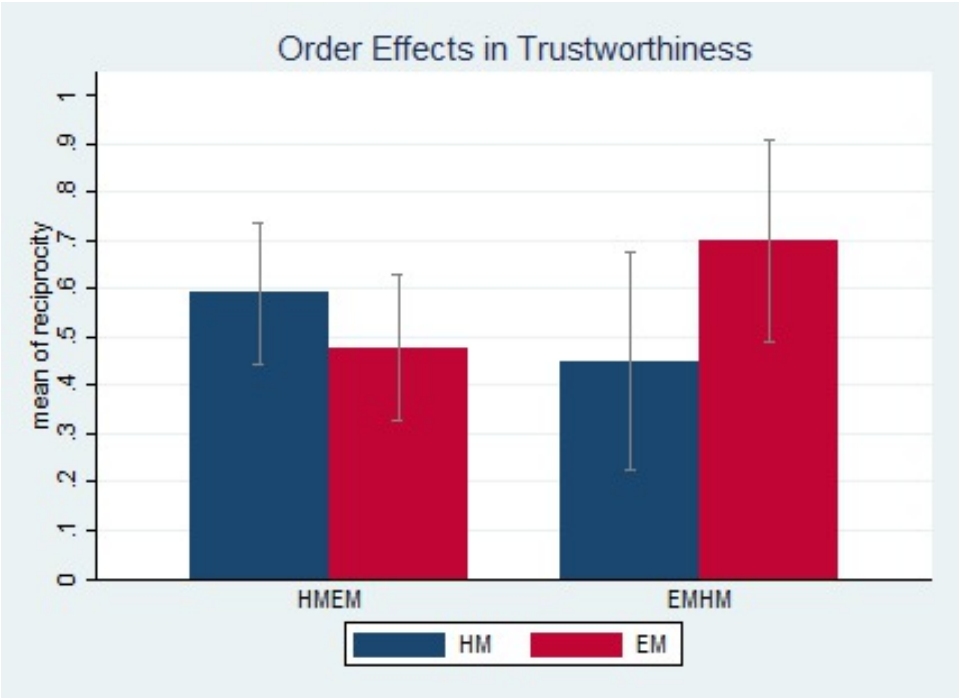


Figure 2.10: Order Effects in Trustworthiness

2.3.4 The Public Good Game

In our public good setting, given the existence of the dominant strategy, theoretical predictions suggest that participants should free ride and keep the £10 for themselves. Following the findings of the long existing literature on positive contributions in public good games³⁹, we expect at least some positive contribution and in regards with the house money effect's examination we conjecture that:

Hypothesis 2.4. *Contributions to the public account will be lower when using EM rather than HM.*

Our hypothesis is supported by our experimental data which show that participants contributed on average less with EM than with HM. Although the difference on average contributions (see also Figure 2.11) seems small both parametric and nonparametric tests suggest that it is statistically significant (Wilcoxon signed rank test p -value= 0.0375).

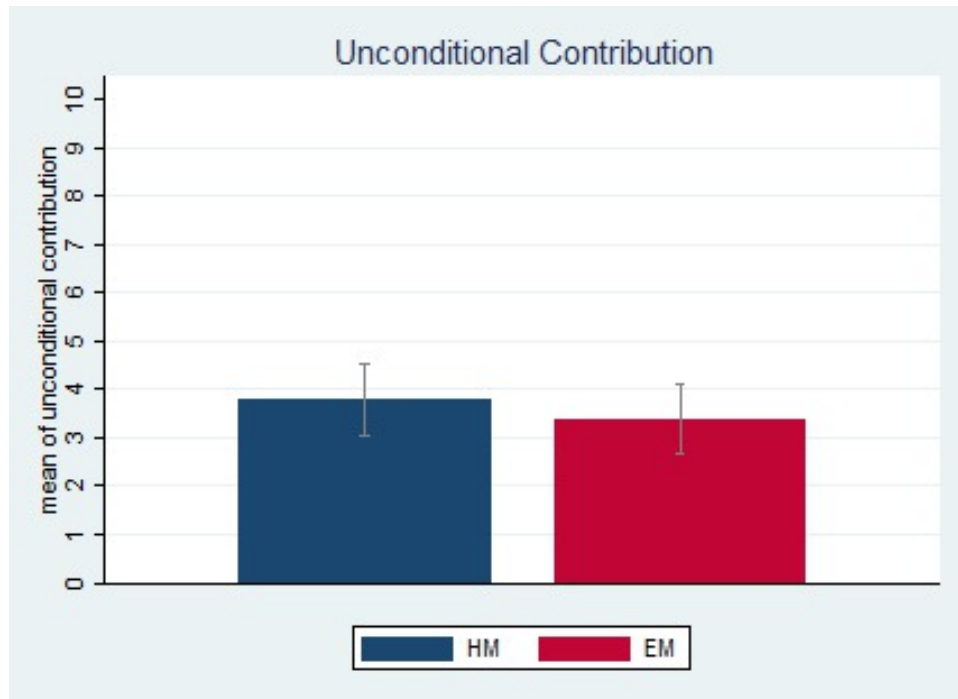


Figure 2.11: Unconditional Contribution

We also look at differences in contribution behaviour between subjects and our findings confirm Hypothesis 2.4 in that case, too. Particularly, we find that contributions to the public account were on average lower when using EM rather than HM when the public good game decision was made for the 1st time in the experiment⁴⁰.

³⁹See Ledyard (1995) for a review on cooperation in social dilemmas.

⁴⁰We discuss the findings from when the decision to contribute to the public account was made for a 2nd time in a later section when we discuss the order effects due to the within-subject design.

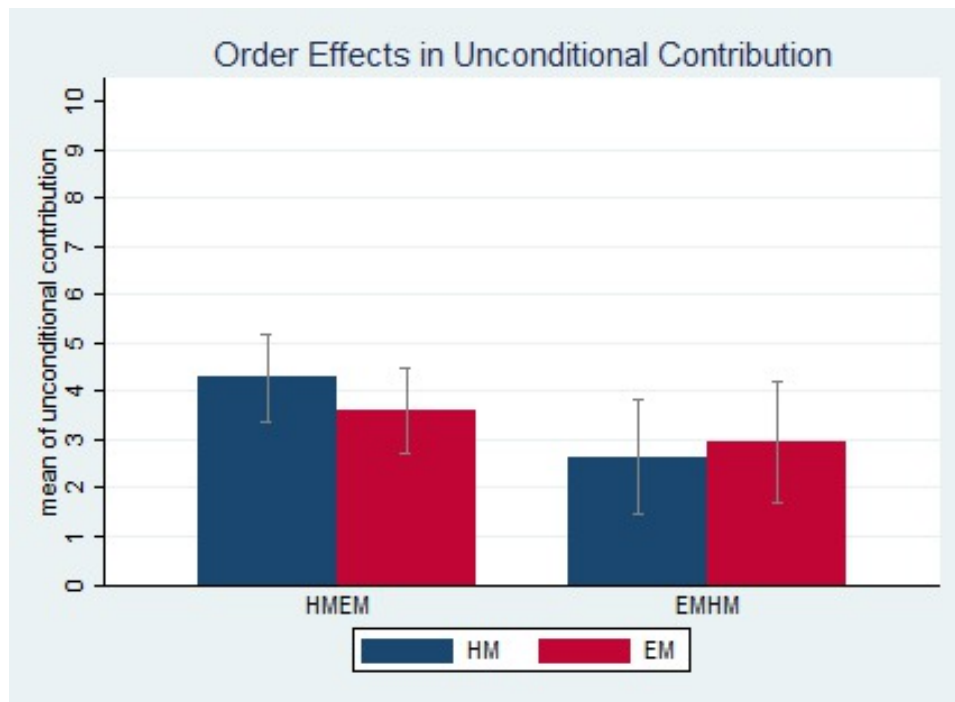


Figure 2.12: Order Effects in Unconditional Contribution

Our results are in accordance with those of Muehlbacher and Kirchler (2009) where participants who earn their endowments by exerting a high level of effort and without information about the asymmetry of the origin of the endowments within their groups seem to contribute less to the public good than those who receive their endowments more easily in the low effort condition; but as we discuss later on we do not think that our findings contradict previous work on the house money effect in public good games but they rather contribute to the discussion on the context dependency of the house money effect and argue for the necessity of considering context and its respective features when inference from experimental data is made⁴¹.

2.3.5 Context - Dependency of the House Money Effect

Our results above already make the case for the context-dependency argument for the house money effect but to further support our argument we make -as our experimental design allows us to- direct comparisons between the decision to trust and the decision to take a risky bet. As we discuss earlier on our experimental design's section the set of lotteries that participants are asked to decide upon is designed such that it maps the decision of the first mover in the trust game onto the decision on a risky bet. The two decisions seem equivalent from the point of view of theory as the participant in both cases faces a choice between keeping the £10 or risk them to potentially get £15 (or £0). The difference in the two decisions consists in the type of uncertainty that the participant faces in

⁴¹See discussion on “context matters and is not completely controlled by the experimenter” by Levitt and List (2007).

each case: in the lottery setting the participant faces state uncertainty while in the trust game the first mover faces strategic uncertainty. Our hypothesis tests the equivalence of the two decisions by comparing the switching probability in lotteries, that is, the probability that participants switch from the safe choice to the risky one in the set of lotteries, with the elicited beliefs on trustworthiness. More specifically, we ask participants how many among the other participants in the room they think they will transfer back £15 ; we translate this in a percentage and we interpret it as the perceived probability of receiving £15 back by the second mover. We form our hypothesis as following:

Hypothesis 2.5. *The behaviour in the trust game as a first mover with HM (or EM) is similar to the behaviour in the risky individual decision with HM (or EM).*

Our findings suggest that behaviour differs across the two contexts both with HM and EM (Wilcoxon signed rank test p -value < 0.01 , for both). Particularly, we find that the perceived probability of trustworthiness, namely of receiving £15 back by the second mover, that made participants to trust is significantly lower than the probability that made them switching to the risky bet in the set of lotteries. These findings are consistent with earlier work on the relation of trust and risk. Indicatively, we report results by Eckel and Wilson (2004) who similarly to us they use a Holt and Laury (2002) like risk measure which as their other two risk measures is not significantly correlated with trust. In the same vein, Houser et al. (2010) using a trust environment and a risk environment similar to ours⁴² and a Holt and Laury risk measure, conclude as well that investments distributions differ significantly between trust and risk environments and although risk attitudes predict investment decisions in risk games they do not in trust games.

Regarding this last result, we also consider some probit specifications to further explore the decision to trust. Our results indicate that the origin of money is always statistical significant across all specifications and earned money appears to reduce the probability of trusting but our risk measurement (‘number of safe choices made’), on the other hand, seems to do poorly in explaining the probability to trust across different specifications. The opposite holds for the trustworthiness (‘reciprocate’) and the contribution to the public account (‘PGG contribution’) which are always statistically significant positively related to the probability of trusting. Finally, regarding these probit specifications gender seems to be irrelevant for the decision to trust while order effects also appear to be insignificant. We do discuss though further on order effects in the next session.

⁴²They use the Berg et al. (1995) investment game and participants face as a second mover either another person or a computer, respectively.

Table 2.3: Risk, Trust & PGG Contribution

	(1) Risk	(2) Trust	(3) Contribution	(4) Trust	(5) Trust
Earned money	0.0172 (0.17)	-0.589** (-2.93)	-0.432* (-2.20)	-0.649** (-3.22)	-0.575** (-2.90)
Session EMHM	-0.339 (-0.87)	0.397 (1.24)	-0.997 (-1.24)		
Sex	-0.495 (-1.32)	0.464 (1.48)	1.356 (1.41)		
Risk		-0.0341 (-0.34)	0.0817 (0.31)	-0.0631 (-0.62)	
PGG contribution				0.156** (3.03)	
Belief Trustworthiness					1.755*** (3.34)
Constant	7.188*** (24.07)	0.0999 (0.12)	2.719 (1.24)	0.236 (0.31)	-0.580 (-1.75)
Observations	116	116	116	128	128
Pseudo R^2		0.073		0.135	0.116

t statistics in parentheses

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

2.3.6 Order Effects & the within Subject Design

Charness et al. (2012) argue that within-subjects designs are more naturally aligned with most theoretical mindsets, for instance, it is more likely for a theorist to imagine an agent in a market reacting to a price change rather than two agents in separate markets with different prices. Likewise for us, our interest focuses on how a person reacts into holding and spending earned money rather than house money. Apart from boosting the internal validity, since this does not depend anymore on random assignment, the within-subject design was also chosen on the grounds of economizing on the number of participants we employed and of course, moderating the total cost of the experiment.

Given that in within analyses participants are exposed to multiple treatments, one has to worry about the order of exposure affecting the reference and framing of treatments. For that we consider whether order matters. As we see earlier in the probit specifications for the decision to trust the dummy for order effects appears statistically insignificant⁴³. We do take a closer look though by considering decisions with HM and EM in each kind of session.

Observing Figures 2.8 2.10 and 2.12 we can easily spot the decline in percentage of trust or trustworthiness and in the level of contributions in the public good game suggesting learning across the two plays of the games⁴⁴ but as we earlier discuss our results within subjects are robust between subjects as well both when decisions were faced for 1st and 2nd time; with only exceptions when trust decision is made for the first time where difference although of the same sign is not statistically significant and when decision for contributions in the public account was made for a 2nd time where we observe that contributions with HM are lower (but not statistically significantly lower) than contributions with EM. We attribute the first inconsistency to the fact that the 1st decision to trust was the very first decision to be made in all our sessions while regarding the second inconsistency we claim that given that in sessions where decisions with EM were made first and those with HM followed (EMHM sessions) contributions in the public account with EM were very low and coupled with as we described it above, contributions with HM were even lower. However this ‘learning’ bias we observe is independent of the order of decision our participants were exposed to.

