Essays on Inequality, Fairness and Cooperation

Amir Jafarzadeh

Thesis presented for the degree of Doctor of Philosophy in Economics

School of Economics
University of East Anglia

December 2025

Abstract

This thesis explores how individuals perceive inequality, fairness and coordination in strategic and non-strategic environments using experimental economics. Across three studies, it examines whether redistribution preferences depend on procedural fairness and the source of inequality, and how individuals coordinate in environments where outcomes hinge on shared expectations.

The first study investigates the role of procedural fairness by testing whether redistribution preferences differ when inequalities arise from unequal opportunities. Using a novel experimental framework inspired by Sugden and Wang (2020), participants engaged in a card game with equal or unequal opportunities before making redistribution decisions. Results indicate that redistribution choices were largely unaffected by equality of opportunity, suggesting that participants prioritise outcomes over processes when evaluating fairness.

The second study extends this inquiry by examining the effects of strategic choice and strategic luck on redistribution. Unlike prior work that focuses on non-strategic settings and random lotteries, this experiment situates inequality within a strategic coordination game, where incomes depend on both individual and others' decisions. Comparing redistribution under strategic and non-strategic conditions, the findings reveal that initial income distributions exert stronger influence than (strategic and non-strategic) choice versus luck as the source of inequality itself.

The third study shifts focus to coordination behaviour in pure coordination games, addressing why players successfully align choices when labels are present. In a 30-round experiment using labels from diverse categories, participants achieved coordination rates above chance. While online frequency measures (website rankings and Google search rankings) did not predict success, label frequency in books significantly enhanced coordination, highlighting the role of cultural salience in shaping focal points.

Together, these studies contribute to the literature on inequality, fairness and coordination by showing that individuals often emphasise distributive outcomes over procedural considerations, that the initial distributions of incomes outweighs luck versus choice as the origins of inequality, and that cultural salience underpins

coordination success.

Contents

List of Figures

List of Tables

Acknowledgements

Completing this PhD has been a long and deeply formative journey, and I have been fortunate to be supported by many people along the way.

First and foremost, I owe my deepest gratitude to my supervisors, Professor Robert Sugden and Dr Sheheryar Banuri, whose guidance shaped both this thesis and my development as a researcher.

I am profoundly grateful to Bob for his exceptional mentorship. He was always available when needed, and his approach to supervision was defined by constructive dialogue, intellectual openness, and genuine collaboration. At every stage of the research, we worked through problems together, thinking jointly rather than following a predetermined path. Throughout our many discussions, he always sought to persuade rather than dictate, encouraging independent thinking while providing steady guidance. Beyond his scientific influence, I have learned from him important lessons about academic integrity, professionalism, and ethical conduct, qualities that I hope to carry forward throughout my career.

I am equally thankful to Sheheryar for his generous and constant support. Alongside his scientific guidance, he offered patience, understanding, and encouragement, always attentive not only to the progress of the research but also to how I was doing personally. What I admire most in both of my supervisors is that fairness is not merely an object of their research, but something they practice in everyday academic life, particularly in their support for scholars from diverse and developing backgrounds.

I am sincerely grateful to my internal examiner, Dr Stefan Penczynski, and my external examiner, Professor Bertil Tungodden, for their thoughtful and constructive feedback during the viva. Their comments have been invaluable in helping me further develop this work into high quality papers. I particularly appreciated the professionalism, intellectual generosity, and genuinely collaborative spirit of our discussion.

I would also like to thank colleagues and staff in the Department of Economics, including Amrish Patel, Anders Poulsen, Boon Han Koh, Mike Brock, Peter Moffatt, Stefania Sitzia, and Theodore Turocy, for their insightful comments and constructive feedback in seminars and presentations. I am grateful as well to the many researchers and participants at international conferences and workshops whose questions and suggestions helped refine this work.

I also extend my sincere appreciation to the School of Economics, the administrative staff, the Centre for Behavioural and Experimental Social Science (CBESS), and the Laboratory for Experimental and Decision Research (LEDR), whose support and resources have been invaluable throughout my academic journey.

My thanks also go to colleagues in the department, Corrado Di Maria, Emiliya Lazarova, Franco Mariuzzo, Georgios Papadopoulos, James Watson, Martin Bruns, Ritchie Woodard, and Rui Silva, for their collegiality and for creating a supportive and intellectually stimulating academic environment during my years in Norwich. I am also grateful to Scott Summers, Sean Ennis, Silvia Ferrini, and Xin Le, and Scott Summers for their collegial support and encouragement throughout my journey.

I am especially grateful to my friends Alex, Alice, Andrea, Azade, Diego, Junjun, Selvamalai, Sehrish, Valentina, Victoria, for their friendship, encouragement, and for making my years in Norwich happier and more meaningful beyond academia.

Special thanks goes to Ellie, whose steady encouragement, patience, and constant support made this journey lighter and more grounded. Her presence has meant more than words can fully express.

Finally, I would like to thank my family; my parents, Hashem and Adeleh; my sisters, Raheleh and Ghazaleh; my brother-in-law, Babak; and my wonderful nieces, Atrina and Atrisa, for their love and support throughout this journey.

This accomplishment is as much yours as it is mine.

Introduction

Challenges related to inequality, fairness, and coordination are central to economics and social science in general. Inequality shapes redistributive conflicts and political economy outcomes, while fairness considerations determine the extent to which individuals accept income inequalities. Coordination problems, meanwhile, highlight how societies generate conventions and shared expectations in the absence of central authority or explicit communication. This thesis investigates these themes experimentally, focusing on how people perceive fairness in redistributive contexts and how they achieve coordination in environments where success depends entirely on mutual expectations.

The first chapter examines the role of procedural fairness, and specifically equality of opportunity, in shaping redistribution preferences. Much of the literature distinguishes between outcome-based fairness (where unequal distributions are judged unjust and require correction) and procedural fairness (which evaluates the acceptability of outcomes based on the fairness of the process that generated them). Based on our understanding of philosophical and economic theories, from Rawls (1971)'s conception of fair equality of opportunity to Hayek (1976)'s view of markets as rule-based systems, we suggest that individuals may accept unequal outcomes if they arise from equal opportunities. To test this empirically, Chapter 1 develops a novel experiment inspired by Sugden and Wang (2020), in which participants play a card game under both equal and unequal opportunities before entering a redistribution stage. The results show that redistribution preferences remain largely unchanged across treatments: individuals continue to redistribute similar amounts regardless of opportunity structure. This finding suggests that participants give greater weight to outcomes than to procedural fairness, contributing to debates about whether equality of opportunity serves as a fairness benchmark in practice.

The second chapter extends the analysis of fairness to the origins of inequality, focusing on the distinction between choice and luck in strategic versus non-strategic settings. Normative theories such as luck egalitarianism argue that inequalities due to choice are acceptable, while inequalities due to luck require compensation (Dworkin, 1981*a*; Roemer, 1993). A large body of experimental work supports this intuition in lottery-based, non-strategic contexts (e.g., Cappelen et al., 2007; Almås et al., 2010). Yet these studies largely overlook the fact that in real-world strategic environments, individuals' outcomes depend not only on their own actions but also on the actions of others, giving rise to what might be called strategic luck. Chapter 2 introduces an experimental design that compares redistribution in a strategic coordination game with redistribution in a non-strategic framework adapted from Cappelen et al. (2013). Incomes in the strategic setting are shaped by both individual choices and interdependent outcomes. The findings reveal that while distinctions between (strategic and non-strategic) choice and luck have some effect, redistribution is influenced more strongly by initial income distributions.

The third chapter shifts focus from redistribution and fairness to coordination behaviour in pure coordination games. These games, where multiple equilibria exist with identical payoffs, pose the puzzle of how players succeed in coordinating without communication. Classical game theory predicts randomisation, yet experiments consistently find higher-than-random coordination, attributed to focal points (Schelling, 1960; Mehta et al., 1994*b*). A key unresolved question is what makes some labels more salient than others. Prior research highlights heuristics such as typicality or prominence (Hargreaves Heap et al., 2017), but it remains unclear whether long-term cultural familiarity or recent prominence better explains salience. Chapter 3 addresses this by systematically comparing three sources of label salience: frequency in books (reflecting long-term cultural exposure), online search trends (recent prominence), and specialised website rankings (domain-specific recognisability). In a 30-round experiment using labels from ten categories, coordination rates exceeded chance. Importantly, coordination success was significantly associated with book frequency, while online and website measures had little effect. This suggests that coordination relies on culturally entrenched shared knowledge rather than short-term popularity.

Together, these three studies contribute to the literature on inequality, fairness, and coordination in several ways. First, they show that individuals prioritise distributive outcomes over procedural fairness benchmarks such as equality of opportunity. Second, they demonstrate that in strategic contexts, redistributive behaviour is shaped less by abstract distinctions between choice and luck than initial distributions of outcomes. Third, they reveal that coordination is grounded in culturally shared knowledge, with long-term cultural salience playing a more decisive role than recent or domain-specific prominence. By integrating insights from experimental economics, political philosophy, and game theory, this thesis advances our understanding of how individuals evaluate fairness and contribute to coordination problems in both redistributive and strategic settings.

Chapter 1

Is Inequality Acceptable? An Experiment on Procedural Fairness

This paper investigates whether individuals' redistribution preferences are influenced by initial inequalities result from inequalities of opportunity, a key concept in debates on fairness. Drawing on the distinction between procedural fairness (focused on equal access to opportunities) and ex-post fairness (focused on the fairness of outcomes), we design a novel experiment to test whether individuals' preferences for redistribution change when opportunities are equal or unequal. We conducted the experiment within a new game theoretical framework based on Sugden and Wang (2020), in which participants engage in a card game with varying opportunities and then decide how much to redistribute in a dictator game. The results show that participants' redistribution preferences remain unchanged despite manipulation of equality of opportunity, suggesting that they prioritise outcomes over procedural fairness. This finding aligns with some research in the literature, arguing that most individuals focus more on outcomes than fairness in process. By exploring the relationship between procedural fairness and redistribution preferences, this paper contributes to the ongoing debate on the role of equality of opportunity in shaping perceptions of fairness in economic contexts.

1.1 Introduction

An important empirical question in debates on fairness is: Do people actually care about equality of opportunity? Do individuals perceive equality of opportunity as a fair procedural standard and, as a result, accept unequal outcomes generated by this procedure? This question lies at the heart of discussions surrounding inequality and fairness.

There has been a long-standing debate about the acceptability of inequality, particularly regarding which forms of inequality are more justifiable. Broadly, inequality can be divided into two categories: inequality of outcomes (such as disparities in income and wealth) and inequality in the process (such as inequality in access to essential resources by different social groups which is called inequality of opportunity). In economics, considering inequality of outcomes is more frequent.

One regular way of evaluating fairness in economics is to maximise a social welfare function with distributional weights, as in the literature about optimal taxation that balances efficiency with fairness (Mirrlees, 1971). In this framework, some inequality reduces social welfare and the government should optimally reduce it, subject to incentive constrains. However, the second approach is equality of opportunity which is a property of procedural fairness. Philosophically, the modern conception of equality of opportunity was introduced by Rawls (1958, 1971), who argues that fairness is not about equalising outcomes but ensuring equal opportunities.

According to this line of thought, attention should be directed at the rules of the system in which people have interactions rather than the results of that system. That is why Rawls advocates for policies aimed at levelling opportunities rather than incomes. In the same line, classical liberal economists like Hayek (1976) and Buchanan (1964) assert that the market should be evaluated as a system of rules. Our understanding from this thought is that if we want to apply fairness considerations to the market, this should be in the framework of procedural fairness, e.g. equality of opportunities, that consider rules of the procedures rather than outcomes.

Procedural fairness refers to the fairness of the process by which outcomes are determined, regardless of the final distribution of those outcomes. One key condition for procedural fairness is equal access to opportunities. We define equality of opportunity as follows: if person A's opportunities form a superset of person B's, then A has more opportunities. For example, if two players in a game each have two identical strategies and payoffs to choose from, their opportunities are equal. However, if one player has two strategies while the other one has those two strategies plus an additional one, the first player has fewer strategies, indicating inequality of opportunity.

This perspective studies the effect of equality of opportunity over equality of outcomes, making it an ex-ante fairness approach. Fairness judgments, therefore, should focus on equal access to opportunities rather than redistribution of income or wealth. In contrast, the ex-post fairness perspective evaluates fairness based on final distributions, such as income levels, and considers inequalities in outcomes inherently unfair or requiring correction.

Beyond these normative debates, there is empirical research on individuals' preferences for redistribution in relation to different fairness considerations. The research questions vary in different studies. Some aim to test preferences for equality in outcomes (e.g., Fehr and Schmidt, 1999) or to test the effect of some factors creating unequal outcomes like effort (or choice) versus luck (e.g., Cappelen et al., 2007). However, some others test procedural fairness (e.g., Bolton et al., 2005). This paper is in line with this branch of literature to test a factor in the procedural fairness view. This factor is equality of opportunities. The reason we choose this, is that one intuitive condition under which market outcomes may be perceived as fair is the presence of equal opportunities. If all individuals in a market environment have the same opportunities, they may be more likely to accept unequal outcomes (Sugden and Wang, 2020).

If this hypothesis holds, individuals who earn their outcomes in a market with unequal opportunities should show a greater preference for redistribution compared to those who earn their outcomes in a market with equal opportunities. To test this hypothesis, we design an experiment that examines individuals' acceptance of unequal outcomes arising from (in)equality of opportunity.

To explore how inequality of opportunity affects redistribution preferences, we design an experiment with two conditions: equal opportunity (control) and unequal opportunity (treatment). The outcome-generating process is competitive, resembling markets, ensuring that outcomes are always unequal, and the inequality level is the same in both treatments. After the outcome-generating phase, participants enter a redistribution phase, where their preferences for redistributing earnings can be observed. Since we aim to study the effect of equality of opportunity, we manipulate the sets of opportunities available to each participant. Specifically, participants engage in a two-stage process: first, they earn income through a simple card game, and second, they decide how to redistribute earnings in a dictator game. In the outcome-earning stage, two participants play a simple card game. In the fair version, both players receive three cards, while in the unfair version, one player receives three cards, and the other receives only two.

Previous studies on procedural fairness typically use randomisation or arbitrary selection to create fair or unfair scenarios. However, real-world discrimination often involves systematic biases based on group identity (e.g., gender, race, religion). To simulate such realistic discrimination in our experiment, we use elements from the minimal group paradigm (Tajfel et al., 1971, 1979; Chen and Li, 2009), categorising participants into groups based on trivial criteria. Participants experience both fair and unfair versions of a card game, where unfairness is generated by assigning different opportunities (cards) based on group membership. After each version of the game, participants engage in a redistribution phase where they decide how much of their earnings to share with their co-player. This design allows us to compare individuals' redistribution preferences in a fair rule card game with an unfair rule card game, shedding light on individuals' preferences for redistribution based on equality of opportunities.

The results indicate that subjects' redistribution preferences remain unchanged when equality of opportunities is manipulated. Specifically, they redistribute similar amounts of their income in both fair and unfair versions of the game. To confirm that participants understood the distinction between the fair and unfair versions of the card game, we included survey questions after the game to give their ratings of the fairness of the game (1 = completely unfair, 7 = completely fair). Participants gave an average rating of 2.74 to the unfair rule game and 5.63 to the fair rule games, showing that they clearly differentiated between the two versions. These findings suggest that subjects are not influenced by changes in equality of opportunity; instead, they appear to prioritize outcome fairness over procedural fairness.

Our contribution in this paper lies in shedding light on an important debate in economics regarding individuals' redistribution preferences, specifically whether they are shaped by a view of fairness rooted in procedural fairness (focused on equality of opportunity in the outcome-generating process) or in outcomes, regardless of the procedure. We approached this question by defining equality of opportunity within a game-theoretical framework and designing a novel experiment that clearly tests individuals' redistribution preferences in response to manipulated opportunities. Our within-subject design, where only one variable (equality of opportunity) was changed while all other variables remained constant, allowed us to make clear comparisons and contribute to this literature.

The following content of the chapter is arranged as follows: In Section 1.2, we will introduce the seminal studies related to our research questions. In Section 1.3 the details of our experimental design are explained. Our hypotheses will be specified in Section 1.5. Section 1.6 reports data analysis and explain the results related to the hypotheses. Finally, Section 1.7 is the conclusion.

1.2 Literature Review

The fairness concept has a long history in economics, especially experimental/behavioural economics (Hoffman and Spitzer, 1985; Bolton, 1991; Rabin, 1993; Hoffman et al., 1994; Camerer and Thaler, 1995). In general, there are two branches of literature in experimental economics related to fairness: Ex-post fairness and ex-ante fairness. The first one is related to distributive justice or outcome fairness in which individuals decide the redistribution of incomes after an initial distribution has been determined. The research question in this line of research includes preferences for redistribution or equality of outcomes. One of the foremost studies in this branch of literature is the inequality aversion model (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) in which each individual is averse to inequality between themself and the other. In the ultimatum game, which is one of the games played in these studies, the proposer sends an offer to the recipient who can accept the distribution of money, which leads to earning the amount of money determined by the proposer's suggestion, or rejection which leads to both participants earning nothing for the experiment. According to the model, individuals derive utility from their own payoffs and disutility stemming from differences between their payoffs and their co-players' payoffs, so they make a trade-off between their own payoffs and equality.

In this model, the factors making inequality are neglected. Recent studies, in this branch of research, have been trying to study the factors causing inequality and showing that people's behaviours are different if inequality results from different factors. Researchers add a stage where subjects can earn money before the redistribution stage to study the effects of effort and luck on the redistribution of incomes (Konow, 2000; Fong, 2007; Gee et al., 2017). In these papers, the findings suggest that if the inequality stems from luck, people have more inclination to redistribute the total income of their groups, but if the inequality is the result of their efforts, they think that they deserve what they earn, leading them to have less desire for equitable earnings. But, Cappelen et al. (2007, 2010) focus on more factors, such as the level of effort (the amount of time they choose to play), ability (their ability to give correct answers at the determined time interval) and luck (the price for each correct answer which is randomly assigned to them) which are representatives of choice, productivity, and chance, respectively. What distinguishes these studies from other pieces of research in the literature is that their design allows them to study people's preferences for responding to different aspects of effort, which are based on people's choice (effort level) or beyond their choices (productivity, which is based on background, education etc.), and compare them with luck separately (price is a random exogenous variable).

Considering these factors, Cappelen et al. (2007, 2010, 2013) can distinguish subjects who show preferences for egalitarian fairness from liberal, meritocratic, and choice egalitarian fairness. The common elements in the designs related to this branch of literature are that people earn money based on some factors, and then one in each group has the role of a dictator who should decide how to redistribute the total income of participants in the group. In fact, the fairness views in this paper and other pieces in this line of research pertain to distributive justice or outcome fairness which is a property of the ex-post fairness view. Our research is related to this branch of literature because our research question is related to the preferences for redistribution which is related to outcomes. However, the factor we want to study (procedural fairness) is not usually studied in this line of research.

Another strand of research focuses on ex-ante fairness in which individuals compare their ex-ante situations, not their final outcomes. Research questions pertaining to this line of research focus on procedures. Some scholars focus on the expected outcomes of the procedures, and some others on the procedures per se, no matter what the distribution of outcomes or expected outcomes is. There are several approaches in this line of research. Firstly, there are some studies related to expected outcomes in which there is a trade-off between individuals' own expected outcome and disutility from differences between their own and others' expected outcomes, so individuals compare expected outcomes to evaluate the fairness of an allocation procedure involving uncertain outcomes (Trautmann, 2009; Krawczyk, 2011; Saito, 2013). Their idea of procedural fairness is the equality of individuals' expected outcomes. In fact, they think that equality of opportunities (a property of procedural fairness) means having procedures that can create the same expected outcomes or the same chance of winning. Moreover, other studies in this line of research rely on fairness as randomness (Blount, 1995; Bolton et al., 2005; Sebald, 2010; Chlaß et al., 2019). They compare the results of biased procedures (procedures in which the results are in favour of some and against the other) with random procedures, understood as fair procedures in which there is no bias. To make it clearer, two papers related to fairness as randomness and expected outcomes will be explained in more detail as follows.

Bolton et al. (2005) explore procedural fairness through ultimatum-style games with different mechanisms for proposal generation, focusing explicitly on whether procedures impact the acceptance of unequal outcomes. They contrast games in which procedures are inherently fair (unbiased random draws) against those that are unfair (biased draws or human proposers). Their findings reveal that participants are significantly more accepting of unequal outcomes when generated through unbiased, fair procedures compared to biased, unfair ones. This closely relates to this paper, as both studies investigate procedural fairness—though Bolton et al. (Bolton et al., 2005) emphasise randomness in procedure, whereas this experiment directly manipulates equality of opportunity.