Finally, a last argument in favour of our within-subject design is the following: in a between-subject design there might exist a potential difficulty of participants to perceive any difference between ‘house’ money and earned money given the way entitlements over earned money are induced, given the small size of stakes, and given that the money is credited on account rather than given in hand⁴⁵ (payments were only made at the end of the experiment) while in the within subject design they observe both scenarios at once and can make a decision not in vacuum⁴⁶.

⁴³Results were similar when we considered order effects dummy in OLS regressions for contributions in public good games and for risk variable.

⁴⁴Similarly we observe a consistent increase in the mean of safe choices in the risky decision that participants face (see Figure 2.5).

⁴⁵Reinstein and Riener (2009) find a strong tangibility effect on total donations in a charitable giving experiment.

⁴⁶See Charness et al. (2012).

2.3.7 Limitations of the Experimental Design

At the last part of this section where we discuss our results, we would also like to acknowledge the limitations of our experimental design. These limitations have been mentioned throughout this chapter but we choose to also list them here in attempt to motivate further research on the related topics.

We shall start by discussing the real effort task to induce ‘own’ money or -as we refer to it throughout the thesis- earned money. The puzzles were relatively easy and the quota of 10 correct answers out of 25 quizzes was quite low as we tried to avoid heterogeneity of endowments in pairs and groups for the trust game and the public good game respectively. Considering our results, these aspects of our experimental design have not deterred us from observing evidence of entitlements’ emergence but we believe that higher stakes and/or great ‘difficulty’ on the process of earning the endowments might have resulted in an amplified house money effect observed in the trust game and the public good game or even in the case of the decision over lotteries. Clark (2002) uses an interesting approach: instead of using a real effort task he asks participants to bring their own \$8 and he ensured an identical final wealth distribution between treatments of house money and own money by adding an unannounced \$8 participation fee to subjects accumulated earnings in the treatment with own money before they received payment at the end of their sessions. Another example is that of the work of Cardenas et al. (2014) who gave the participants cash in advance but they find only a small and indirect house money effect in decisions under uncertainty.

We consider talking explicitly here about the within-subject element of our experimental design redundant as we believe that the subsection above covers all the relevant issues but -following comments of one of the external examiners- we believe that we could have consider randomising the order of decision and most importantly, the order of belief and decision tasks.⁴⁷ Another interesting -and useful for future research- comment⁴⁸ of a colleague from the University of Edinburgh was the addition of a simpler game that involves only social preferences like the dictator game in the experimental design. Along the same lines, future research should take under consideration some scepticism regarding the Holt and Laury (2002)’s measure of risk aversion.⁴⁹

⁴⁷The importance of the latter becomes more apparent mainly in our discussion about anticipatory reciprocity at the next chapter.

⁴⁸See also footnote 4 of this chapter for further discussion on this.

⁴⁹See for instance recent work by Bosch-Domenech and Silvestre (2013) on measuring risk aversion with lists

2.4 Conclusion

This chapter picks up where the first chapter concludes: there is a general consensus in the related literature regarding the context-dependency of the house money effect, but no agreement regarding the question of what drives the latter. In this chapter, we tackle both of these research questions. We used a within-subject experimental design which allows us to examine the house money effect and to compare findings across three different contexts: an individual decision involving risk (a set of lotteries); a group decision involving social preferences (a public good game); and lastly a group decision involving both risk and social preferences (a trust game). These key aspects of our experimental design help us, first, to pin down the contextual features which may amplify (or nullify) the house money effect and secondly, to test directly and disentangle the two factors that may drive it, namely increased risk seeking in the presence of a prior gain or ‘just desert’ and fairness considerations.

Our experimental findings confirm the arguments that contextual differences matter and may affect the emergence of the house money effect and more specifically, we find the house money effect present both in the decision to trust (but not in the decision to show trustworthiness) in the trust game and in the decision to contribute in the public good game, but (crucially) not in the risky individual decision. Risk attitudes and risky behaviour seem to be robust across the different origins of money and also our Holt and Laury (2002)-like risk measure seems to perform poorly in terms of explaining the trust decision in the trust game (and the decision to contribute in the public good game). Therefore, our results appear to favour interpretations of the house money effect as a result of social preferences of fairness and deservingness rather than a consequence of increased risk-seeking due to different mental accounting over windfall gains.

We believe that our work adds to the oft-discussed debate in economics over whether ‘windfall money’ is treated differently than earned (regular) income. Perhaps most importantly, our work also contributes to the discussion on the way forward for the experimental methodology in terms of both the design of experiments and the interpretation and generalisation of experimental findings. The context-dependency issue opens the path for further research on contexts not considered here. In particular, given that we have argued in favour of interpretations of the house money effect as a result of fairness and desert preferences, it would be worthwhile to assess the house money effect within the context of individual differences in social preferences. Our third chapter deals specifically with such considerations.

Chapter 3

The House Money Effect and Individual Differences in Social Preferences

3.1 Introduction

In the previous two chapters we argued in favour of interpretations of the house money effect as a result of fairness and ‘desert’ considerations. Given these conclusions, we consider it worthwhile to assess the house money effect within the context of individual differences in social preferences. Both the house money effect and individual differences in social preferences have mainly been used to explain cooperation in social dilemmas. This chapter combines these two branches of the experimental literature by testing the implications of house money on cooperation in social dilemmas. To this end, we employ a linear public good game and more specifically, the innovative experimental design of Fischbacher et al. (2001), which allows us to observe contributions both with the direct response method (‘unconditional contributions’) and the strategy method (‘conditional contributions’). The former, along with elicited beliefs, helps us discuss ‘anticipatory reciprocity’ while the latter allows us to classify participants according to their behaviour in the public good game. By adding the within-subject element in our experimental design we can test the robustness of this classification across the different origins of endowments. To our knowledge, this is the first study to examine the implications of the house money effect in reciprocal behaviour (or conditional cooperation, as it is named throughout this chapter).

Our results indicate that the house money effect is present in the public good game, but the types’ classification is robust across the origin of money. Contrary to Harrison (2007)’ results, we find that participants’ decision to free ride or not (contribute or not) is independent of the origin of money. However, given that the decision to contribute has been made, contribution levels may vary -actually be lower- when money is earned rather than windfall-endowed. Beliefs also appear to be affected by the origin of the money, and when compared with unconditional contributions this provides evidence of anticipatory reciprocity. Our last findings relate to the discussion of the behavioural validity of the strategy method, and

suggest that although it seems to do well in identifying conditional co-operators, positive conditional contributions may appear deflated when compared to positive unconditional ones (both with house money and earned money).

The rest of the chapter continues as follows. We first present the related literature with a main focus on individual differences in social preferences. We then present the linear public good game with its prediction of the reciprocity hypothesis as well as our experimental design. Next, we present and discuss our results while the last section concludes.

3.2 Related Literature

Over the last decades experimental research has provided ample evidence that the classical paradigm of “homo economicus”- the rational and selfish economic man - might not be able to explain behaviour in the real world. People seem to make donations in dictator games (Kahneman et al. (1986); Forsythe et al. (1994); Camerer (2003)), reject even high shares in the ultimatum game (Guth et al. (1982); Camerer (2003)) and last but not least cooperate in situations of social dilemmas (Ledyard (1995)). Evidence suggests that people may not only care about their own material payoffs, but they might also consider other things like others’ payoffs. A large literature, both theoretical and experimental, has emerged on other-regarding preferences suggesting that people’s decisions might be affected by altruism, fairness, reciprocity and, efficiency motives which shape their social preferences.

Most of these theories still assume rationality but they relax the assumption of selfishness by assuming that people have other-regarding or social preferences. Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) assume that people care about ‘fairness’ and they define this ‘fairness’ concern as inequity-aversion, that is, the dislike for unequal payoff distributions. Besides these outcome-based models of other-regarding behaviour, other approaches that focus on intentions of economic agents have been developed. The most influential paper in this regard is by Rabin (1993), while Dufwenberg and Kirchsteiger (2004) and Falk and Fischbacher (2006) extend Rabin’s work to extensive form games. Charness and Rabin (2002), motivated by the confoundedness in explanations that models of other-regarding preferences had suggested, present a model that embeds difference aversion, social-welfare preferences and other preferences (such as reciprocity). A common property of these models of social preferences is that economic agents are heterogeneous in their preferences. This implies that theoretical predictions about individual behaviour may differ between agents even if they face the same decision problem. This implication has triggered a whole new and closely related branch of literature that discusses heterogeneity (individual differences) in preferences among economic agents.

Individual differences in other-regarding preferences have been suggested as one of the main explanations of the observed behaviour in social dilemmas situations. Andreoni (1995) in his novel paper makes the first systematic attempt to separate the hypothesis that cooperation is due to kindness, altruism or warm-glow from the hypothesis that cooperation is simply the result of errors or confu-

sion. He concludes that apart from the need for the experimental methodology to improve in order to eliminate confusion in economics experiments, experimental and behavioural research should also shift to include studies of preferences for cooperation.

Theories of altruism, commitment, fairness, competition, social efficiency and reciprocity have been suggested to explain and describe behaviour in public and social dilemmas situations. Croson (2007) presents a direct comparison of competing theories of social preferences by using evidence from linear public good games. Her work has provided strong support for reciprocity theories over either theories of commitment or altruism.

Reciprocity theories find their origins in Sugden (1984)'s paper where he argues in favour of reciprocity as a means to explain other-regarding behaviour due to the wide range of testable predictions it entails. Charness and Rabin (2002) also argue in favour of developing models that incorporate reciprocity as it is a prevalent phenomenon that has been ignored.

Fischbacher et al. (2001)) investigate whether people are conditionally cooperative in a public goods game environment. Participants in their experiment play a one-shot public good game with a variant of the strategy method that directly elicits participants' willingness for conditional cooperation. They classify their participants into three groups: 50% of them are conditional cooperators, 30% free-riders and 14% percent made 'hump-shaped' contributions.

At approximately the same time, Houser and Kurzban (2001) suggest another experimental design, what they call a circular public good game, to capture the heterogeneity of behaviour in public good games and classify participants accordingly. They classify participants into three groups: strong free-riders (28%), conditional cooperators or reciprocators (29%) and strong cooperators (25%). In a later paper, Kurzban and Houser (2005) run a similar experiment and find that their subjects fall into three types (cooperators, reciprocators and free-riders) as well, that an individuals type is stable and that a group's cooperative outcomes can be remarkably well predicted if one knows its type composition.

According to Fischbacher et al. (2001) conditional cooperation can be considered as a motivation on its own or it can be a consequence of altruism, warm-glow, inequity aversion or reciprocity¹.

Keser and Winden (2000), whose work is among the first to examine conditional cooperation, distinguish between two aspects of conditional cooperation: future-oriented behaviour and simple reactive behaviour². Regarding these second aspect of reactive behaviour, their findings confirm what Keser (1997) had earlier found: behaviour in public good games is oriented towards the average behaviour of the other group members in the previous period. This behaviour is also in the same vein as the principle of reciprocity which according to Axelrod (1984) is observed in prisoner's dilemma situations, where people appear to reply

¹Although Fischbacher et al. (2001) discuss 'conditional cooperation' as a more general concept that might be driven by different kinds of social preferences, we do argue in the next section that is mostly and mainly refer to reciprocal behaviour. (see Section 3.3)

²The first aspect of conditional cooperation is irrelevant to our analysis since our experimental design abstracts away from both the dynamic element of repeated games and the 'partners' matching protocol.

to cooperation with cooperation and to defection with defection.

We have so far presented a summary of the literature on other-regarding preferences and individual differences. Given that the first chapter of this thesis is an extensive literature review on the house money effect, it would be repetitive to provide a survey on the house money effect here too. It is however very important to clarify the obvious question arising by our introduction and our claim that this chapter combines these two branches of the experimental research: how do individual differences come into the discussion of the house money effect that we have examined so far in this thesis?

Looking back to the literature presented in Chapter 1, already in Harrison (2007) work we observe a rough classification between free riders and those making positive contributions. Cherry et al. (2005) in their work on the impact of endowment origin and heterogeneity on public good contributions introduce the concept of an ‘anticipatory reciprocity’ effect in order to interpret the patterns of contributions with high and low endowments in heterogeneous groups. Anticipatory reciprocity implies -as observed in their results- that participants contribute more when others are also able to contribute more. Cherry et al. (2005) conclude that rather than being generous with their earned wealth, people seem to anticipate a reciprocation of their contributions. They relate this to conditional cooperation introduced by Fischbacher et al. (2001) and underline the difference between the two experimental designs, since in their design as opposed to that of Fischbacher et al. (2001) - participants with high endowment could do very little since those with low endowment are limited in terms of their ability to reciprocate.

In their later work Kroll et al. (2007) argue that the anticipatory reciprocity effect might overshadow the house money effect, and they examine these phenomena in a new context of a best-shot public good game, where behavioural motive for anticipatory reciprocity should be eliminated as only the highest contribution sets the level of public good provision and all other lower contributions are unnecessary. Their findings suggest that participants seem to be concerned about efficiency and fairness. With windfall endowments efficient outcomes appear to be the salient distributive preference while with earned endowments efficiency might not be considered as a fair outcome. Kroll et al. (2007) relate their results with the developing at that time literature on social preferences as summarised earlier on in this section and they conclude that future research should try to understand how contextual cues can affect social preferences.

Lastly, Oxoby and Spraggon (2009) find evidence which supports the concept of anticipatory reciprocity as discussed by both Cherry et al. (2005) and Kroll et al. (2007). Using an experimental design of a two-person public good game with strategy method³, they observe an ‘inverse found money effect’ according to which participants who earned their endowments and were matched with someone who did not were more unconditionally and conditionally cooperative. They attribute this ‘inverse found money effect’ to anticipatory reciprocity. Partici-

³Their design follows that of Fischbacher et al. (2001) and Keser and Winden (2000) and of course is similar to our experimental design. Differences between the experimental designs are discussed in the following section regarding our experimental design.

pants expect those with windfall endowment to contribute (more) and they do so themselves.

To our knowledge, there are only few studies that actually examine the house money effect in combination with other-regarding preferences. Danneberg et al. (2012) analyse if individual inequality aversion as measured by simple experimental games depends on the origin of monetary endowments in those experiments, that is, ‘house money’ versus earned money. Their results suggest that individual inequality aversion as measured by the model of Fehr and Schmidt (1999) is not generally robust to the way endowments emerge and that inequality aversion has only low predictive power for individual behaviour as it seems to perform best when the endowment is windfall and small.

Danková and Servátka (2013) present a study on the house money effect and negative reciprocity. Similar to us they explore the implications of windfall endowments on observed reciprocal behaviour but, in contrast to this paper they focus on negative reciprocity. For that reason they employ a two-player Taking Game in which the First Mover (FM) decides whether or not to take an amount of money from the Second Mover’s (SM) endowment, who can retaliate in return. They implement three treatments where in the first treatment SM’s endowment is entirely ‘house money’; in the second and third it consists of both ‘house’ and earned money. In one case the FM can take the ‘house money’ part of the endowment and the SM can retaliate using his/her earned money part. In the other case, the FM takes the earned part and the SM can retaliate using ‘house money’. They conjecture two reasons why the origin of endowment might matter for negative reciprocity: first, using earned money as opposed to house money might increase the costs of negative reciprocity due to this money being in a different mental account and thus lead to less retaliation and second, decreasing an endowment consisting of earned money might be considered a stronger violation of property rights and lead to more retaliation. Their results suggest that participants retaliate more in both cases⁴.

⁴Closely to this literature there is an extensive literature on retaliation and punishment mainly suggesting that an increase in the perceived cost of reciprocation/retaliation may diminish its frequency and/or extend. Although relevant to our discussion we want to avoid to tire the reader interest and focus and for that reason we just cite here some of the most important paper of that literature: Ostrom et al. (1992), Fehr and Gächter (2000), Anderson and Putterman (2006), Carpenter (2007), Nikiforakis and Normann (2008), Nikiforakis (2008), Herrmann et al. (2008), Nikiforakis and Engelmann (2011).

3.3 The Linear Public Good Game

In a typical linear public good game⁵, the individual decision to be made is to allocate an initial endowment between a private and a public account. Each player's payoffs are given by the following expression:

$$\pi_i = (e_i - c_i) + m \sum_{i=1}^n c_i, \quad \frac{1}{n} < m < 1$$

where e_i is the initial endowment, c_i the contribution to the public account, m the public account multiplier and n the number of members of the group that might or might not contribute to the public account. When $m < 1$, contributing to the public account is never optimal for the self-interested individual. Contributing one unit to the public account earns him/her only m , and costs him/her 1. When $1/n < m$, contributing to the public account is always socially optimal for the group as a whole as contributing one unit to the public account costs an individual 1, but earns $n \times m$ for the group.

The traditional hypothesis of pure self-interest assumes a utility function in which individuals are concerned only about their own payoffs. Following the notation above we have:

$$U_i = (e_i - c_i) + m \sum_i c_i$$

When individuals care only about their own material payoffs, a pure public goods problem like the one our participants face generates a unique equilibrium in which each of n symmetric individuals, acting individually rational and choosing the dominant strategy, will contribute zero, that is, $c_i^* = 0$ independently of what others will do. This last bit implies that there will not be any correlation between what an individual contributes and what others in the group contribute; thus $\delta c_i^* / \delta c_j = 0$.

Assuming reciprocity⁶ (individuals reciprocate or match contributions of others) and using notation from above, the problem for each individual appears as follows:

$$U_i = (e_i - c_i) + m \sum_i c_i$$

s.t. $c_i \geq \min(c_i^c, c_j)$.

where c_i^c is the optimal level of contribution under commitment hypothesis which implies that an individual contributes to the public account the level of contribution he/she would most prefer that every member of the group make. Under reciprocity hypothesis we expect positive contributions $c_i^* > 0$ and also in equilibrium there can be a positive correlation between one's own contribution and others contribution $\delta c_i^* / \delta c_j > 0$.

⁵The literature also refers to that as Voluntary Contribution Mechanism

⁶Following Croson (2007).

3.4 Experimental Design

This experiment is part of a larger study on the house money effect. We present here the part of the experiment that is relevant to our research questions in this chapter⁷.

Participants were asked to make decisions on a linear public good game similar to that of Fischbacher et al. (2001). They were divided into groups of four. We used random matching and we kept a strangers' protocol across the experiment. Participants were informed about their performance and their payoff only at the end of the experimental session. The only information the participants received was the number of the group they belong to and their individual number in the group. This information was given to facilitate the understanding of the instructions and both numbers were selected randomly by the computer⁸.

This situation consisted of two types of decisions: an unconditional contribution and a conditional one. The unconditional decision is similar to the one made in a typical linear public good game (as the one described in the previous section). Participants were asked to decide how much⁹ of the £10 would they be willing to keep in a private account and how much to transfer -if any- in a project. The earnings for each participant would be determined by the following equation¹⁰ :

$$\pi_i = (e_i - c_i) + 0.4 \sum_{i=1}^n c_i$$

The second conditional decision¹¹ required participants to fill in a 'transfer table'. This 'transfer table' looked like Table 3.1 and participants had to indicate how much they would be willing to transfer to the project if the average contribution of the others in their group was the respective number.

Following Fischbacher et al. (2001) we made the conditional decision incentive compatible by employing the following rule to determine the earnings in this situation. Participants were told that, after they had all made both types of decisions, a random mechanism would determine which of the two decisions would be relevant for the determination of their actual earnings in this situation. In each group, for one randomly chosen participant the 'transfer table' became this participant's relevant decision. For the other three group members their unconditional 'transfer' was their relevant contribution decision. For each member of the group, the probability that the 'transfer table' would have been the payoff-relevant decision was 1/4.

⁷For the full experiment the reader can refer back to Chapter 2 where the experimental design is discussed.

⁸The usefulness of the individual numbers will be shown in the next paragraphs.

⁹They were only allowed to transfer increments of £1.

¹⁰This equation was not presented to participants. Instructions included in the Appendix A.1 demonstrate how the potential earnings were explained to participants during the experimental session.

¹¹Although the data from this second decision are not relevant to our analysis in this chapter we present the whole experiment for the reader to have a clear images of all the decisions participants went through.

Your Transfer Table	
Other group members' average transfer	Your transfer
0	
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	

Table 3.1: Transfer Table

We deviate from Fischbacher et al. (2001)'s design by using terms like 'transfer' and 'transfer table' rather than unconditional and conditional contribution as in order to avoid any priming towards conditioning decisions since reciprocity is a central concept of our analysis.

This was a within-subject experiment. Participants had to decide twice on their unconditional and conditional transfers once using £10 that was credited to their account by the experimental team (HM) or £10 that they had earned by performing the real effort task (EM)¹².

We also elicited beliefs on average contributions of others. Precision on beliefs was rewarded in an attempt to elicit true beliefs. Details on how precision was rewarded is available in the Appendix A.1 with the experimental instructions.

Acknowledging the complexity of the game, the reading of the instructions was accompanied by a power point presentation¹³ in order help them to understand the rules of the tasks. Some additional instructions were added in each of the screens along the experiment and participants were welcomed to ask questions to clarify any confusion regarding the instructions.

Given that our main focus is the house money effect and how this affects conditional cooperation in public goods games we consider Fischbacher et al. (2001)'s simplest and easiest experimental design that allows us to directly test for correlation or no correlation between one's contribution and others' in the group average contribution, therefore testing directly for evidence of conditional cooperation and, most importantly, facilitating the classification of participants according to their conditional contribution patterns.

¹²See Chapter 2 for more details on the real effort task.

¹³The power point presentation is also available in the Appendix A.2.

3.5 Results

In this section we present and discuss our experimental results. We skip the discussion on participants as this experiment is part of a larger experiment discussed in Chapter 2. The reader can refer to Section 2.3.1 for more information regarding the participants.

3.5.1 The HME in the Public Good Game

We start our analysis here first by discussing our preliminary findings in the public good game. In the linear public good game setting presented in Section 3.3, theoretical predictions, given the existence of the dominant strategy, imply that the participants should keep the £10 and contribute nothing to the public account, that is, they should free ride. The existing experimental literature though suggests that at least some positive contributions should be expected and in regards with the house money effect's implications we expect differences in contributions to the public account made by different kind of money (house money vs. earned money). In particular we conjecture that:

Hypothesis 3.1. *(Unconditional) contributions to the public account will be lower when using EM rather than HM.*

The above hypothesis is confirmed by the data as both parametric and non-parametric tests show significant difference between mean and median values of contributions in house money vs. earned money (Two-sample t test p -value= 0.0451; Wilcoxon signed rank test p -value= 0.0375). The house money effect seems to be present in the public good game setting¹⁴, that is, the origin of money seems to matter and this is also indicated in Figure 3.1.

Following the empirical analysis of Harrison (2007) on the experimental data of Clark (2002), we particularly focus on the positive contributions in the public account. We construct a binary variable that takes a value equal to one when the unconditional contribution is positive and zero in any other case, that is, when the unconditional contribution is zero. Our conjecture, if the house money effect is present and affects the decision to contribute or not, is the following:

Hypothesis 3.2. *Positive contributions will be less probable when using EM rather than HM.*

We can also read this as 'free-riding behaviour will be more common with EM rather than with HM at stake'. Our results¹⁵ - as also presented in Figure 3.2 - indicate that the decision to contribute or not (free-ride or not) is independent from the origin of money (Two-sided sign test p -value= 1.0000; Wilcoxon signed-rank test p -value= 0.5637) as we observe no significant difference on the percentage of positive contributions across the two types of money in stake. From this latter result in conjunction with our very first result presented here in this

¹⁴This preliminary result has also be presented in Chapter 2 as Hypothesis 2.4.

¹⁵A probit for positive contribution suggested a non-significant (negative) coefficient for earned money.

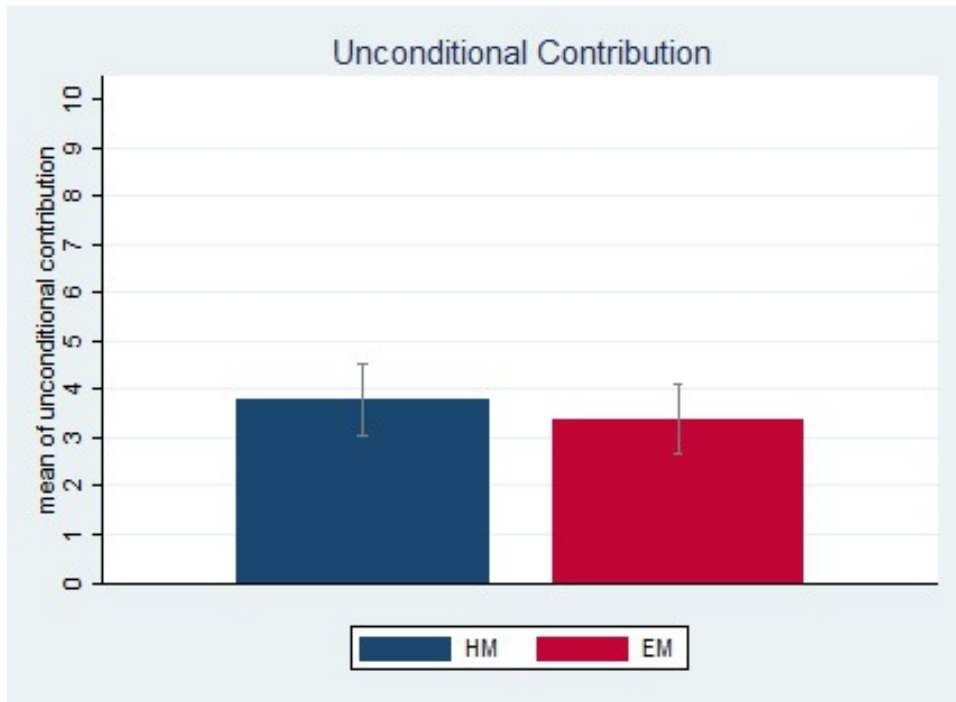


Figure 3.1: Unconditional Contribution in PGG

section, which suggests that house money is related with more -on average- generous contributions compared to earned money, we infer that these differences are driven by differences in levels of positive contributions rather than by having more people free-riding when using earned money.