Chlaß et al. (2019) investigate preferences for procedural fairness by designing mini-games (dictator, ultimatum, and yes-no games) that vary only in procedural rights rather than in expected outcomes. Participants were willing to pay to secure fair procedures even without clear material advantages, indicating intrinsic preferences for fair processes independent of outcomes. Their results suggest individuals value fairness in decision-making rights, which is in line with this study of whether participants prioritise fairness of opportunities (procedures) or distributional outcomes. While their study emphasises decision rights, this paper specifically isolates equality of opportunities within competitive outcome-generation processes.

In relation to ex-ante fairness, the mentioned studies do not explicitly focus on the route of individuals' earnings. Instead, they concentrate on either individuals' expected outcomes of the procedures, or compare random draws (considered fair) to biased procedures (considered unfair). In fact, they focus on the procedure of redistribution or allocation and their definition of equal opportunity is related to the redistribution procedure. Although their ideas of procedural fairness have provided useful insights to the literature, they have some limitations in terms of procedural fairness. However, there is a paper in this line of research that considers the route of earnings. Cappelen et al. (2024) investigate how individuals evaluate income inequality that stems from unequal opportunities using a large-scale experimental study conducted in the United States and Scandinavia. In the experiment, participants completed real-effort tasks under either advantaged or disadvantaged conditions, where the advantaged group earned more per task. Importantly, all participants were matched to control for inherent productivity, ensuring that the income inequality was driven solely by differences in opportunity. Spectators (uninvolved third parties) were then asked to make real redistribution decisions between paired workers. The focus of this paper is spectators who are not engaged in the outcome-generating procedures, instead show their preferences for redistribution for two other active players who are engaged in the procedure. Additionally, the way they manipulate inequality of opportunities is based on randomness. So, their research question and design are different from the aim of this paper.

The same as in Cappelen et al. (2024)'s paper, our definition of procedural fairness is equality of opportunities in the outcome-generating procedure. However, the difference is that our definition of equality of opportunities is neither necessarily the same expected outcome in the distributional stage nor based on randomness. In addition to their definition of fair procedures, the way these studies make unfair procedures is not based on strong justification. In the studies, there are biases against some players and in favour of others, but there is no reason behind the biases. Subjects are randomly assigned to these biases which cannot cover many situations in which individuals are discriminated systemically. This may lead participants not to perceive the unfairness of the procedure.

There is an alternative conceptual approach emerging in the literature, which specifically considers fairness related to equal opportunities during the outcome-generating stage. A key experimental study illustrating this perspective is Sugden and Wang (2020). They aim to answer the question of whether markets are fair. The authors see a market as an example of a Hayekian procedure: “a rule-governed procedure in which interactions between individuals can produce consequences that were not deliberately chosen by any individual participant but are not merely the result of chance” (Sugden and Wang, 2020). They design fair and unfair competition games and real-effort tasks before a vendetta game in which subjects show their feelings about the results stemming from the games. In the result of the first part (competition game or real-effort task), the two players in each pair are assigned to the roles of winners and losers depending on their performance in the competition games. Winners take 9 and losers take 3 lottery tickets. Then, they play a vendetta game in the second part. The two players in each pair start with these unequal shares (9 vs. 3). Taking turns (loser first), each player can choose to take blocks of three tickets from their opponent. For each block taken, one ticket is gained and two are destroyed. Players may also choose to take no tickets. The game ends if either both players have fewer than three tickets, or a player who could still take tickets chooses not to do so on two consecutive turns. The game is played in real time to allow emotional responses to influence decisions.

Their results show that subjects tend to accept results from fair procedures rather than unfair procedures, which is in line with other research in the literature, but what is important in their design is to introduce fair procedure as an equal opportunity procedure in a strategic situation, so they use strategic fairness instead of the concept of fairness as randomness. They add, moreover, a procedure in which subjects earn money (competition game). This procedure did not exist in experimental papers related to ex-ante fairness. It seems the paper is the first one in ex-ante fairness research in experimental economics that distinguishes procedural fairness from fairness as randomness.

However, there are some problems with the design. Firstly, there are no clear relationships between effort and ability with procedural fairness, so it is not clear whether subjects’ behaviours are based on the fairness or unfairness of procedures or their ability and efforts. So, a good design to compare procedural fairness with unfairness should be in a way that can control talent and skill, i.e., there is not any skill or ability in the design. Secondly, in the vendetta game, consecutive actions, after the first one, are not necessarily based on procedural fairness or unfairness, they may be more related to the other player's previous choice of taking tickets (feeling that my tickets are stolen, and they want to reciprocate their opponent's behaviour no matter what was the procedure). Finally, one element of their design which is not a problem per se but has little external validity, is related to the way they manipulate inequality of opportunities. In their design, subjects deliberately select a seat, and they are not informed that their choice has any effect on the design. After choosing their seats, they are informed that based on their seat numbers (even or odd), they will be divided into two groups A and B. Then, each player from group A has 1 card while players from group B have 3 cards to play. So, the allocation of cards is based on their seat numbers. So, they rely on arbitrary rules for making different allocations of cards, but a sizable part of observed income inequality can be attributed to inequality among social groups (United Nations and Affairs, 2020).

Although the definition of procedural fairness by Sugden and Wang (2020) is close to our definition, their research question is different from our study. Their research question is eliciting individuals' feelings about procedural fairness. The players' earnings in the card game are lottery tickets, and these are the objects that can be taken in the Vendetta game. The Vendetta game seems a good game for eliciting individuals' feelings of resentment, but the game is not clear about attributing participants' anger to specific participants or the specific procedure. So, this is not a clear way to elicit individuals' preferences for redistribution based on procedural fairness. Since our study aims to elicit individuals' preferences for equality of outcomes based on procedural fairness, we design a clearer experiment to address this research question by adding a dictator game after the earning procedure.

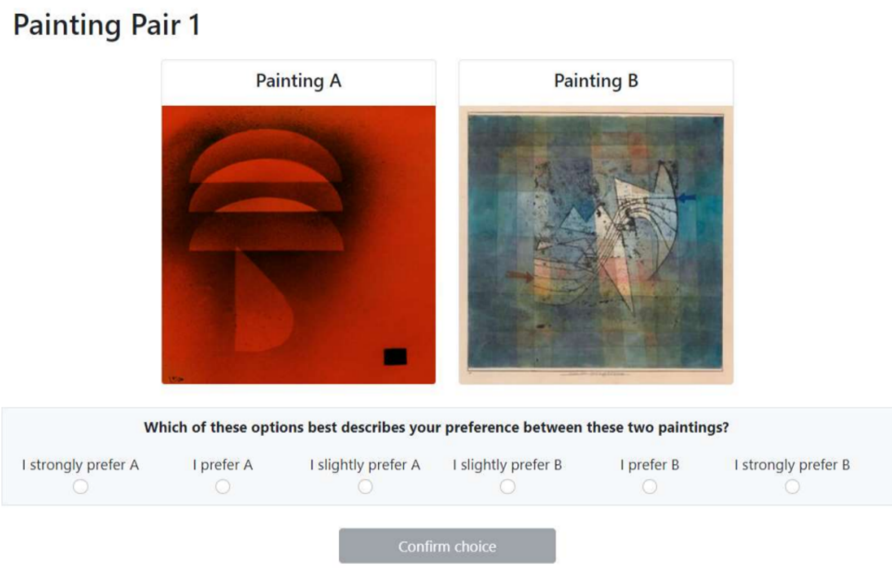
1.3 Experimental design

In the experiment, there is an outcome-generating procedure before redistribution to study the effects of equality of opportunities on individuals' preferences for equality of outcomes. The way we manipulate opportunities in different treatments is based on group discrimination which can augment the external validity of the experiment. We also suggest a version of a competition game in which the level of individuals' ability is the same and they need only a basic ability (distinguishing higher numbers from lower numbers) to participate in the game. To do so, we can control people's abilities, so subjects' earnings depend on only the (un)fairness of the procedure. We also suggest a costly transfer stage as a redistribution phase in which people only show their preferences for equality of outcomes based on their earnings which are based on (un)fairness of the procedure. Our experiment consists of three stages, stage 1 for making groups based on the minimal group paradigm, stage 2 with a series of card games, and stage 3 with a costly taking procedure. Moreover, there is a final step for giving subjects their earnings. Before starting the experiments, subjects are informed that they will play several games, and they are given instructions for one experiment at a time.

1.3.1 Stage 1: Making groups

In this stage, subjects are divided into two groups based on their preferences for several paintings. The procedure to do it is a part of the minimal group paradigm stemming from Chen and Li (2009). Each subject is given 5 pairs of paintings (one page for each pair of paintings) belonging to Klee or Kandinsky. Subjects are not told which one belongs to which painter and they do not even know that the tasks involve a fixed pair of painters. They are asked to choose the option that best describes their preferences between the two paintings in each pair. There are 6 options on each page from I strongly prefer A to I strongly prefer B. We consider participants' preferences for Klee's and Kandinsky's paintings and rank them based on their preferences for Klee's paintings. The first half of the participants with higher preferences for Klee is Klee's preferred group and the second half is Kandinsky's preferred group.

Figure 1.1: Showing Preferences for Paintings by Subjects



After all participants in a session finish choosing options based on their preferences, they are divided into the Klee-preferred group and Kandinsky-preferred group. Subjects do not know about the painters of the paintings; they are only informed whether they are in the Klee's preferred group or the Kandinsky's preferred group. Then, we want them to be engaged in some activities to intensify ingroup feelings. They are given some information about the style of Klee's (Kandinsky's) paintings if they are in the Klee (Kandinsky) group. Simultaneously, they are given a new pair of paintings, one by Klee and one by Kandinsky, and learn which one belongs to Klee (Kandinsky). Then, they are informed that they will be shown 2 new sets of paintings. In each set, there are five paintings belonging to 5 different painters (one belongs to Klee/ Kandinsky and the other 4 belong to others). They should distinguish Klee paintings if they are in the Klee's preferred group or Kandinsky paintings if they are in the Kandinsky's preferred group. They have the chance to chat with others in their group for one minute for every set of paintings. In each group and each set, one person is selected randomly as a spokesperson to summarize the chat and to select the correct option. If a group's answer for each round is correct, all members of the group are given 5 points[1], so each person has the chance of earning 10 points in total. However, they are not informed about their earnings until the end of the game. This is because there is a probability that a group answers all questions wrongly which leads to inhibiting the group feeling which can be different in different groups. This can cause experimenters to lose control of the variables.