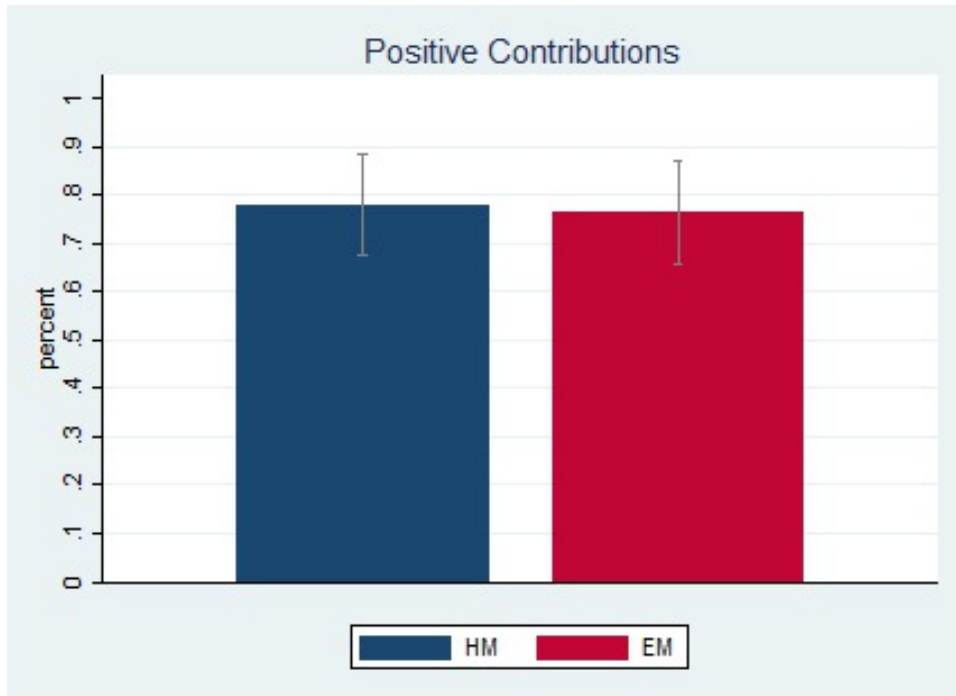


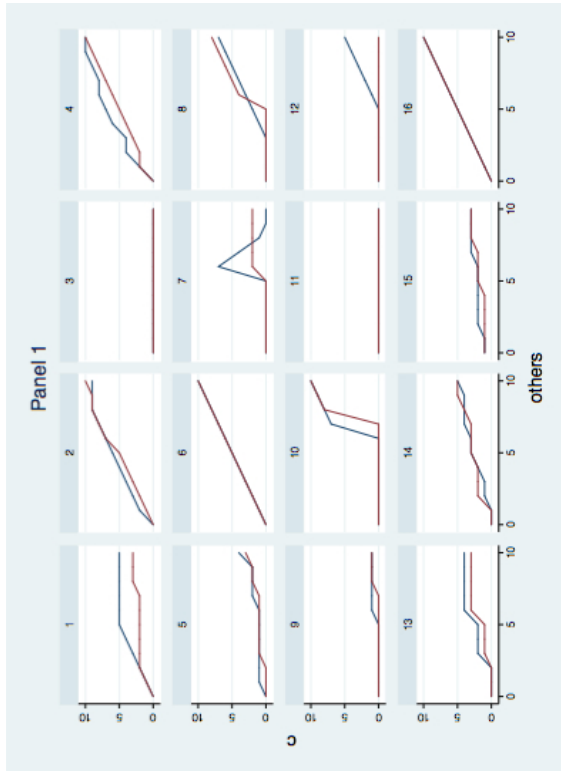
Figure 3.2: Percent of Positive Contributions in PGG

Our results seem to contradict those of Harrison (2007) who reports that house money has a major effect on the fraction of free riders, but no clear impact on the levels of positive contributions. By contrast, in our case we find that the house money seems not to affect the decision to contribute or not while it does affect the level of contributions and particularly in such a way that contributions are more generous with house money. We believe that differences between Harrison's and our results arise from two different features in our experimental designs. We employ a one-shot public good game while Clark (2002)¹⁶'s experimental design collects data from a (10-times) repeated public good game. This repeated game feature of Harrison's data may involve more learning than our one-shot design inducing more equilibrium behaviour. A second feature of our experimental design that may suggest a reason for these striking differences in our results is the within-subject approach we use as opposed to the between-subject design of Clark. The latter in combination with the fact that Clark recruited participants with the requirement that you must bring \$8 to the experiment may make the case for differences in our subject pools. In support of these two arguments made here, as we see in our later discussion, we do observe a low percentage of free-riders in our experimental data both with house money and earned money.

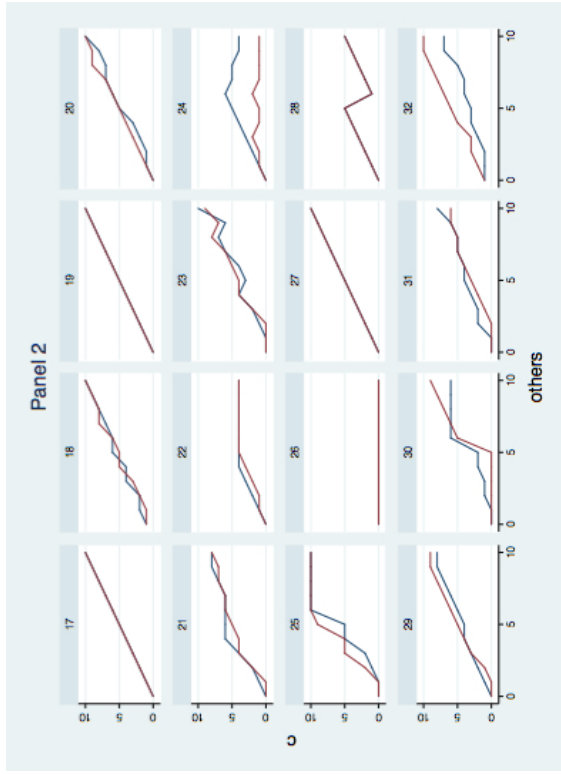
3.5.2 Individual Differences in Contributions and the Role of the Origin of Money

To examine further the conjecture that the observed house money effect seems to be driven by higher contributions when using house money rather than by fewer free riders, we shall focus now on the conditional contributions of participants. Conditional contributions help us to form an impression of individual contribution patterns. As we discussed in the previous section, our experimental design facilitates elicitation of any reciprocal behaviour. In Figure 3.3 we present the contribution pattern of each participant both with HM and EM. This figure is already a good indicator of the discussion to follow. At first glance it is apparent that the contribution patterns seem relatively similar across both types of money and we are even able to observe that any differences are mainly in terms of levels of contributions rather than contribution patterns.

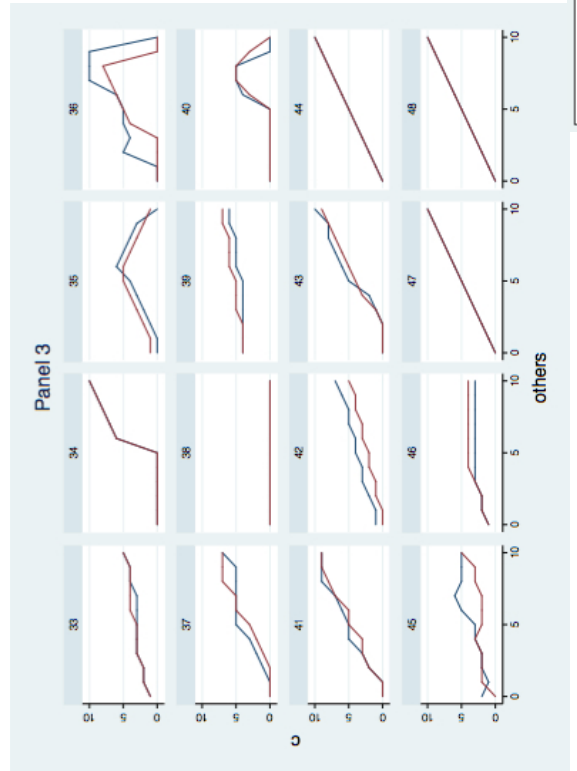
¹⁶Just a reminder that Harrison's work is a comment of earlier work by Clark.



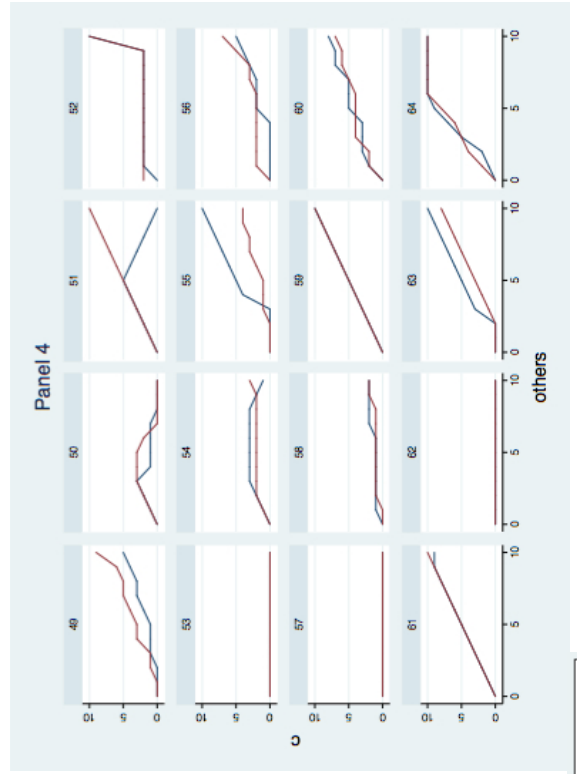
(a) Participants 1-16



(b) Participants 17-32



(c) Participants 33-48



(d) Participants 49-64



Using Fischbacher et al. (2001)'s innovative experimental design and technique of classification we have classified participants in three main categories: free riders, conditional co-operators and humped-shaped contributors. We begin, first, with the classification in the case where participants used HM:

- Free Riding: Seven¹⁷ participants, that is, 10.9% of participants, exhibited purely selfish behaviour by contributing zero in all entries of the contribution table.
- Conditional cooperation: We have classified 33 participants (51.56%) as conditional co-operators. We first identify those who we call **perfect conditional co-operators**: nine¹⁸ subjects exhibit this behaviour by contributing exactly as much as their group's average (Spearman correlation $\rho = 1$). We then consider a group of conditional co-operators **with small deviations** from perfect conditional cooperation. This is based on the shape of the graph of their contributions relative to others', which in more formal terms translates a Spearman correlation coefficient that lies in the range $0.99 < \rho < 1$. In this subcluster, we have assigned nine¹⁹ participants. Finally, we classified as **imperfect conditional co-operators** those that seemed to condition their contribution on others' average contribution, but they either contributed less (or in a few cases more) than what others did (below or above the 45-degree line). Alternatively, these participants may have employed a strategy whereby they contributed a fixed amount for some levels of others' contribution and a larger fixed amount for higher contributions by others, so that their graph would look more like a positively -sloped step function. In terms of the Spearman correlation coefficient we focused here on the range $0.94 < \rho < 0.99$, and given those criteria we have managed to classify as imperfect conditional co-operators 15 participants²⁰ when using HM. There are some participants with lower Spearman correlation coefficients than those used above that still exhibited a conditional cooperation - like pattern of contribution but we discuss those in the 'Other' category.
- 'Hump-shaped' contributions: Fourteen²¹ of the participants (21.9%) displayed this contribution behaviour. More precisely, in this cluster we have included participants that were close to conditional cooperation up to approximately £5 average contribution of others but thereafter either starting decreasing their contributions or contributed a fixed amount.
- Other: In this subcategory we include those who, despite not strictly falling into anyone of the above mentioned categories still manifest patterns of contribution that may relate to conditional cooperation. For instance, we either

¹⁷See participants 3, 11, 26, 38, 53, 57, 62.

¹⁸6, 16, 17, 19, 27, 44, 47, 48, 59.

¹⁹See participants 2, 4, 18, 20, 29, 41, 43, 61, 63.

²⁰See participants 13, 14, 21, 23, 25, 31, 32, 33, 37, 39, 42, 49, 55, 60, 64.

²¹See participants 1, 7*, 22, 24, 28*, 30, 35, 36, 40*, 45, 46, 50, 51, 54.

observe participants contributing very small amounts compared to the others' contribution but even in this case their (small) contribution is increasing in the average contribution of others²². Others tended to contribute 0 for average contribution of others below (approximately) £5 and then displayed a conditional cooperation pattern afterwards²³. For that reason, we actually choose to call this cluster '**quasi-conditional co-operators**' and 10 participants have been classified as such, that is 15.6%.

We now proceed to present the corresponding classification when EM was used by the participants while making decisions about their contributions in the public good game. We have followed exactly the same classification criteria as above.

- Free riders: Eight²⁴ participants (12.5%) displayed free-riding behaviour by making 0 contributions at any level of others' average contribution. They consist of exactly the same participants who free rode with HM, as well as participant 12 who displayed quasi-conditionally cooperative behaviour when using HM.
- Conditional co-operators: 53.1% of participants, that is 34 out of 64, have been classified as conditional co-operators. We observed eleven participants exhibiting **perfect conditional cooperation** consisting of the same cohort observed in the HM case plus two additional participants (participant 51 and participant 61). In the subcategory of those **with small deviations** from perfect conditional cooperation we find once again the same participants as before, with only exception being participant 61 who now behaves as mentioned above- as a perfect conditional co-operator. Last but not least, the consistency of the group of **imperfect conditional co-operators** remained unchanged across the different sources of money.
- 'Hump-shaped' contributors: When using EM, only eight participants (12.5%) displayed this pattern of behaviour, who incidentally also exhibited such behaviour when using HM. Six participants made 'hump-shaped' contributions with HM, but now with EM they either behaved as quasi-conditional co-operators²⁵ or (in the case of participant 51) as perfect co-operators.
- Other: Fourteen participants (21.9%) using EM behaved as quasi-conditional co-operators. To those with (small) contributions, but still increasing in the average contribution of others, participants 24, 45 and 54 were added. Those three participants had displayed a 'hump-shaped' contribution pattern when using HM. Participants 7 and 30 -previously classified as 'hump-shaped' contributors with HM - joined the other subgroup of those who tended to contribute 0 for average contribution of others below (approximately) £5 and conditional cooperation pattern thereafter, while participant 12 switched to free-riding with EM.