1.3.2 Stage 2: Earning procedure: the fair and unfair higher card game

Many real-world institutions involve competition and create inequality; the market is one example. Competition is not necessarily based on skill or effort. Therefore, the outcome-generating procedure should be competitive and induce inequality of outcomes. In addition, it should be in a way that we can control the level of effort and abilities and the only thing that can make a difference in earnings should be the rules of the game. The game that we think is a good fit with these situations is a simple card game that will be explained as follows.

In this stage, subjects are informed that they should play a series of card games. Each person from Klee's preferred group should play a card game with a person in Kandinsky's preferred group. Within group selection is based on randomisation. There are two roles in each game (role A and role B). In our experiment, participants are first asked to indicate their preference between two abstract paintings, one by Klee

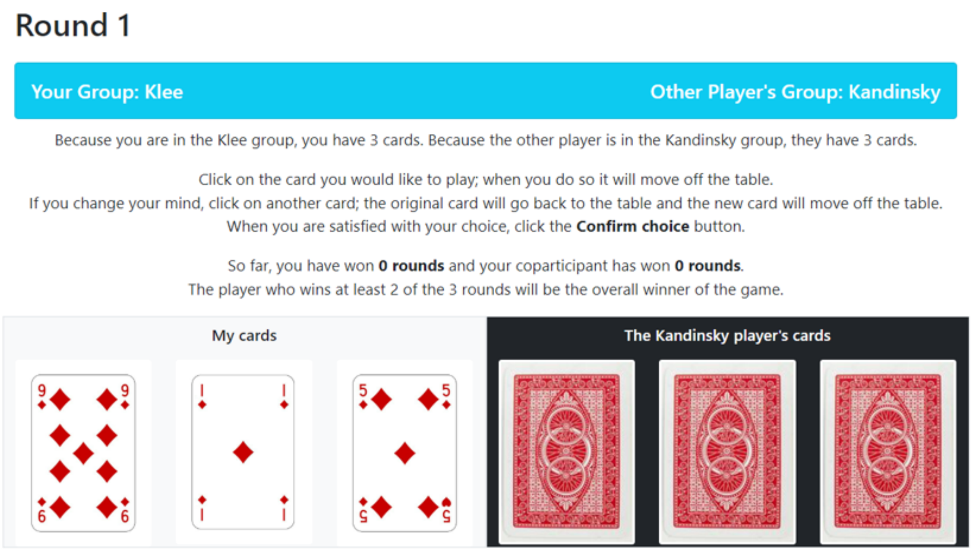
[1]We use a currency called points which has a value of 0.2 pounds, which means for any 5 points they are given 1 pound.

and one by Kandinsky. Based on these preferences, they are assigned to one of the Klee- and Kandinsky-preferred groups. Across sessions, the labels for Groups A and B are randomly linked to these two groups. For consistency in analysis, we refer to the group associated with a preference for Klee as Group A and the group associated with a preference for Kandinsky as Group B, regardless of their actual label in a given session.

Subjects are informed about the first series of card games. Here, we assume that subjects start playing fair rule card games. So firstly, we explain fair rule card games.

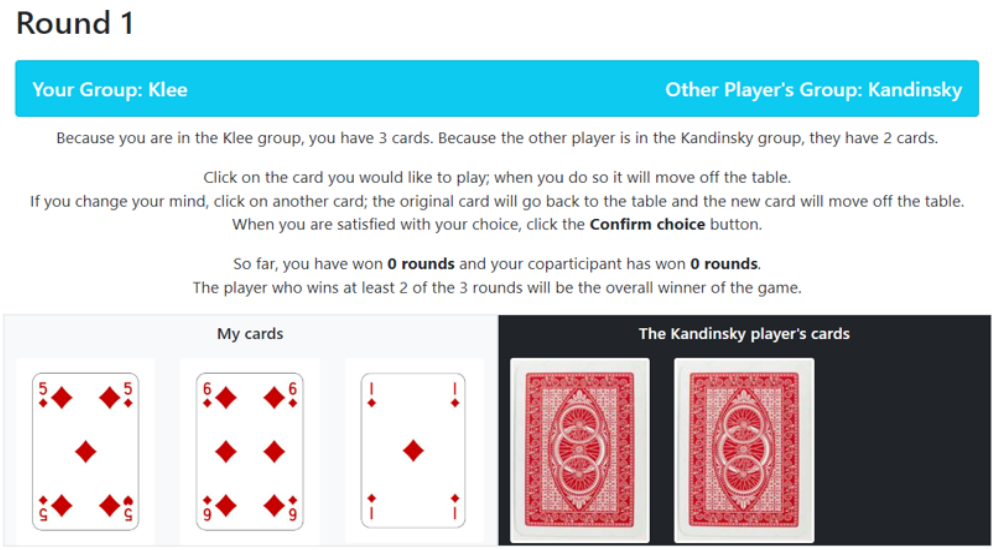
Fair Rule Card Game: Each person in group A is matched randomly with a person in group B to play the first series of card games, in which they play card games three times. In each round, players A and B are given 3 cards with a number on the face of each card. Each player can see the numbers of their own cards, but not for their co-players. In each round, each player selects a card among their 3 cards. The winner of the round is the player who plays the card with the higher number. The overall winner is the person who wins at least two rounds of three rounds. The overall winner is given 90 points, and the loser is given 30 points (each point is worth £0.20). The numbers on the cards are between 1 to 10. The deck consists of 10 unique cards numbered from 1 to 10, with each number appearing only once. Importantly, each pair of players shares a single deck, so no card can be given to both players. In each round, six cards are randomly drawn from the shared deck (three for Player A and three for Player B), ensuring that the two players receive different cards.

Figure 1.2: A Card Game in Fair Rule Games



Unfair Rule Card Game: The unfair rule game is the same, but the number of cards given to one of the players is different. In the unfair rule card game, player A is given 2 cards while player B is given 3 cards.

Figure 1.3: A Card Game in Unfair Rule Games



In each session, the order of the fair and unfair games is selected randomly. So, some subjects play the fair rule game first and then the unfair rule game, and others vice versa. Subjects for each version of the card game are randomly selected in a way that ensures that their co-players are different in different versions of the games.

1.3.3 Stage 3: Costly transfer procedure

In this stage, subjects are informed of their earnings, which can be 90 or 30 points as winners or losers, respectively. Then, they should decide how many points of their co-players' earnings they want to take or how many points from themselves they want to give to their co-players, but they are informed that half of the amount taken/given goes for earnings, and half of it is lost. For example, a loser, who earns 30 points and her coplayer earns 90 points, can have the option to take 0 to 90 points from her coplayer or give them 0 to 30 points to him. If she chooses 20 points to take, her points increase by 10 and become 40, while her co-player loses 20 points and his earnings become 70 points. If she wants to give her coplayer 10 points, her earnings decrease by 10 points, but her coplayer's earnings increase by 5 points. In each match, there are two proposals, but only one of them is randomly selected and kept until the end of the experiment. After each card game, there is a transfer procedure in which participants should choose to take or give, or neither, and then, if they choose taking or giving, then a slide shows up and they should choose how many points they want to take or give. One specific person, for example, plays a fair rule card game with a transfer procedure and then an unfair rule card game with a transfer procedure.

After the first transferring procedure, subjects are not informed of the results until the end of the experiment. So, after the first transferring procedure, subjects are again randomly assigned and then play another version of the card game, followed by a new transferring procedure. After the second transfer, subjects are informed to wait until all players finish their procedures, and then all subjects in the session go for the final stage.

After the mentioned 3 stages and a following survey, subjects are informed of all earnings in all stages of the experiment. The total earnings of subjects are the number of points they earn from two questions in the first stage (maximum 10 points total) plus their earning after the transfer procedures in two games. It should be noted that each subject is faced with two versions of card games and two transferring procedures. One of the redistribution proposals of the two players is randomly selected for payments. These prizes plus the show-up fee (£3) are the total earnings of each person. At the end of the experiment, there is a survey asking some questions related to the subject's socio-economic background. This survey aims to address the question of whether subjects' preferences for equality based on procedural (un)fairness are affected by their socio-economic variables like gender, race, etc. After the survey, they are informed of their earnings and then should go to the experimenter and take the money.

Overall, our experimental design includes three stages. First, making groups based on the minimal group paradigm (Klee-Kandinsky's preferred group) happens in stage 1. Then, subjects (each subject from Klee's preferred group with each subject from Kandinsky's preferred group) play two versions of the card game (stage 2), each one followed by a transferring procedure (stage 3).

1.3.4 Checking participants' perception of the procedural fairness

At the end of the experiment, participants complete a survey designed to assess their perceptions of fairness in the two versions of the card game. Specifically, we asked how fair they found the game in which players received an unequal number of cards (unfair rule) compared to the game in which both players received an equal number of cards (fair rule). Participants rated each version on a 7-point scale, where 1 indicated "completely unfair" and 7 indicated "completely fair.

1.3.5 Experimental procedure

We conducted 21 sessions, each consisting of 8 participants, resulting in a total of 168 participants. During these sessions, each individual played two versions of the card game and made transfer decisions accordingly. As a result, we obtained 336 observed data points, representing the transfer choices made by the participants after playing each version of the game.

Out of the 336 observations, 168 were related to transfers after playing the fair rule games, while the remaining 168 were associated with transfers after playing the unfair rule games. The 168 observations from the unfair games were further divided, with 84 observations for advantaged players (i.e., players who had been dealt 3 cards) and 84 for disadvantaged players (i.e., players who had been dealt 2 cards).

Since we are conducting a difference analysis for the winners and losers of the card game, it is essential to distinguish between these two categories. We have a total of 168 losers and 168 winners in our dataset. Among the losers, 84 participants played the fair rule games (referred to as Fair Losers - FL), 57 were disadvantaged players (Disadvantaged Losers - DL), and 27 were advantaged players (Advantaged Losers - AL).

As for the winners, we also have 168 participants, with 84 winners from the fair rule games (Fair Winners - FW), 27 winners from disadvantaged players (Disadvantaged Winners - DW), and 57 winners from advantaged players (Advantaged Winners - AW).

1.4 Formal definition of inequality of opportunities

In this section, we introduce our formal definition of inequality of opportunities.

In the literature, there are some seminal experimental economics papers related to procedural (un)fairness. Sugden and Wang (2020) introduce inequality of opportunities in a strategic way. Instead of comparing procedurally fair interactions with randomness as suggested in the literature, they compare them with procedurally biased interactions in strategic situations. Based on their definition, symmetric games (identical strategies with identical outcomes) produce equality of opportunities. For example, a battle of the sexes is a fair game in this definition. Starting from a fair game, if the game is revised by adding strategies for one player, and if playing any of those strategies can create an inequality in favour of that player, the revised game is defined to be biased towards that player, no matter whether the extra strategies are dominated or not, despite Chlaß et al. (2019).

Our definition is the same as Sugden and Wang (2020)'s definition. So, our definition is based on the expected outcomes in strategic situations. In our experiment, once one player chooses her card, she does not know whether playing that card leads her to win or not. So, she has an expected outcome, which is based on the probability of winning.

In our game, there is a clear dominant strategy that most players play (playing the highest card in our experiment). While reaching Nash equilibrium is most likely to happen, it is not necessary that all players play this strategy. One might say that it is completely clear that a rational player should play the dominant strategy, which is playing the highest card here, but it is not a plausible assumption that all players make rational choices. So, our definition of opportunities is based on new strategies that benefit players without considering if they are non-dominated (Sugden and Wang, 2020). Games with equal opportunity are those that have identical strategies and expected outcomes. Procedurally biased games, in our definition, are those that have a bias towards one player. It is worth mentioning that a game can be procedurally neither fair nor biased.

1.4.1 Fairness and bias in the card game

In the following, we will describe formally fair and biased games.

Case 1 (Fair games): Each player is dealt two cards in order from a single deck $D = \{1, \dots, 10\}$. Each player i has a deal $d_i = (d_{i,1}, d_{i,2}, d_{i,3})$. (Note that subscripts 1, 2 and 3 refer to the order in which the two cards were dealt to i.)

The set of all possible deals for i (D_i) is the set of all three-element subsets of D. A strategy for i is a function of d_i ($s_i(d_i)$) which, for each deal $d_i \in D_i$, selects one of the cards in that deal. The player who chooses the higher card gets a payoff of 1. For each strategy profile (i.e., one strategy chosen by each player), the payoff function selects an expected payoff profile (i.e., one expected payoff for each player). (E.g., if each player's strategy is 'choose the highest card', the payoff profile is (0.5, 0.5).)

It is immediately obvious that the game is isomorphic with respect to the players and therefore fair (according to the (Sugden and Wang, 2020) criterion).

Case 2 (biased game): Player i is dealt two cards in order (d_i), player j is dealt 3 cards in order (d_j). The set of all possible deals for i (D_i) is the set of all two-element subsets of D. The set of all possible deals for j (D_j) is the set of all three-element subsets of D.

Consider the set of strategies for player j. A strict subset of these strategies can be defined by $s_j(d_{j,1}, d_{j,2}, d_{j,3}) = s'_i(d_{j,1}, d_{j,2})$, i.e., j ignores the third card, he always chooses either card 1 or card 2, and that choice is independent of the value of the third card. Clearly, a game in which player j was required to choose from this strict subset would be equivalent to a 2 by 2 isomorphic game. For the same reasoning that case 1 is fair, this 2 by 2 isomorphic game is fair. So, Case 2 differs from Case 1 by having additional strategies for j.

One strategy for i (and an optimal strategy) is 'choose the higher card'. Call this S_i^*.

One strategy for j (and in fact an optimal strategy) is 'choose the highest card'.

Call this S_j^*.

It is obvious that if this strategy combination is played, j's expected payoff is higher than i's. So, by the Sugden and Wang (2020) criterion, the game is biased in favour of j.

1.5 Hypothesis

This design allows us to study people's preferences for redistribution based on procedural fairness. In the design, the only variable changed is the rule of the card game; one game has a fair rule, which means that each player in every pair has 3 cards, while the other game is an unfair rule game in which one player has 3 cards (advantaged player) and another one has 2 cards. All other things remain unchanged; overall winners who win at least 2 rounds of the game over 3 rounds earn 90 points while losers earn 30 points, which means the inequality is fixed in all situations. Then there is a redistribution after each game in which all individuals can propose their proposals about taking from or giving to their co-players. The hypotheses of our experiment are about participants' proposals in the redistribution procedure. What is important in this research is a comparison of redistribution after a fair rule card game (control group) and after an unfair rule card game (treatment group).

Half the proposals are from fair rule games and another half from unfair rule games. Our investigation is about comparing participants' transfers in fair rule games and unfair rule games. Since the feasible redistribution options are different for losers and winners (losers can choose to take up to 90 points from their co-players and give them up to 30, while winners can take up to 30 points from their co-players and can give them up to 90 points), our comparisons for losers are separated from winners.

Before starting the explanation of hypotheses, we should define our measure of redistribution. We define the transfer made by a player as follows. If the player gives, their transfer is the decrease in their payoff, recorded as a negative number. If the player takes, their transfer is the decrease in the payoff of the other player, recorded as a positive number. So the range of possible transfers is $[-30, 90]$ for losers and $[-90, 30]$ for winners.

Hence, we put forward two hypotheses as follows.

H1: The transfer by losers in unfair rule games is different from that of losers in fair rule games.

If our claim about procedural fairness and acceptance of the fair procedure is correct, then we expect losers in the fair rule game to transfer different amounts compared to the unfair rule game. This hypothesis has two sub-hypotheses. Firstly, the expectation is that participants who lose the games while having 2 cards versus their co-players having 3 cards (disadvantaged losers) want to transfer more points to themselves (take more or give less) compared with winners in the fair rule game, since they have had fewer opportunities. Secondly, winners who have 3 cards while their co-players have 2 cards (advantaged winners) want to transfer fewer points to themselves (taking less or giving more) compared with winners in the fair rule game.

This hypothesis is divided into two hypotheses:

- **H1a:** Disadvantaged losers in unfair rule games transfer more points to themselves than losers in fair rule games.
- **H1b:** Advantaged losers in unfair rule games transfer fewer points to themselves than losers in fair rule games.

In line with hypothesis H1, hypothesis H2 applies similar principles for winners.

H2: The transfer by winners in unfair rule games is different from that of winners in fair rule games.

This is divided into two sub-hypotheses as follows. Firstly, winners who have 2 cards versus their co-players having 3 cards (disadvantaged winners) want to transfer more points to themselves (take more or give less) compared with winners in the fair rule game, since they have had fewer opportunities. Secondly, winners who have 3 cards while their co-players have 2 cards (advantaged winners) want to transfer fewer points to themselves (taking less or giving more) compared with winners in the fair rule game.

- **H2a:** Disadvantaged winners in unfair rule games transfer more points to themselves than winners in fair rule games.
- **H2b:** Advantaged winners in unfair rule games transfer fewer points to themselves than winners in fair rule games.

1.6 Results

In this section, we will analyse the data from the experiment and test hypotheses H1 and H2.

1.6.1 Perceived fairness across games

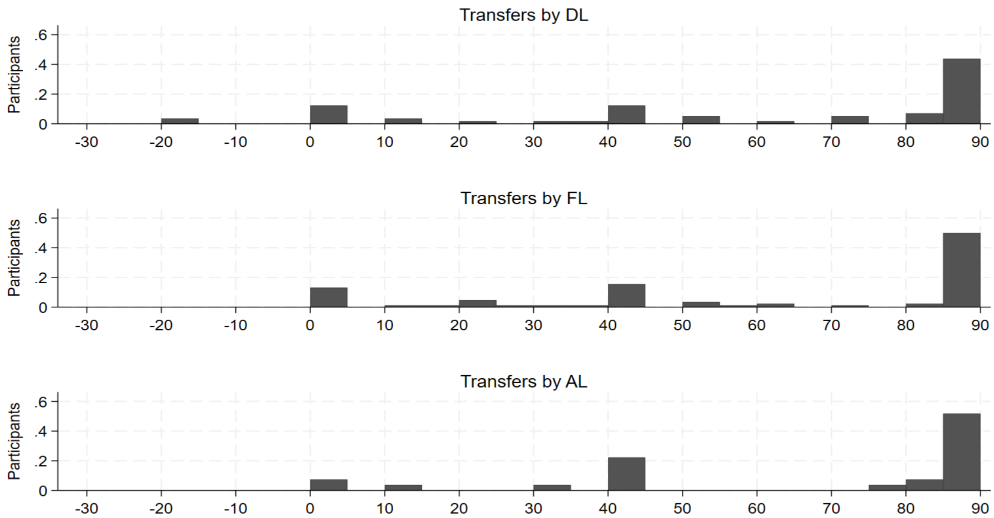
In the survey following the experiment, we asked subjects' fairness view about the two versions of the game: The game in which participant and their coplayer had different number of cards (unfair rule game) and the game in which they had the same number of cards (fair rule game). The range is between 1 to 7: 1 for completely unfair and 7 for completely fair. The average of participants' rate was 2.74 for unfair rule game and 5.63 for fair rule game which shows participants clearly understood the unfairness of the unfair rule game and distinguished it from the fair rule game[2]. These two averages are statistically different from each other (p-value: 0.0000).

1.6.2 Statistics and non-parametric tests

Our primary interest lies in comparing the transfer decisions of Fair Losers with those of Disadvantaged Losers, as well as with those of Advantaged Losers. Similarly, we wish to compare the transfer decisions of Fair Winners with those of Disadvantaged Winners and Advantaged Winners. Upon analysing the dataset, we did not find any statistically significant differences between the means of transfers for Fair Losers and Disadvantaged Losers, nor between Fair Losers and Advantaged Losers. The same applies to the comparisons for winners.

To gain a deeper understanding of the distribution of these variables, we can visualize their respective distributions in figures as follows.

Figure 1.4: Distribution of Transfers by Losers



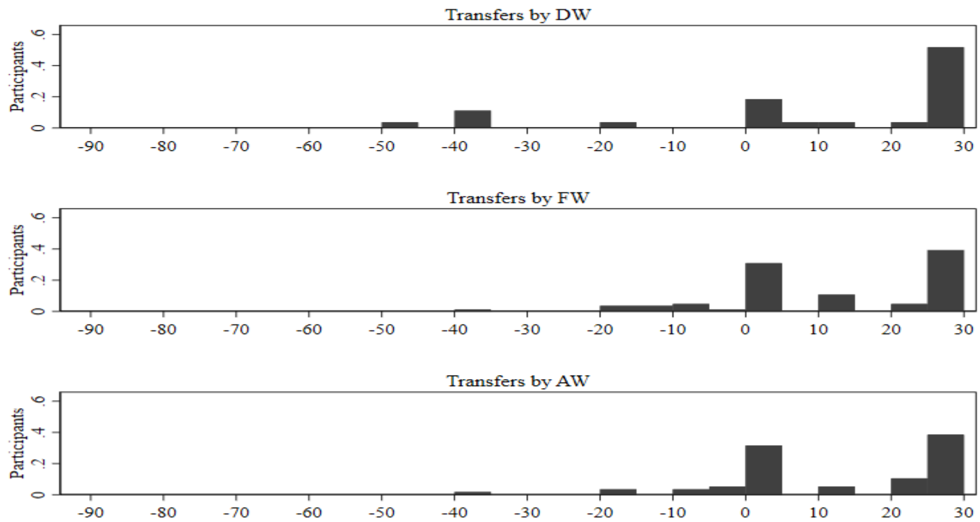
Note: TreatDL: disadvantaged losers. TreatFL: fair losers. TreatAL: advantaged losers.