²²See participants 5, 9*, 15, 58.

²³See participants 8, 10, 12, 34, 52, 56.

²⁴See participants 3, 11, 12, 26, 38, 53, 57, 62.

²⁵See participants 7, 24, 30, 45, 54.

It is already evident by the figures presented for each participant and by our discussion above that classification appears to be independent of the origin of the money used when deciding on the contributions in this particular linear public good game. We observed that only eight²⁶ participants (12.5%) changed their pattern of conditional contributions across the different sources of money. In reality for some of them (like participant 61) this was only a slight ‘change’ but we have decided to classify him/her differently to maintain consistency with the criteria we have used throughout. Our classification both with HM and EM seems to be consistent with those of Fischbacher et al. (2001) and Houser and Kurzban (2001) and Kurzban and Houser (2005) with the only difference being that we document a lower percentage of both free-riders²⁷ and perfect co-operators. The above evidence seems to support our hypothesis that:

Hypothesis 3.3. *The types’ classification remains robust across treatments and is independent of the way endowments have emerged.*

To test this hypothesis more rigorously, we computed the Spearman correlation coefficient for each participant both with HM and EM and we found that there is no statistically significant difference across the different sources of money (Wilcoxon matched-pairs signed-rank test p -value= 0.4907).

3.5.3 The HME and the Anticipatory Reciprocity

We now turn the focus of our analysis to the beliefs about others in order to discuss within our experiment the concept of ‘anticipatory reciprocity’. As it has been discussed extensively in Section 2 of this chapter ‘anticipatory reciprocity’ refers to the situation where people anticipate a reciprocation of their contributions, and for that they tend to contribute more when others are also able to contribute more. We shall start our analysis in this subsection by discussing the beliefs about others’ contributions in the public account. We first test whether participants change their beliefs about others when they know that earned money is at stake rather than house money. We expect that beliefs about others’ behaviour are also subject to the house money effect and for that we set our testing hypothesis as following:

Hypothesis 3.4. *Beliefs about others’ average contributions in the public account differ across different sources of money.*

The experimental data seem to confirm the above hypothesis, as participants were expecting other participants to contribute on average less when earned money was used rather than when house money was at stake (Wilcoxon signed rank test p -value= 0.0000). We do recognise that this statement of beliefs could

²⁶See the discussion on classification earlier and also the corresponding figures or participants 7, 12, 24, 30, 45, 51, 54, 61.

²⁷Regarding the low percentage of free riders we suggest two possible impromptu explanations the high percentage of female participants (a probit specification and a Spearman correlation test suggest positive relation between the probability to make a positive contribution and being a female) and the fact that we intentionally excluded students of economics and other related degrees from our subjects’ pool in order to avoid any previous knowledge of game theory.

work as a kind of self-justification mechanism for participants, that is, participants use this question about expressing their beliefs as a way of justifying their contribution decision. We have two arguments against this claim. First, as it is apparent in our experimental design, stating ‘true’ beliefs is incentivised by paying participants on ‘successful guesses’. Given the anonymity that is maintained throughout our experiment and the possibility of earning up to £2 more²⁸, we believe that participants were strongly incentivised to truly revealed their beliefs about others. Our second argument comes from the comparison of unconditional contributions and beliefs about others. We have tested the following hypothesis:

Hypothesis 3.5. *Participants (unconditionally) contribute in the public account as much as they believe that other participants will contribute on average.*

Comparing unconditional contributions and beliefs about others’ average contribution, we report that when house money was at stake participants contributed less than what was stated as their belief of others’ contribution on average (One-sided sign test p -value= 0.0066; Wilcoxon test p -value= 0.0148), while with earned money they appeared to match their contributions with their beliefs (Two-sided sign test p -value= 0.2529; Wilcoxon test p -value= 0.2194). These results suggest that participants do not always seem to care about matching their contribution to others’ contributions something that is also suggested by our discussion above on types’ classification. At this point and relating our results here to the discussion in chapters 1 and 2, we would interpret this change in behaviour between house money and earned money as a possible recognition of others’ entitlement over their earned money. People in general seem to expect a lower average contribution with earned money as discussed earlier, but at the same time they also acknowledge the fact that others have expended the same level of effort as themselves to earn this £10, meaning that they ultimately match their contributions to their beliefs. We consider this result a manifestation of ‘anticipatory reciprocity’ as that was discussed by Cherry et al. (2005), Kroll et al. (2007) and Oxoby and Spraggon (2009) who used it to explain evidence of the house money effect that they only find in asymmetric cases with heterogeneous endowments or best-shot production technologies. Our findings seem to diverge from theirs at the point that we find evidence of the house money effect even in homogeneous and symmetric cases. In the same vein with our results Muehlbacher and Kirchner (2009) conduct an experiment where participants are not informed about the asymmetry of the origin of the endowments within their groups and find that participants who earn their endowments by exerting a high level of effort contribute less to the public good than those participants who receive their endowments more easily in the low effort condition.

²⁸We kept this maximum reward small to avoid any hedging confounds as according to Blanco et al. (2010) hedging confounds by rewarding accuracy for belief elicitation might not be completely eliminated but are not a major problem unless the experimental setting offers very prominent hedging opportunities.

3.5.4 Conditional Contributions and the Behavioural Validity of Strategy Method

To further examine the points raised above, we have created the variable 'conditional contribution' c , which is the entry in the contribution table that corresponds to the beliefs of that particular participant. That is, we check the belief of a participant and track down the corresponding 'conditional contribution' in the contribution table. We then first test whether:

Hypothesis 3.6. *'Conditional contributions' are higher when HM is at stake rather than EM.*

Parametric and non-parametric tests (Two-sample t test p -value= 0.0165; Wilcoxon signed rank test p -value= 0.0086) confirm this hypothesis which is in accordance with **Hypothesis 3.1** on unconditional contributions. Given that we have already demonstrated the robustness of types' classification, this result also supports our argument that any house money effect observed is due to differences in the levels of contributions rather than changes in the decision to free ride or not. We also test whether unconditional contributions differ from conditional ones. Our intention here is to examine whether the strategy method setting we use has any effect on contributions. We set up our testing hypothesis as following:

Hypothesis 3.7. *'Conditional contributions' should not be significantly different from 'unconditional contributions'.*

However, our data reject this hypothesis showing that unconditional contributions both with house money and earned money are statistically significantly greater than conditional contributions respectively (Wilcoxon signed rank test p -value < 0.05 for both cases). Participants seemed to be affected by the strategy method framing and contribute less than when they do not condition their contributions. This result does not disprove the behavioural validity of strategy method since, as opposed to Fischbacher and Gächter (2009) who have argued in favour of strategy method by finding that people identified by the strategy method as conditional co-operators also behave as conditional co-operators under the direct response method; our experimental design of one-shot public good game does not allow us to classify participants under their direct response. Our finding just suggests that positive unconditional contribution were greater than positive conditional ones (both with house money and earned money) rather that contributions patterns change when strategy method rather than direct response is used.

Considering this 'conditional contribution' variable again, we last compare conditional contributions with the corresponding average others' contribution. Particularly, we conjecture that:

Hypothesis 3.8. *'Conditional contributions' are lower than the corresponding average contributions of others.*

This hypothesis is in line with the results from **Hypothesis 3.5** although there is a difference in the case of earned money where participants seem to behave differently than before. Our data here show that both with HM and EM

conditional contributions are below the corresponding average contributions of others (Wilcoxon signed rank test p -value= 0.0000 for both cases). This finding is in line with Fischbacher and Gächter (2010) who show that the decline of cooperation can be driven by the fact that most people have a preference to contribute less than others (rather than by their changing beliefs of others' contributions). The last two Hypotheses 3.7 and 3.8 combined imply that removing uncertainty about others' contributions by using the strategy method lowers average contributions. We believe that this result could be attributed to anticipatory reciprocity as well. As participants are called on to state how much to contribute in the public account given others' average contribution, they anticipate with certainty the maximum contribution by others and thus contribute accordingly whereas unconditional contributions can be inflated due to the uncertainty about others' contributions.

3.6 Conclusion

This chapter combines two branches of the experimental literature, that of the house money effect which has extensively been discussed in the previous two chapters and that of individual differences in social preferences. Both these have been used to explain cooperation in social dilemmas but also social preferences have been suggested as a possible explanation for the house money effect. This earlier research motivates our work here where we test the implications of house money on cooperation in social dilemmas. Using the innovative experimental design of Fischbacher et al. (2001), we observe both ‘unconditional contributions’ (with the direct response method) and ‘conditional contributions’ (with the strategy method). The strategy method approach allows us to classify participants according to their behaviour in the public good game while by adding the within-subject element in our experimental design we can test the robustness of this classification across the different origins of endowments. Unconditional contributions along with elicited beliefs help us reconsider the discussion around anticipatory reciprocity as an explanation of the house money effect.

Our findings first confirm the presence of the house money effect in the public good game and second, contrary to Harrison (2007) they support that the decision to free ride or not as well as the types’ classification is independent of the origin of money. Our experimental evidence seems consistent both with anticipatory reciprocity and fairness or deservingness concerns. Participants both unconditionally and conditionally contributed lower with earned money rather than with house money and they anticipated contribution of others in a similar fashion; but in the case of earned money they matched their (unconditional) contributions to their beliefs. We consider the latter consistent with our earlier discussion on the interpretation of the house money effect as ‘Lockean desert effect’. Our last findings report differences between conditional and unconditional contribution suggesting that the strategy method approach may deflate contributions.

We believe that our findings here add on the discussion on the house money effect and the treatment that it should receive within the experimental methodology; but more importantly, we believe we contribute to the ongoing debate regarding cooperation in social dilemmas. Our findings suggest that further research on empirically testing other-regarding preferences models can shed light to confounding results.

[Appendices]

Appendix A

This appendix includes the experimental instructions for the two kinds of sessions, namely HMEM and EMHM; an example of the power point presentation that accompanied the instructions; the short questionnaire; and examples of the puzzles used in the real effort task.

A.1 Experimental Instructions

Experiment Instructions

Dear participants,

Welcome and thank you for participating in the experiment. The experiment will last about 1 1/2 hours. Please switch off your mobile phones and remain silent from now on until the end of the experiment. You will have the opportunity to ask questions in a few minutes. No discussions between the participants are allowed and you should always face your screen unless a member of the experimental team reads instructions. Do not try to check the screens of other participants around you.

The experiment consists of a number of different situations. In each situation you will be asked to make a choice. We will describe each situation successively and explain precisely what you have to do. You will also receive instructions on the computer screen and be guided throughout each stage.

Each of these situations is independent and should be treated as such (i.e. your choice in one situation has no implications for another).

In each situation, your earnings will depend on your choices, possibly the choices of others and chance. You will have the opportunity to ask any questions related to the instructions before you take the actual decision. This is only to make sure you understood the instructions. When each situation is over, there will be a very short pause and then we will introduce the next situation. To be more precise before the decision to be taken in situation there is a screen with the title "Instructions". When you have this screen in front of we will have a pause while the member of the experimental team explains the instructions for this particular situation. You will only be informed about the outcomes of all situations at the end of the session. We will show you a screen with the outcomes corresponding to each situation and another one with your final payment.

At the end of the session, the computer will choose one of the situations randomly for each of you and this situation shall be used as the basis for determining your payment. That is, **your actual earnings will depend on the choices you made in one of the situations only**. On your desk there is a Short Questionnaire about some background characteristics, which we kindly ask you to answer before you leave the room at the end of the session. All this information will be kept confidential and anonymous.

As we have already mentioned before we ask you to remain silent during the whole experiment. Those who do not respect the silence requirement will be asked to leave the experimental room. Once the experiment is finished, please remain seated. You will be called up successively by the number on your table; you will then receive an envelope with your earnings and you will be asked to sign a receipt.

Important note: The CEREP lab has a **strict "no deception" policy**. That means that **under no circumstances** will participants to experiments be deceived. All the information you will receive from us is true. For example, if we tell you that you have been paired to another participant in the room, this is indeed the case.

Finally, note that your participation is considered voluntary and you are free to leave the room at any point if you wish to do so. In that case, we will only pay you the show up payment of £3.

Please, leave all the instructions & the Short Questionnaire on your table when you leave the room. You can take notes on the pages of the instructions if you wish to do so.

If you have any questions so far, please raise your hand now.

Situation 1

We have formed pairs of participants at random in the room. Each pair consists of a First Mover (FM) and a Second Mover (SM). The allocation of roles will be determined randomly by flipping a coin by the computer only after you have made your choices both as a FM and as a SM.

We –the experimental team- will credit the account of each FM with £10. As FM, you will choose whether to keep this £10 or transfer it to the SM you are paired with.

- If the FM decides to keep the £10, the SM earns nothing.
- If the FM decides to transfer the £10 to the SM, the money will be tripled by us before it is passed on to SM. That is, SM will receive £30.

As SM, you will be able to choose whether to keep the £30 or transfer £15 back to the FM you are paired with.

- If SM keeps the £30 and does not transfer anything to the FM, SM's account will be credited with £30 and FM earns nothing (£0 credited to FM's account).
- If SM transfers £15 back to the FM of the pair, both FM and SM will earn £15 (both accounts will be credited with £15).

Predictions about Others' Choices

In this situation, you will also be asked to predict the choices of other participants (both as FM and as SM). Specifically, we will ask you to state how many of the other 15 participants in the room do you expect will transfer £10 as FM / transfer £15 back as SM.

You will be rewarded for correct guesses with additional payment.