In figure 1.4, the distribution of transfers by losers can be observed. We can observe that the patterns of transfers made by losers are consistent across different situations. There is a notable spike in participants choosing to take 90 points, which represents the maximum number of points they can take. The percentage of participants opting to

[2]You can see the screenshot of these survey questions in figure 1.A.1 and figure 1.A.2 in the appendix 1.A.

take all points from their co-players varies between 43% to 48% in different scenarios. Another significant spike occurs in participants choosing to take 40 points, indicating a desire to equalize their earnings, aligning with an egalitarian perspective. A third spike is observed in participants choosing to take or give nothing, implying their contentment with their current situations. The percentage of participants choosing to give points to others remains consistently lower than 13% across all situations.

Figure 1.5: Distribution of Transfers by Winners



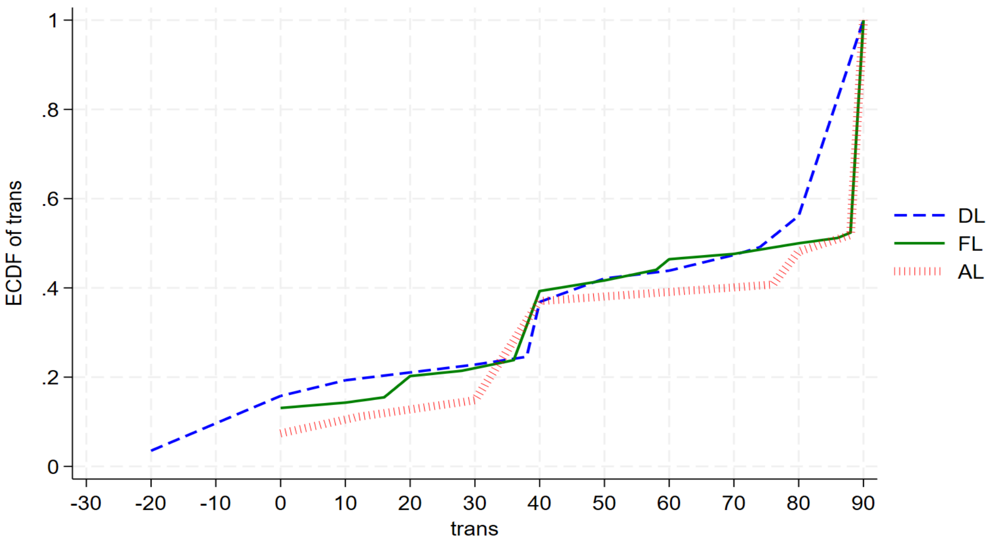
Note: DW: disadvantaged winners. FW: fair winners. AW: advantaged winners.

In figure 1.5, the distribution of transfers by winners can be observed. A similar pattern emerges in the redistribution behaviours of winners. More than 38% of winners express a preference to take all earnings from their co-players (i.e., choose a transfer of 30). In scenarios involving fair games and transfers of advantaged winners, approximately 30% of winners are content with their results and choose to take or give nothing, while only 18.5% of disadvantaged winners exhibit the same behaviour. The percentage of winners willing to give points to others remains relatively low, comprising less than 10% in the cases of advantaged and fair winners, and less than 20% in the case of disadvantaged winners.

The data presented in figures 1.6 and 1.7 show that the patterns for all situations concerning losers and winners are similar, except for disadvantaged winners, where there are slight differences.

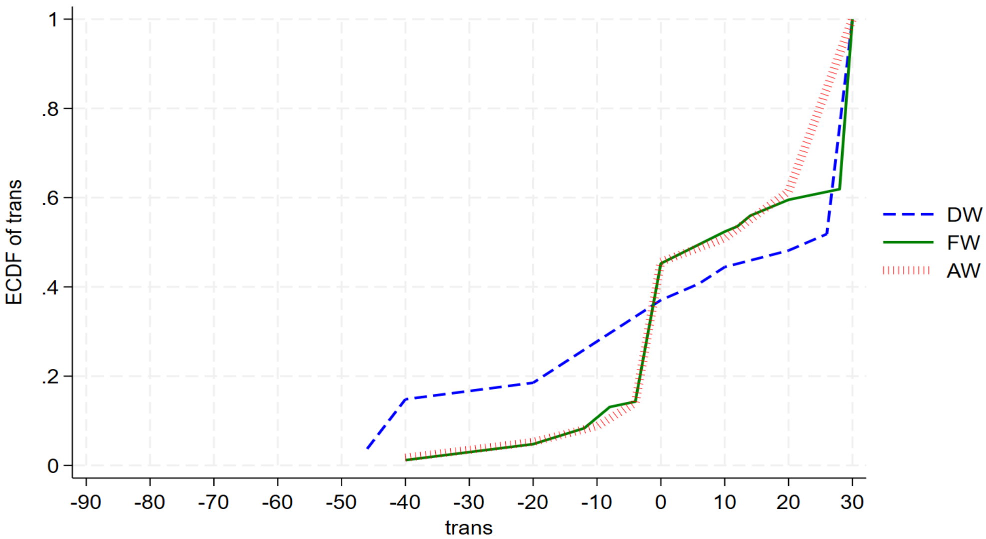
To compare the samples related to the hypotheses, non-parametric tests were employed, as they do not rely on assumptions about the distribution of the data (for example, normality or linearity) (Corder and Foreman, 2009). The Wilcoxon Mann-Whitney (rank-sum test) test was used to compare losers in fair rule games (FL) with advantaged losers (AL) and losers in fair rule games (FL) with

Figure 1.6: Cumulative Distribution of Transfers by Losers



Note: TreatDL: disadvantaged losers. TreatFL: fair losers. TreatAL: advantaged losers.

Figure 1.7: Cumulative Distribution of Transfers by winners



Note: TreatDW: disadvantaged winners. TreatFW: fair winners. TreatAW: advantaged winners.

disadvantaged losers (DL) for hypotheses H1a and H1b, respectively. The tests show that transfers by fair losers (FL) are slightly lower than those by disadvantaged losers (DL), although the difference is not statistically significant (z = -0.315, p = 0.7532). Similarly, transfers by fair losers (FL) are slightly higher than those by advantaged losers (AL), but this difference is also not statistically significant (z = 0.464, p = 0.6494).

The same tests were used for H2a and H2b, which compare winners in fair rule games (FW) with advantaged winners (AW) and winners in fair rule games (FW) with disadvantaged winners (DW). The tests show that transfers to fair winners (FW) are higher than those to disadvantaged winners (DW), as indicated by the mean values (11.86 vs. 9.85), although this difference is not statistically significant ($z = 0.414$, $p = 0.6789$). In contrast, transfers to fair winners (FW) are slightly lower than those to advantaged winners (AW), but again, the difference is not statistically significant ($z = 0.097$, $p = 0.9229$).

Note that the number of observations varies across the treatment groups: there are 84 observations for fair losers (FL), 57 for disadvantaged losers (DL), and 27 for advantaged losers (AL). Similarly, for the winner conditions, the sample sizes are 84 for fair winners (FW), 57 for disadvantaged winners (DW), and 27 for advantaged winners (AW). These differences in group sizes are important to consider when interpreting the results, as the relatively small sample sizes for the AL and DW groups may reduce the statistical power of the tests conducted for these conditions. Fisher's exact tests for the percentage of givers and takers are also non-significant[3].

To gain further insights, within-subject tests were conducted to compare participants' behaviour across different conditions. Specifically, each individual's redistribution behaviour following a fair rule game was compared with their behaviour after being either an advantaged or disadvantaged player in an unfair rule game. It is important to note that the number of observations in these comparisons is limited, as they require participants to have been either winners or losers in both games. Even with this within-subject approach, no significant differences were observed[4]. This suggests that individuals' preferences for redistribution do not vary substantially between fair and unfair rule settings. The lack of statistically significant differences suggests that participants' redistribution preferences are not significantly different in different treatments. Participants' redistributive decisions do not appear to be significantly influenced by whether they are playing under fair or unfair rules.

1.6.3 Regression Analysis

In this study, we estimate regressions of the transfer (Transfer) on treatment condition (Fair), game order (Second Game), and subject identifiers (Subject), separately for losers and winners. The variables used in the regressions are defined as follows:

Transfer: The same definition as we mentioned previously. If the player gives, their transfer is the decrease in their payoff, recorded as a negative number. If the player takes, their transfer is the decrease in the payoff of the other player, recorded as a positive number.

Fair: treatment effects. If the card game is fair, it is 1; otherwise 0.

[3] For more details, see Appendix 1.C.

[4] For more details, see Appendix 1.D.

SecondGame: the order of the games. If the result is from the second card game, it is 1, otherwise 0.

Subject: subjects' id which is unique for each participant.

The regression models for losers (j = l) and winners (j = w) are separately specified as:

$$\text{Transfer}_{ij} = \beta_{0j} + \beta_{1j} \cdot \text{Fair}_{ij} + \beta_{2j} \cdot \text{SecondGame}_{ij} + u_{ij}$$

Where $j \in \{l, w\}$ and standard errors are clustered at the subject level.

This formulation allows us to investigate whether the effects of fairness and game order on redistribution behaviour differ depending on whether a participant was in a loser or winner position.

Given that the dependent variable (Transfer) is continuous and hierarchical, we use ordinary least squares (OLS) regression, and therefore, logistic or probit models are not required (Hansen, 2022).

However, comparing groups, such as losers in fair-rule games with losers in unfair-rule games, poses additional challenges. Specifically, since some observations come from the first card game and others from the second game (where participants already have experience), a potential correlation between error terms and the dependent variable may arise. To address this issue, clustering standard errors at the individual (subject) level is recommended (Moffatt, 2015).

Finally, it's important to note that the absence of significant results in our analysis may partly be due to the limited sample sizes in some of our subgroups. Such limited sample sizes can affect the statistical power of our tests, potentially leading us to incorrectly fail to reject null hypotheses (type II errors). Therefore, caution is needed when interpreting these null findings, as they may reflect insufficient power rather than true absence of effects.

In table 1.1, regressions of the model can be observed. The dependent variable is the participants' transfers. There are two models. The first is related to transfers of losers and in the second the dependent variable is the transfers of winners. The independent variables are SecondGame, which is the order of games (whether the redistribution resulted from the first game or the second game), Unfair which is a treatment effect variable (whether the game is fair or unfair) and a constant variable. This is the result of OLS considering the clustering method on the individual's level.

The findings from the regression models reveal that the treatment effect, which represents the distinction between fair rule games and unfair rule games, is not statistically significant in both models. This indicates that, in this experiment, the fairness or unfairness of the games did not significantly influence participants' transfer decisions.

The order of the games has a mild significant effect on transfers. This suggests that

Table 1.1: Regression results

VARIABLES	Transfers of losers	Transfers of winners
Secondgame (order of the game)	7.527*	2.702
	(4.186)	(2.521)
Fair (treatment effect)	-1.073	0.252
	(4.186)	(2.521)
Constant	57.34***	10.19***
	(4.552)	(2.497)
Observations	168	168
R-squared	0.012	0.005

Note: Robust standard errors in parentheses.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

losers' redistribution preferences slightly vary depending on whether they played the first or the second game. Specifically, losers tend to slightly take more points in the second game, regardless of the first game's version. Whether the initial game followed fair rules or unfair rules, losers still took more points in the second round. This might be because they learn to take more points, and not because of the different versions of the game. However, the order of the game variable is not significant for winners.

Additionally, the regression analysis based on other dependent variables, such as gender, risk-taking preferences, trust in others, and personal finance levels, do not yield any statistically significant results. This indicates that these variables do not have a significant influence on participants' transfer decisions.

For more detailed information regarding the regression results with other depended variables, readers can refer to Appendix D, where the complete results of these additional regression analyses are reported.

1.7 Conclusion

The results of this research show that participants have similar redistribution preferences for unequal outcomes resulting from both equality and inequality of opportunity. In the first glance, one might point out that this is because 40% to 50% of participants choose to take away all possible points from their opponents. It is true that the preference to take the maximum amounts affects the results since this reduces the variation in the data. However, there is some evidence that this is not so unusual. For example, in Cappelen et al. (2013), 36% of all participants took the maximum payoff for themselves, while in our results, the average for all treatments is 43%.

There are some possible explanation of why there is more taking in our experiment than in Cappelen et al. (2013)'s. First, in our experiment, participants have to decide on redistribution while they are matched with a participant in another group. In fact, every participant from Klee's preferred group is matched with a participant from Kandinsky's preferred group. So, their perception that their co-players belong to the other group might be a reason why they want to take large shares of points from their co-players (out-group effect). This is in line with the results of Chen and Li (2009) showing that allocating money to other participants is less if they belong to different groups than if they belong to the same group. According to this, if participants in our experiment were matched with participants from their own group, they might have behaved differently, taking less and giving more consideration to fairness. Or if they were not assigned to different groups and if the manipulation stage were based on individual tasks rather than making groups, preferences might have been different. However, since the group-making manipulation was the same for all participants, this cannot be a reason why participants did not have different preferences for fair rule and unfair rule treatments.

Second, in our design, we use the strategy method to take participants' proposals for redistribution and only one of the proposals was randomly selected for final payments. This procedure is repeated in both games. Players are not matched with the same co-players in both games, but in both games they are matched with another person from the other group. It is possible that they think that their co-players from the other group want to take everything from them, so that it is reasonable to take everything from their co-players in both games no matter what happens. In other words, this is because of potential negative reciprocity. So, applying strategy method might lead to potential negative reciprocity.

Third, in our experiment, there is a 50% loss for any amounts participants choose to transfer. The reason we use this is that in standard economics and in the inequality aversion model (Fehr and Schmidt, 1999), a tax on transfers should reduce transfers, so make losers take less and winners give less. Particularly, in Fehr and Schmidt (1999)'s model, the disutility of disadvantageous inequality (having less money or being a loser in the terminology of this paper) is greater than the disutility of advantageous inequality (having more money or being a winner in the terminology of this paper). For example, a recent meta-analysis of experimental data has found mean values of α (disutiluty of disadvantageous inequality) is 0.39 and, that of β (disutility of advantageous inequality) is 0.17 (Nunnari and Pozzi, 2022). Therefore, the introduction of a "tax" on transfers in our experiment is expected to reduce the incentive to take the full amount, thereby moderating extreme behaviours. This design feature is likely to increase the power of the tests by generating more variation in transfer behaviour across treatment conditions.

The main conclusion of the experiment is that participants are more concerned about outcomes than procedures, particularly when considering the preferences of active players who are directly engaged in the outcome-generating process. This finding contrasts with Sugden and Wang (2020), that found that participants were more accepting of inequalities when they resulted from fair procedures, underscoring the importance of procedural fairness. While both studies focus on the behaviours of active participants, the divergence in results may arise from differences in experimental design; our study offers a more direct test of the relative importance of outcomes versus procedures for redistribution preferences.

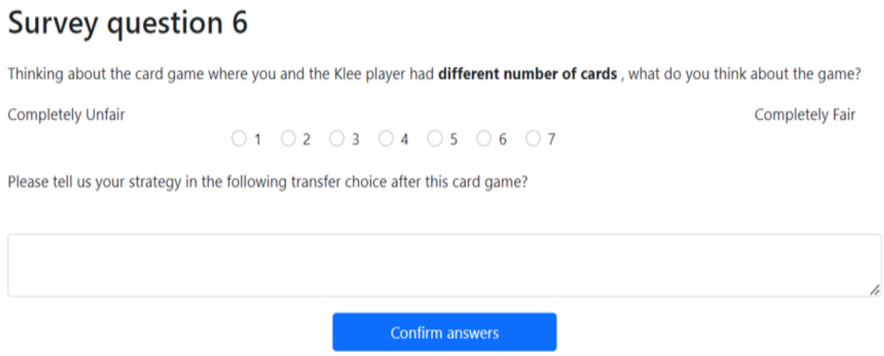
In contrast, Cappelen et al. (2013) found that although some participants adhered to the principle of equal ex ante opportunities, the majority favoured redistribution after outcomes were realised, suggesting a preference for ex post fairness. This finding is in line with our results, indicating that outcome-based fairness considerations may be more dominant in certain contexts. However, their experiment does not involve a direct comparison between outcomes and procedures through explicit procedural manipulations. Future research could further investigate the conditions under which outcomes or procedures are more influential in shaping fairness perceptions.

1.A Participants' perceptions of fairness

These are two questions related to participants' perceptions of the unfair and fair rule games.

1.A.1 Unfair rule game

Figure 1.A.1: Survey Question about Unfair Rule Game


Survey question 6

Thinking about the card game where you and the Klee player had **different number of cards** , what do you think about the game?

Completely Unfair ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 Completely Fair

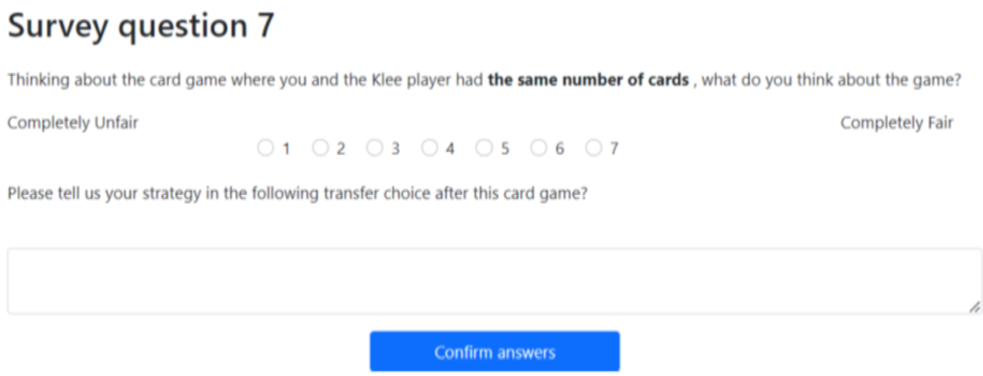
Please tell us your strategy in the following transfer choice after this card game?

Confirm answers

Participants gave average of 2.7 for this game.

1.A.2 Fair rule game

Figure 1.A.2: Survey Question about Fair Rule Game


Survey question 7

Thinking about the card game where you and the Klee player had **the same number of cards** , what do you think about the game?

Completely Unfair ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 Completely Fair

Please tell us your strategy in the following transfer choice after this card game?

Confirm answers

Participants gave average of 5.6 for this game.

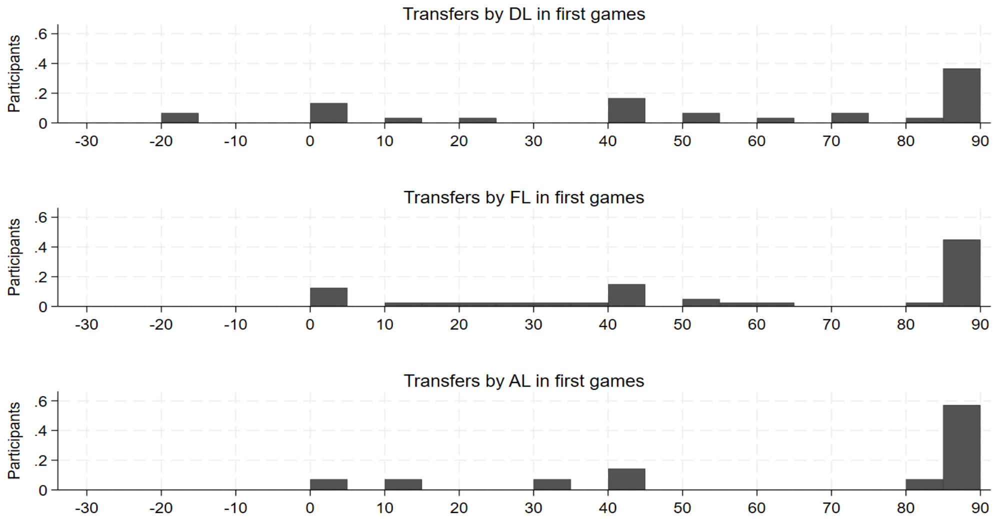
1.B Distributions of transfers in different games

you can see distributions of transfers in different games as follows.