That is, if you predict **exactly** how many of the 15 other participants in the room they said they would transfer £10 as FM / transfer £15 back as SM you will gain £2 for each prediction, respectively. If your prediction deviates by -1 or +1 of the actual number you will gain £1 for each prediction, respectively. If your prediction deviates by -2 or +2 you will gain £0.50 for each prediction, respectively. If your prediction deviates by -3 or +3 or more you will gain nothing (£0), respectively.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 2

We will now propose to you nine different choices between a fixed amount of money (£10) that we have credited to your account and an all-or-nothing lottery.

The lottery will work as follows: The computer will roll a ten-sided die. According to the rule stated in the lottery, you will earn nothing (£0) if the die indicates some of the numbers or £15 if the die indicates the rest of the numbers.

For example, **if you choose the lottery** that states { £0 if the die shows 1 or 2; £15 if the die show 3, 4, 5, 6, 7, 8, 9, 10} you will earn nothing if the die indicates 1 or 2 and earn £15 if the die indicates 3, 4, 5, 6, 7, 8, 9 or 10. **If you choose to keep the £10** rather than picking the all-or-nothing lottery, you will earn £10.

You will be asked to indicate your preferred option in each of the cases.

Finally, the computer will pick at random a number between 1 and 9 to determine which of the 9 cases will be relevant to determine your earnings in this situation.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 3

We have formed groups of four participants at random in the room. You have been assigned to one of these groups. None of you will know who is in his/her group. The experimenter is the only one who knows who is in which group. You will only know the number of the group you belong to and your individual number in the group.

For example, if you are told that your group number is 5 and your number in the group is 3, then you are member number 3 in group 5.

Each member of the group has to decide on the division of £10 that has been allocated to each of you by us, the experimental team. You can put this £10 in your private account or you can transfer it fully or partially into a project. Each pound you do not transfer to the project will automatically be credited to your private account.

Your income from the private account:

For each £1 you put in your private account, you will receive exactly £1. Nobody except you earns something from your private account.

Your income from the project:

We will add up the transfers made by the four members of your group to the project. Each member will then receive an income from the project calculated as follows:

Income from the project = Sum of all transfers of the members of the group $\times 0.4$

Your total income:

Your total income is the sum of your income from your private account and your income from the project.

We will ask you to make two types of decisions in this situation, which we will refer to below as the “transfer” and “transfer table”.

- You first decide how much of the £10 you want to transfer to the project without knowing what the other three members of your group intend to transfer to the project. We will call this your “transfer”.
- Your second task is to fill in a “transfer table”. The table contains all possible average transfers by the other three group members (rounded up to the closest integer). For each number in the table, you simply have to insert in the input box how much of the £10 you want to transfer to the project if that number was the average transfer by the other three members. For example, you will have to indicate how much you would transfer to the project if the others transfer £0 on average to the project, how much you would transfer if the others transfer £1, £2 or £3 etc. You can insert any integer numbers from 0 to 10 in each input box. All input boxes must be filled in.

After all the members of your group have made a “transfer” and filled their “transfer table”, a group member number will be chosen at random. For example, if number 2 is selected, this means that every participant with number 2 in each group will have been selected. For this participant, his/her “transfer table” choices will be relevant, while for the other members of the group their “transfers” will be the relevant ones. By ‘relevant’ we mean that the **sum of all transfers of the members of the group** will be the “transfers” of member numbers 1, 3 and 4 and the respective choice in the “transfer table” for the member number 2. The respective choice in the “transfer table” for the member number 2 is the transfer on the table that corresponds to the average of the “transfers” of the other 3 members.

Two examples should make this clear.

Example 1: Assume that **your number in the group** is selected randomly. This implies that your relevant decision will be your “transfer table”. The “transfer” is the relevant decision for the other three group members. Assume they made “transfers” of £0, £2 and £4. The average transfer of these three group members is, therefore, £2. If you indicated in your “transfer table” that you will transfer £1 if the others transfer £2 on average, then the total transfer of the group to the project is given by $£0+£2+£4+£1=£7$. All group members, therefore, earn $0.4 \times £7 = 2.8$ from the project plus their respective income from their private account (e.g. your earnings would be $£9+£2.8=£11.8$). If, instead, you have indicated in your “transfer table” that you would transfer £9 if the others transfer £2 on average, then the total transfer of the group to the project is given by $£0+£2+£4+£9=£15$. All group members therefore earn $0.4 \times £15 = £6$ from the project plus their respective income from the private account (e.g. your earnings in this case would be $£1+£6=£7$).

Example 2: Assume **that the random mechanism did not select your number in the group**, implying that the “transfer” is taken as the **payoff-relevant decision** for you and two other group members. Assume your “transfer” is £6 and those of the other two group members are £8 and £10. The average transfer of your “transfer” and those of the two other group members, therefore, is £8. If the group member whose number in group has been selected indicates in his/her “transfer table” that he/she will transfer £1 if the other three group members transfer on average £8, then the total transfer of the group to the project is given by $£6+£8+£10+£1=£25$. All group members will therefore earn $0.4 \times £25 = £10$ from the project plus their respective income from the private account (e.g. your earnings would be $£4+£10=£14$). If, instead, the randomly selected group member indicates in his/her “transfer table” that he/she transfers £9 if the others transfer on average £8, then the total transfer of that group to the project is $£6+£8+£10+£9=£33$. All group members will therefore earn $0.4 \times £33 = £13.2$ from the project plus their respective income from the private account (e.g. your earnings would be $£4+£13.2=£17.2$).

Estimate of Others’ Decision

In addition, in this situation, you will be asked to estimate the decision of other members of your group. Specifically, we will ask you to state how much of the £10 you think each of the other three members of your group will transfer to the project on average.

You will be rewarded for correct guesses with additional payment.

That is, if you estimate **exactly** how much is the average “transfer” of the other three group members you will gain £2. If your estimate deviates by -1 or +1 of the actual average “transfer” you will gain £1. If your estimate deviates by -2 or +2 you will gain £0.50. If your prediction deviates by -3 or +3 or more you will gain nothing (£0).

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 4

This situation is similar to the one described in **Situation 1**.

Once more, we have formed pairs of participants at random in the room. Each pair consists of a First Mover (FM) and a Second Mover (SM). None of you will know whom you have been paired with and it is definitely **not** the person you were matched with in Situation 1. The allocation of roles will be determined randomly by flipping a coin by the computer only after you have made your choices both as a FM and as a SM.

In this situation you are now asked to use the £10 you earned by solving the puzzle in Situation *. That is, if you are chosen as **FM** you are now **using the £10 you earned in Situation ***. As FM, you will choose whether to keep this £10 or transfer it to the SM you are paired with.

- If the FM decides to keep the £10, the SM earns nothing.
- If the FM decides to transfer the £10 to the SM, the money will be tripled by us before it is passed on to SM. That is, SM will receive £30.

As SM, you will be able to choose whether to keep the £30 or transfer £15 back to the FM you are paired with.

- If SM keeps the £30 and do not transfer anything to the FM, SM's account will be credited with £30 and FM earns nothing (£0 credited to FM's account).
- If SM transfers £15 back to the FM of the pair, both FM and SM will earn £15 (both accounts will be credited with £15).

Note that your partner in your pair has also gained the £10 that he/she will be using if he/she is selected as a FM by solving the same "puzzle" in **Situation***. (If not, that will be explicitly stated to you.)

Predictions about Others' Choices

In this situation, you will also be asked to predict the choices of other participants (both as FM and as SM). Specifically, we will ask you to state how many of the other 15 participants in the room do you expect will transfer £10 as FM / transfer £15 back as SM.

You will be rewarded for correct guesses with additional payment.

That is, if you predict **exactly** how many of the 15 other participants in the room transferred £10 as FM / transferred £15 back as SM you will gain £2 for each prediction, respectively. If your prediction deviates by -1 or +1 of the actual number you will gain £1 for each prediction, respectively. If your prediction deviates by -2 or +2 you will gain £0.50 for each prediction, respectively. If your prediction deviates by -3 or +3 or more you will gain nothing (£0), respectively.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 5

In this situation you are again asked to participate by using the £10 earned at **Situation ***.

Recall that the result of Situation 4 -like the results of all the previous situations- is irrelevant for the purposes of this situation because only one of the situations will be **payable to you**.

This situation is similar to the one described in **Situation 2**. We will again propose to you nine different choices between a fixed amount of money (the £10 from **Situation ***) and an all-or-nothing lottery.

The lottery will work as follows: The computer will roll a ten-sided die. According to the rule stated at the lottery, you will earn nothing (£0) if the die indicates some of the numbers or £15 if the die indicates the rest of the numbers.

For example, **if you choose that lottery** that states { £0 if the die shows 1 or 2; £15 if the die show 3, 4, 5, 6, 7, 8, 9, 10} you will earn nothing if the die indicates 1 or 2 and earn £15 if the die indicates 3, 4, 5, 6, 7, 8, 9 or 10. **If you choose to keep the £10** rather than picking the all-or-nothing lottery, you will earn £10.

You will be asked to indicate your preferred option in each of the cases.

Finally, the computer will pick at random a number between 1 and 9 to determine which of the 9 cases will be relevant to determine your earnings in this situation.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 6

In this situation you are asked to participate by using the £10 earned in **Situation ***.

This situation is similar to the one described in **Situation 3**. We have again formed groups of four participants at random in the room. You have been assigned to one of these groups. None of you will know who is in his/her group. The experimenter is the only one who knows who is in which group. You will only know the number of the group you belong to and your individual number in the group. For example, if you are told that your group number is 5 and your number in the group is 3, then you are member number 3 in group 5. The composition of your group will not be the same as the one in Situation 3. You will be matched with other participants in the room but not the same as before.

Each member of the group has to decide on the division of £10 that he/she earned by solving the “puzzle” in **Situation ***. (If this is not the case, it will be explicitly stated to you). You can put this £10 in your private account or you can transfer it fully or partially into a project. Each pound you do not transfer will automatically be credited to your private account.

Your income from the private account:

For each £1 you put in your private account, you will receive exactly £1. Nobody except you earns something from your private account.

Your income from the project:

We will add up the transfers made by the four members of your group to the project. Each member will then receive an income from the project calculated as follows:

Income from the project = Sum of all transfers of the members of the group x 0.4

Your total income:

Your total income is the sum of your income from your private account and your income from the project.

We will ask you to make two types of decisions in this situation, which we will refer to below as the “transfer” and “transfer table”.

- You first decide how much of the £10 you want to transfer to the project without knowing what the other three members of your group intend to transfer to the project. We will call this your “**transfer**”.
- Your second task is to fill in a “**transfer table**”. The table contains all possible average transfers by the other three group members (rounded up to the closest integer). For each number in the table, you simply have to insert in the input box how much of the £10 you want to transfer to the project if that number was the average transfer by the other three members. For example, you will have to indicate how much you would transfer to the project if the others transfer £0 on average to the project, how much you would transfer if the others transfer £1, £2 or £3 etc. You can insert any integer numbers from 0 to 10 in each input box. All input boxes must be filled in.

After all the members of your group have made a “transfer” and filled their “transfer table”, a group member number will be chosen at random. For example, if number 2 is selected, this means that every participant with number 2 in each group will have been selected. For this participant, his/her “transfer table” choices will be relevant, while for the other members of the group their “transfers” will be the relevant ones. By ‘relevant’ we mean that the **sum of all transfers of the members of the group** will be the “transfers” of member numbers 1, 3 and 4 and the respective choice in the “transfer table” for the member number 2. The respective choice in the “transfer table” for the member number 2 is the transfer on the table that corresponds to the average of the “transfers” of the other 3 members.

Remember the examples given in Situation 3.

Estimate of Others’ Decision

In addition, in this situation, you will be asked to estimate the decision of other members of your group. Specifically, we will ask you to state how much of the £10 you think each of the other three members of your group will transfer to the project on average.

You will be rewarded for correct guesses with additional payment.

That is, if you estimate **exactly** how much is the average “transfer” of the other three group members you will gain £2. If your estimate deviates by -1 or +1 of the actual average “transfer” you will gain £1. If your estimate deviates by -2 or +2 you will gain £0.50. If your prediction deviates by -3 or +3 or more you will gain nothing (£0).

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Experiment Instructions

Dear participants,

Welcome and thank you for participating in the experiment. The experiment will last about 1 1/2 hours. Please switch off your mobile phones and remain silent from now on until the end of the experiment. You will have the opportunity to ask questions in a few minutes. No discussions between the participants are allowed and you should always face your screen unless a member of the experimental team reads instructions. Do not try to check the screens of other participants around you.

The experiment consists of a number of different situations. In each situation you will be asked to make a choice. We will describe each situation successively and explain precisely what you have to do. You will also receive instructions on the computer screen and be guided throughout each stage.

Each of these situations is independent and should be treated as such (i.e. your choice in one situation has no implications for another).

In each situation, your earnings will depend on your choices, possibly the choices of others and chance. You will have the opportunity to ask any questions related to the instructions before you take the actual decision. This is only to make sure you understood the instructions. When each situation is over, there will be a very short pause and then we will introduce the next situation. To be more precise before the decision to be taken in situation there is a screen with the title "Instructions". When you have this screen in front of we will have a pause while the member of the experimental team explains the instructions for this particular situation. You will only be informed about the outcomes of all situations at the end of the session. We will show you a screen with the outcomes corresponding to each situation and another one with your final payment.

At the end of the session, the computer will choose one of the situations randomly for each of you and this situation shall be used as the basis for determining your payment. That is, **your actual earnings will depend on the choices you made in one of the situations only**. On your desk there is a Short Questionnaire about some background characteristics, which we kindly ask you to answer before you leave the room at the end of the session. All this information will be kept confidential and anonymous.

As we have already mentioned before we ask you to remain silent during the whole experiment. Those who do not respect the silence requirement will be asked to leave the experimental room. Once the experiment is finished, please remain seated. You will be called up successively by the number on your table; you will then receive an envelope with your earnings and you will be asked to sign a receipt.

Important note: The CEREP lab has a **strict "no deception" policy**. That means that **under no circumstances** will participants to experiments be deceived. All the information you will receive from us is true. For example, if we tell you that you have been paired to another participant in the room, this is indeed the case.

Finally, note that your participation is considered voluntary and you are free to leave the room at any point if you wish to do so. In that case, we will only pay you the show up payment of £3.

Please, leave all the instructions & the Short Questionnaire on your table when you leave the room. You can take notes on the pages of the instructions if you wish to do so.

If you have any questions so far, please raise your hand now.

Situation 1

We have formed pairs of participants at random in the room. Each pair consists of a First Mover (FM) and a Second Mover (SM). The allocation of roles will be determined randomly by flipping a coin by the computer only after you have made your choices both as a FM and as a SM.

In this situation, you are asked to use the £10 you earned by solving the “puzzles” in Situation *. That is, if you are chosen as **FM** you are now **using the £10 you earned in Situation ***. As FM, you will choose whether to keep this £10 or transfer it to the SM you are paired with.

- If the FM decides to keep the £10, the SM earns nothing.
- If the FM decides to transfer the £10 to the SM, this money earned in Situation * will be tripled by us before it is passed on to SM. That is, SM will receive £30.

As SM, you will be able to choose whether to keep the £30 or transfer £15 back to the FM you are paired with.

- If SM keeps the £30 and does not transfer anything to the FM, SM’s account will be credited with £30 and FM earns nothing (£0 credited to FM’s account).
- If SM transfers £15 back to the FM of the pair, both FM and SM will earn £15 (both accounts will be credited with £15).

Note that your partner in your pair has also gained the £10 that he/she will be using if he/she is selected as a FM by solving the same “puzzles” in **Situation***.

Predictions about Others’ Choices

In this situation, you will also be asked to predict the choices of other participants (both as FM and as SM). Specifically, we will ask you to state how many of the other 15 participants in the room do you expect will transfer £10 as FM / transfer £15 back as SM.

You will be rewarded for correct guesses with additional payment.

That is, if you predict **exactly** how many of the 15 other participants in the room they said they would transfer £10 as FM / transfer £15 back as SM you will gain £2 for each prediction, respectively. If your prediction deviates by -1 or +1 of the actual number you will gain £1 for each prediction, respectively. If your prediction deviates by -2 or +2 you will gain £0.50 for each prediction, respectively. If your prediction deviates by -3 or +3 or more you will gain nothing (£0), respectively.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 2

In this situation you are again asked to participate by using the £10 earned at **Situation ***.

Recall that the result of Situation 1 is irrelevant for the purposes of this situation because **only one of the situations** will be **payable to you**.

We will propose to you nine different choices between a fixed amount of money (the £10 from **Situation ***) and an all-or-nothing lottery.

The lottery will work as follows: The computer will roll a ten-sided die. According to the rule stated at the lottery, you will earn nothing (£0) if the die indicates some of the numbers or £15 if the die indicates the rest of the numbers.

For example, **if you choose that lottery** that states { £0 if the die shows 1 or 2; £15 if the die show 3, 4, 5, 6, 7, 8, 9, 10} you will earn nothing if the die indicates 1 or 2 and earn £15 if the die indicates 3, 4, 5, 6, 7, 8, 9 or 10. **If you choose to keep the £10** rather than picking the all-or-nothing lottery, you will earn £10.

You will be asked to indicate your preferred option in each of the cases.

Finally, the computer will pick at random a number between 1 and 9 to determine which of the 9 cases will be relevant to determine your earnings in this situation.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 3

In this situation you are asked to participate by using the £10 earned in **Situation ***.

We have formed groups of four participants at random in the room. You have been assigned to one of these groups. None of you will know who is in his/her group. The experimenter is the only one who knows who is in which group. You will only know the number of the group you belong to and your individual number in the group.

For example, if you are told that your group number is 5 and your number in the group is 3, then you are member number 3 in group 5.

Each member of the group has to decide on the division of **£10** that he/she earned by solving the “puzzle” in **Situation ***. You can put this £10 in your private account or you can transfer it fully or partially into a project. Each pound you do not transfer will automatically be credited to your private account.

Your income from the private account:

For each £1 you put in your private account, you will receive exactly £1. Nobody except you earns something from your private account.

Your income from the project:

We will add up the transfers made by the four members of your group to the project. Each member will then receive an income from the project calculated as follows:

Income from the project = Sum of all transfers of the members of the group x 0.4

Your total income:

Your total income is the sum of your income from your private account and your income from the project.

We will ask you to make two types of decisions in this situation, which we will refer to below as the “transfer” and “transfer table”.

- You first decide how much of the £10 you want to transfer to the project without knowing what the other three members of your group intend to transfer to the project. We will call this your “**transfer**”.
- Your second task is to fill in a “**transfer table**”. The table contains all possible average transfers by the other three group members (rounded up to the closest integer). For each number in the table, you simply have to insert in the input box how much of the £10 you want to transfer to the project if that number was the average transfer by the other three members. For example, you will have to indicate how much you would transfer to the project if the others transfer £0 on average to the project, how much you would transfer if the others transfer £1, £2 or £3 etc. You can insert any integer numbers from 0 to 10 in each input box. All input boxes must be filled in.

After all the members of your group have made a “transfer” and filled their “transfer table”, a group member number will be chosen at random. For example, if number 2 is selected, this means that every participant with number 2 in each group will have been selected. For this participant, his/her “transfer table” choices will be relevant, while for the other members of the group their “transfers” will be the relevant ones. By ‘relevant’ we mean that the **sum of all transfers of the members of the group** will be the “transfers” of member numbers 1, 3 and 4 and the respective choice in the “transfer table” for the member number 2. The respective choice in the “transfer table” for the member number 2 is the transfer on the table that corresponds to the average of the “transfers” of the other 3 members.

Two examples should make this clear.

Example 1: Assume that **your number in the group** is selected randomly. This implies that your relevant decision will be your “transfer table”. The “transfer” is the relevant decision for the other three group members. Assume they made “transfers” of £0, £2 and £4. The average transfer of these three group members is, therefore, £2. If you indicated in your “transfer table” that you will transfer £1 if the others transfer £2 on average, then the total transfer of the group to the project is given by $£0+£2+£4+£1=£7$. All group members, therefore, earn $0.4 \times £7 = 2.8$ from the project plus their respective income from their private account (e.g. your earnings would be $£9+£2.8=£11.8$). If, instead, you have indicated in your “transfer table” that you would transfer £9 if the others transfer £2 on average, then the total transfer of the group to the project is given by $£0+£2+£4+£9=£15$. All group members therefore earn $0.4 \times £15 = £6$ from the project plus their respective income from the private account (e.g. your earnings in this case would be $£1+£6=£7$).

Example 2: Assume **that the random mechanism did not select your number in the group**, implying that the **“transfer” is taken as the payoff-relevant decision** for you and two other group members. Assume your “transfer” is £6 and those of the other two group members are £8 and £10. The average transfer of your “transfer” and those of the two other group members, therefore, is £8. If the group member whose number in group has been selected indicates in his/her “transfer table” that he/she will transfer £1 if the other three group members transfer on average £8, then the total transfer of the group to the project is given by $£6+£8+£10+£1=£25$. All group members will therefore earn $0.4 \times £25 = £10$ from the project plus their respective income from the private account (e.g. your earnings would be $£4+£10=£14$). If, instead, the randomly selected group member indicates in his/her “transfer table” that he/she transfers £9 if the others transfer on average £8, then the total transfer of that group to the project is $£6+£8+£10+£9=£33$. All group members will therefore earn $0.4 \times £33 = £13.2$ from the project plus their respective income from the private account (e.g. your earnings would be $£4+£13.2=£17.2$).

Estimate of Others' Decision

In addition, in this situation, you will be asked to estimate the decision of other members of your group. Specifically, we will ask you to state how much of the £10 you think each of the other three members of your group will transfer to the project on average.

You will be rewarded for correct guesses with additional payment.

That is, if you estimate **exactly** how much is the average “transfer” of the other three group members you will gain £2. If your estimate deviates by -1 or +1 of the actual average “transfer” you will gain £1. If your estimate deviates by -2 or +2 you will gain £0.50. If your prediction deviates by -3 or +3 or more you will gain nothing (£0).

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 4

This situation is similar to the one described in **Situation 1**.

Once more, we have formed pairs of participants at random in the room. Each pair consists of a First Mover (FM) and a Second Mover (SM). None of you will know whom you have been paired with and it is definitely **not** the person you were matched with in Situation 1. The allocation of roles will be determined randomly by flipping a coin by the computer only after you have made your choices both as a FM and as a SM.

In this situation, rather than using the £10 earned in Situation *, we –**the experimental team**- will **credit the account** of each FM **with £10**. As FM, you will choose whether to keep this £10 or transfer it to the SM you are paired with.

- If the FM decides to keep the £10, the SM earns nothing.
- If the FM decides to transfer the £10 to the SM, the money will be tripled by us before it is passed on to SM. That is, SM will receive £30.

As SM, you will be able to choose whether to keep the £30 or transfer £15 back to the FM you are paired with.

- If SM keeps the £30 and do not transfer anything to the FM, SM's account will be credited with £30 and FM earns nothing (£0 credited to FM's account).
- If SM transfers £15 back to the FM of the pair, both FM and SM will earn £15 (both accounts will be credited with £15).

Predictions about Others' Choices

In this situation, you will also be asked to predict the choices of other participants (both as FM and as SM). Specifically, we will ask you to state how many of the other 15 participants in the room do you expect will transfer £10 as FM / transfer £15 back as SM.

You will be rewarded for correct guesses with additional payment.

That is, if you predict **exactly** how many of the 15 other participants in the room transferred £10 as FM / transferred £15 back as SM you will gain £2 for each prediction, respectively. If your prediction deviates by -1 or +1 of the actual number you will gain £1 for each prediction, respectively. If your prediction deviates by -2 or +2 you will gain £0.50 for each prediction, respectively. If your prediction deviates by -3 or +3 or more you will gain nothing (£0), respectively.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 5

This situation is similar to **Situation 2**.

We will now propose to you nine different choices between a fixed amount of money (£10) that we - **the experimental team- have credited** to your account and an all-or-nothing lottery.

Recall that the result of Situation 4 -like the results of all the previous & subsequent situations- is irrelevant for the purposes of this situation because **only one of the situations** will be **payable to you**.

The lottery will work as follows: The computer will roll a ten-sided die. According to the rule stated in the lottery, you will earn nothing (£0) if the die indicates some of the numbers or £15 if the die indicates the rest of the numbers.

For example, **if you choose the lottery** that states { £0 if the die shows 1 or 2; £15 if the die show 3, 4, 5, 6, 7, 8, 9, 10} you will earn nothing if the die indicates 1 or 2 and earn £15 if the die indicates 3, 4, 5, 6, 7, 8, 9 or 10. **If you choose to keep the £10** rather than picking the all-or-nothing lottery, you will earn £10.

You will be asked to indicate your preferred option in each of the cases.

Finally, the computer will pick at random a number between 1 and 9 to determine which of the 9 cases will be relevant to determine your earnings in this situation.

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

Situation 6

This situation is similar to the one described in **Situation 3**. We have again formed groups of four participants at random in the room. You have been assigned to one of these groups. None of you will know who is in his/her group. The experimenter is the only one who knows who is in which group. You will only know the number of the group you belong to and your individual number in the group. For example, if you are told that your group number is 5 and your number in the group is 3, then you are member number 3 in group 5. The **composition of your group** will **not** be **the same** as the one in **Situation 3**. You will be matched with other participants in the room but not the same as before.

Each member of the group has to decide on the division of **£10** that has been **allocated to each of you by us, the experimental team**. You can put this £10 in your private account or you can transfer it fully or partially into a project. Each pound you do not transfer to the project will automatically be credited to your private account.

Your income from the private account:

For each £1 you put in your private account, you will receive exactly £1. Nobody except you earns something from your private account.

Your income from the project:

We will add up the transfers made by the four members of your group to the project. Each member will then receive an income from the project calculated as follows:

Income from the project = Sum of all transfers of the members of the group x 0.4

Your total income:

Your total income is the sum of your income from your private account and your income from the project.

We will ask you to make two types of decisions in this situation, which we will refer to below as the “transfer” and “transfer table”.

- You first decide how much of the £10 you want to transfer to the project without knowing what the other three members of your group intend to transfer to the project. We will call this your “**transfer**”.
- Your second task is to fill in a “**transfer table**”. The table contains all possible average transfers by the other three group members (rounded up to the closest integer). For each number in the table, you simply have to insert in the input box how much of the £10 you want to transfer to the project if that number was the average transfer by the other three members. For example, you will have to indicate how much you would transfer to the project if the others transfer £0 on average to the project, how much you would transfer if the others transfer £1, £2 or £3 etc. You can insert any integer numbers from 0 to 10 in each input box. All input boxes must be filled in.

After all the members of your group have made a “transfer” and filled their “transfer table”, a group member number will be chosen at random. For example, if number 2 is selected, this means that every participant with number 2 in each group will have been selected. For this participant, his/her “transfer table” choices will be relevant, while for the other members of the group their “transfers” will be the relevant ones. By ‘relevant’ we mean that the **sum of all transfers of the members of the group** will be the “transfers” of member numbers 1, 3 and 4 and the respective choice in the “transfer table” for the member number 2. The respective choice in the “transfer table” for the member number 2 is the transfer on the table that corresponds to the average of the “transfers” of the other 3 members.

Remember the examples given in Situation 3.

Estimate of Others’ Decision

In addition, in this situation, you will be asked to estimate the decision of other members of your group. Specifically, we will ask you to state how much of the £10 you think each of the other three members of your group will transfer to the project on average.

You will be rewarded for correct guesses with additional payment.

That is, if you estimate **exactly** how much is the average “transfer” of the other three group members you will gain £2. If your estimate deviates by -1 or +1 of the actual average “transfer” you will gain £1. If your estimate deviates by -2 or +2 you will gain £0.50. If your prediction deviates by -3 or +3 or more you will gain nothing (£0).

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to your computer and follow the next instructions on screen.

A.2 Presentation

Welcome to CEREP Lab

Monday 18th March 2013



**Please
Turn off Your
Mobile Phones**

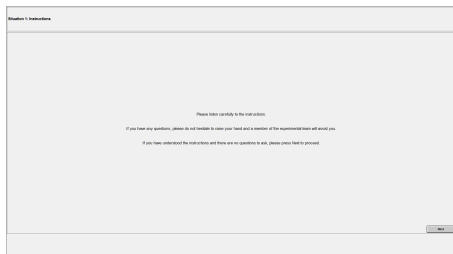


The experiment

- A number of different situations
- In each situation you make a choice/decision
- Situations are **independent**
- Your earnings will depend on your choices, possibly the choices of others & chance

Instructions & Questions

- Before the actual decision:



General Instructions

- You will only be informed about outcomes of all situations at the end of the session
- Actual earnings depend on the choices you made in one of the situations only
- Please fill in the Short Questionnaire on your desk before you leave the room.
- All information is kept confidential and anonymous

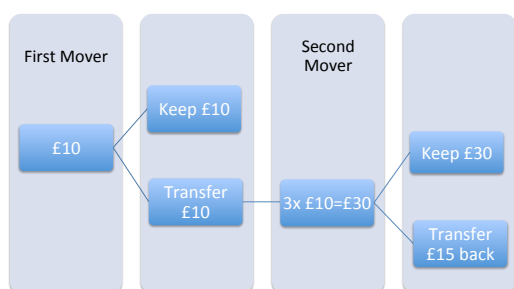
General Instructions (cont.)

- Please remain silent during the whole experiment
- Those who do not respect the silence requirement will be asked to leave the experimental room
- **Once the experiment is finished please remain seated**
- You will be called successively by the number on your table to receive an envelope with your earnings and sign a receipt

General Instructions (cont.)

- CEREP's "no deception" policy
- Your participation is voluntary
- You are free to leave the room at any point
- In that case, you will only receive the show up payment of £3
- At the end please leave the Instructions & the Short Questionnaire on your table when you leave the room
- You can take notes on these pages

Situation 1: Decision



Situation 1: Predictions about Others

- How many of the other 15 participants in this room do you think said that they will transfer £10 if they are chosen as FM?
- How many of the other 15 participants in this room do you think said that they will transfer £15 back if they are chosen as SM?
- Precision awarded by extra £2 per prediction
 - ±1 gives £1 per prediction
 - ±2 gives £0.50 per prediction
 - ±3 or more gives £0 per prediction

Situation 2: Decisions

- 9 different choices between £10 and an all-or nothing lottery

Example

A: { £0 if the die shows 1 or 2; £15 if the die shows 3, 4, 5, 6, 7, 8, 9, 10 }

B: {£10}

- Computer will randomly pick one of the cases to determine your earnings in this Situation

Situation 3: Decision

- Your income from the private account:
For each £1 you put in your private account, you will receive exactly £1. Nobody except you earns something from your private account.
- Your income from the project:
Each member will receive an income from the project calculated as follows:
Income from the project = Sum of all transfers of the members of the group x 0.4
- Your total income:
Your total income is the sum of your income from your private account and your income from the project.

Situation 3: Two kinds of decisions

- “transfer”: how much of £10 you want to transfer to the project
- “transfer table”:

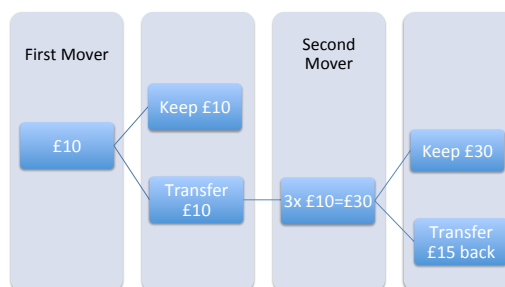
Situation 3: Estimate of Others' Decision

- What is your estimate of the average transfer to the project of the other three group members (round it up to the nearest integer)?
- Precision awarded by extra £2 per prediction
 - ±1 gives £1 per prediction
 - ±2 gives £0.50 per prediction
 - ±3 or more gives £0 per prediction

Situation *: Puzzles

- 25 puzzles given to you
- If you answer 10 or more puzzles correctly you will receive £10
- If you answer less than 10 puzzles correctly you will receive £0
- This £10 will not be directly payable to you but rather will be used in subsequent situations
- You have 10mins to solve (at least) 10 puzzles
- Copy your answers in the computer

Situation 4: Decision



Situation 4: Predictions about Others

- How many of the other 15 participants in this room do you think said that they will transfer £10 if they are chosen as FM?
- How many of the other 15 participants in this room do you think said that they will transfer £15 back if they are chosen as SM?
- Precision awarded by extra £2 per prediction
 - ±1 gives £1 per prediction
 - ±2 gives £0.50 per prediction
 - ±3 or more gives £0 per prediction

Situation 5: Decisions

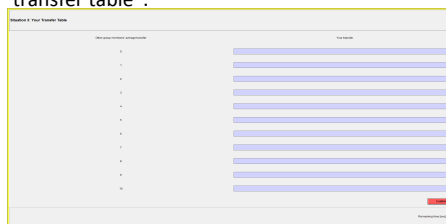
- 9 different choices between £10 and an all-or nothing lottery
- Example
- A: { £0 if the die shows 1 or 2; £15 if the die shows 3, 4, 5, 6, 7, 8, 9, 10 }
- B: {£10}
- Computer will randomly pick one of the cases to determine your earnings in this Situation

Situation 6: Decision

- Your income from the private account:
For each £1 you put in your private account, you will receive exactly £1. Nobody except you earns something from your private account.
- Your income from the project:
Each member will receive an income from the project calculated as follows:
Income from the project = Sum of all transfers of the members of the group x 0.4
- Your total income:
Your total income is the sum of your income from your private account and your income from the project.

Situation 6: Two kinds of decisions

- “transfer”: how much of £10 you want to transfer to the project
- “transfer table”:



Situation 6: Estimate of Others' Decision

- What is your estimate of the average transfer to the project of the other three group members (round it up to the nearest integer)?
- Precision awarded by extra £2 per prediction
 - ±1 gives £1 per prediction
 - ±2 gives £0.50 per prediction
 - ±3 or more gives £0 per prediction

Thank you for your participation

Please remain seated
until you are called up to
receive the envelope with your payment
and sign a receipt

A.3 Short Questionnaire

Table No: _____

Short Questionnaire

1. Gender: Male / Female

2. Age: _____

3. Area of studies: _____

4. Year of studies: _____

5. Nationality: _____

6. Which country have you spent most years of your life living in? _____

7. How much do you spend on average on weekend entertainment? £_____

PLEASE LEAVE THIS ON YOUR TABLE.

THANK YOU FOR YOUR PARTICIPATION

A.4 Puzzles for Real Effort Task

PLEASE DO NOT OPEN THIS UNTIL WHEN A MEMBER
OF THE EXPERIMENTAL TEAM ASKS TO DO SO.

IF YOU DO NOT FOLLOW THE RULES OF THE CEREP
LAB AS DESCRIBED BY THE EXPERIMENTAL TEAM AT
ANY POINT OF THE SESSION YOU ARE RISKING YOUR
EXCLUSION FROM THE EXPERIMENT.

THANK YOU.

Situation *

At this point you are given to 25 “puzzles”. If you manage to solve 10 or more of these puzzles you will receive £10. If you solve less than 10 puzzles correctly you will receive £0.

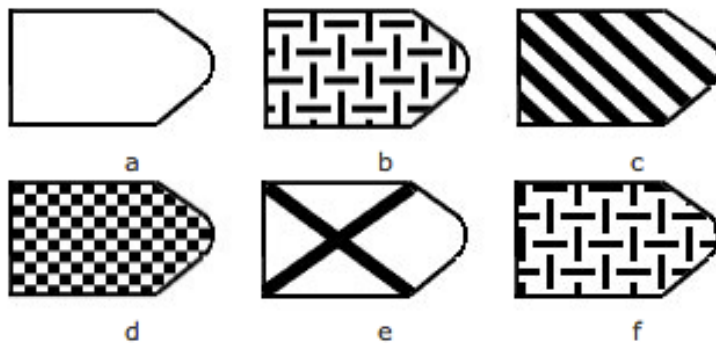
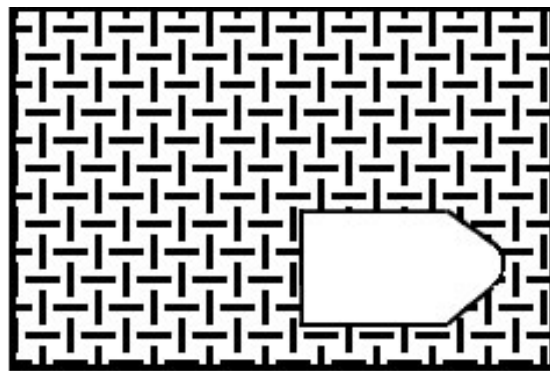
Please note that if you solve the 10 or more puzzles you are asked to, the £10 you earn will **not** be **directly payable** to you **but** rather will be **used in subsequent situations**.

You have 10mins to solve the “puzzles”.

You should copy your answers in the computer following the instructions on your screen.

If you have any questions about the set-up, please raise your hands now and wait for the experimenter to come to you.

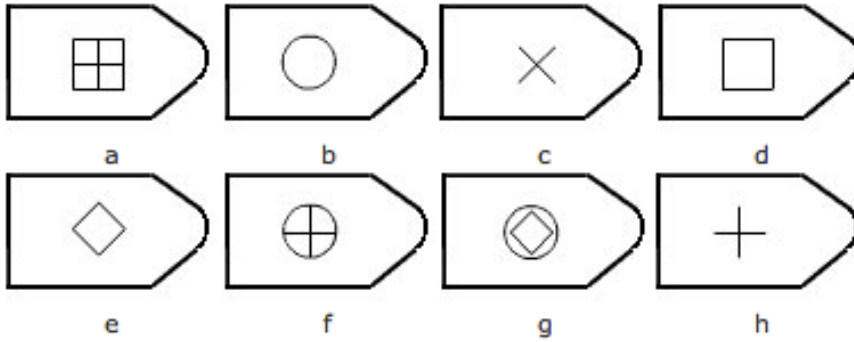
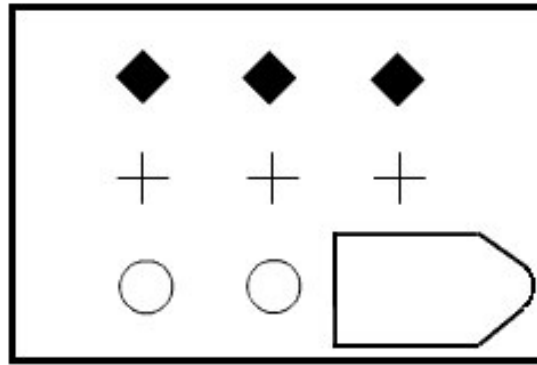
Puzzle 1:



Your answer is ?

- A B C D E F

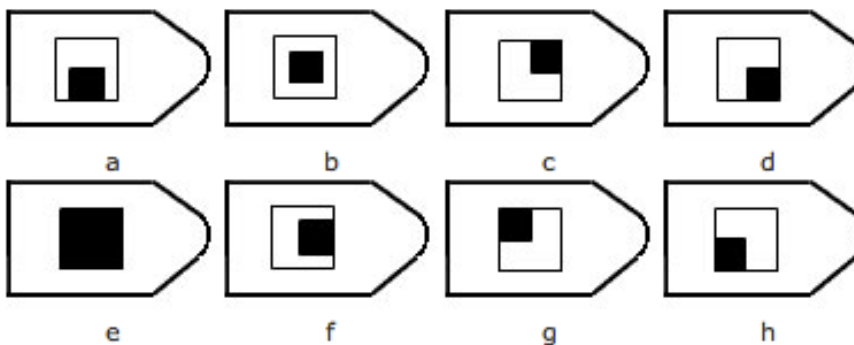
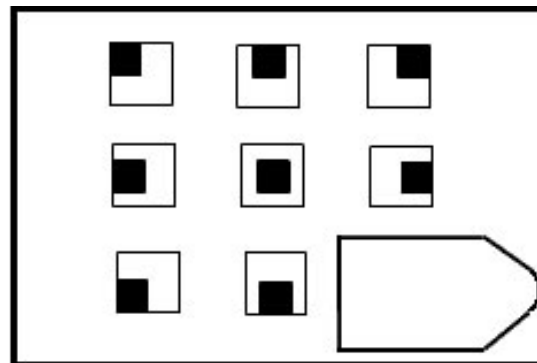
Puzzle 24:



Your answer is ?

- A B C D E F G H

Puzzle 25:



Your answer is ?

- A B C D E F G H

Please make sure you **copy your answers in the computer.**

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