1.B.1 Losers

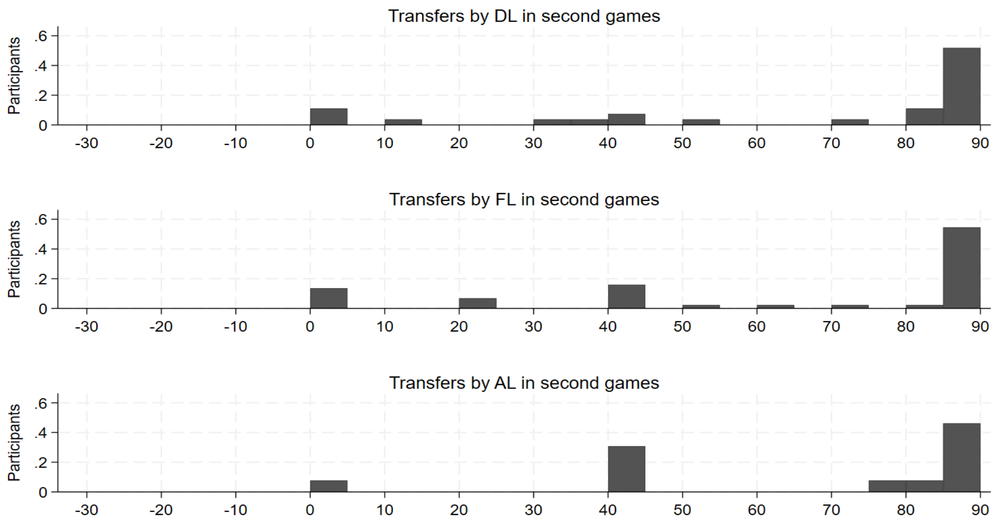
The distributions of transfers in losers separately in the first games from the second games are as follows:

Figure 1.B.1: Distribution of Transfers in First Games by Losers



Note:DL: disadvantaged losers. FL: fair losers. AL: advantaged losers.

Figure 1.B.2: Distribution of Transfers in Second Games by Losers

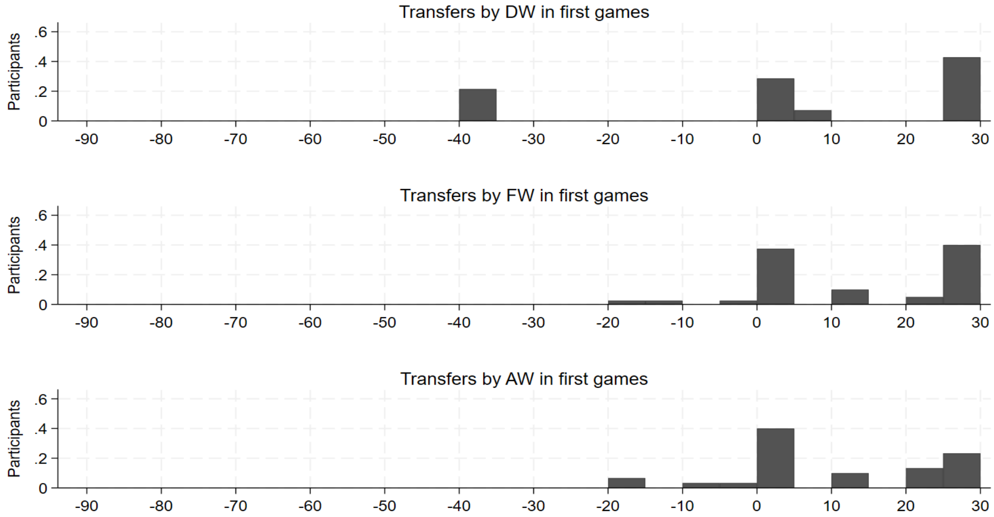


Note: DL: disadvantaged losers. FL: fair losers. AL: advantaged losers.

1.B.2 Winners

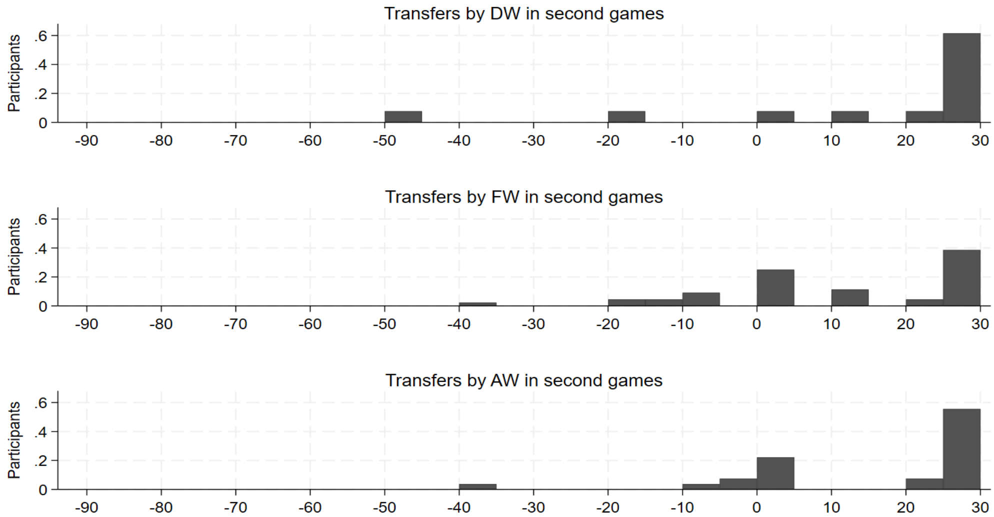
The distributions of transfers in Winners separately in the first games from the second games are as follows:

Figure 1.B.3: Distribution of Transfers in First Games by Winners



Note: DW: disadvantaged winners. FW: fair winners. AW: advantaged winners.

Figure 1.B.4: Distribution of Transfers in Second Games by Winners



Note: DW: disadvantaged winners. FW: fair winners. AW: advantaged winners.

1.C Fisher-exact tests

Table 1.C.1: Fisher's Exact Test Results for Treatment Take Rates

Comparison	Observations	Fisher's exact	1-sided Fisher
DL vs FL	141	0.806	0.415
AL vs FL	111	0.731	0.340
DW vs FW	111	0.509	0.301
AW vs FW	141	1.000	0.551

Notes: "Observations" indicates the total number of participants included in the comparison. "Fisher's exact" is the two-sided p-value from Fisher's exact test, and "1-sided Fisher" is the one-sided p-value.

1.D Within-subject tests

Table 1.D.1: Wilcoxon Signed-Rank Test Results

Comparison	Sum Pos.	Sum Neg.	z	p-value	Exact p
transFL vs transDL	95	112	-0.245	0.807	0.898
transFL vs transAL	13	57	-1.398	0.162	0.188
transFW vs transDW	0	27	-1.725	0.085	0.250
transFW vs transAW	186	144	0.438	0.662	0.723

Notes: Sum of Positive/Negative Ranks are from the Wilcoxon signed-rank test output. p-value is the asymptotic two-sided probability; Exact p is the exact test probability.

1.E Regressions based on extended independent variables

Table 1.E.1: Determinants of Transfers: Losers vs. Winners

VARIABLES	Trans. of Losers	Trans. of Winners
Treatment effect	-6.253	-6.280
	(7.762)	(4.086)
Gender (1 = Male)	7.235	-2.397
	(8.028)	(4.915)
Level of personal finance	1.359	3.066
	(3.847)	(2.216)
Risk-taking preference	-5.107	-3.570
	(3.539)	(2.247)
Trust in others	0.504	-1.246
	(1.935)	(0.943)
Constant	71.590***	27.390**
	(23.180)	(12.480)
Observations	84	84
R-squared	0.060	0.108

Notes: Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

1.F Survey Result

Table 1.F.1 shows that participants, on average, hold moderate views rather than extreme ideological positions.

Table 1.F.1: Survey 1: Liberal vs. Egalitarian Views

Variable	Mean	N	Min	Max	SD	SE	P50
q1 (normalized)	4.90	168	1.00	8.80	1.58	0.12	4.80

Note: This survey is divided into 5 questions; each question asks the participants how much they believe in liberal or egalitarian views. *1* means they think completely *liberal* and *10* means they are *egalitarian*. The variable q1 is normalised 5 questions in one variable.

Table 1.F.2 indicates that participants distinguish which game is fair and which one is not fair.

Additional survey measures are reported in Table 1.F.3, including financial status, risk attitudes, trust, and gender.

Table 1.F.2: Surveys 6-1 and 7-1: Perceptions of Fairness in Games

Variable	**Mean**	**N**	**Min**	**Max**	**SD**
Survey 6-1 (Unequal cards)	2.74	168	1	7	1.73
Survey 7-1 (Equal cards)	5.63	168	1	7	1.57

Note: Ratings range from 1 (completely unfair) to 7 (completely fair). Participants clearly distinguish between the fairness of unequal vs. equal card games.

Table 1.F.3: Surveys 2–4 and Demographics

Variable	**Mean**	**N**	**Min**	**Max**	**SD**	**SE**	**P50**
Level of personal finance	2.95	168	1.00	5.00	1.08	0.08	3.00
Risk-taking preference	2.90	168	1.00	5.00	1.20	0.09	3.00
Trust in others	6.88	168	2.00	10.00	2.25	0.17	7.00
Gender (1 = Male)	0.50	168	0.00	1.00	0.50	0.04	0.50

Note: These variables describe participants' self-assessed financial status (1 = very bad, 5 = very good), risk attitudes (1 = risk-taking, 5 = risk-averse), and trust in others (1 = high trust, 10 = low trust).

Chapter 2

Strategic Fairness and the Source of Inequality

This paper investigates the influence of strategic choice and strategic luck on individuals' preferences for redistribution, extending the existing literature on inequality and fairness in experimental economics. While much of the previous research has examined non-strategic settings where individuals' decisions are independent and luck is typically conceptualised as a lottery, this study introduces a strategic environment where incomes are shaped by both individual choices and the decisions of others. Furthermore, luck is redefined as a factor determined by others' choices rather than a random lottery. Each participant participates in two blocks: Block 1 introduces a novel strategic environment based on a pure coordination game (PC), followed by redistribution over 30 rounds; Block 2 employs a standardised adaptation of the experiment from Cappelen et al. (2013) to measure preferences under non-strategic choice and luck. The findings reveal that factors such as initial income distributions exert more significant effects on individuals' preferences for redistribution compared to the impact of strategic and non-strategic choice and strategic and non-strategic luck.

2.1 Introduction

Inequality is varied across the world and has been increasing within countries (Chancel et al., 2021). Inequality can lead to negative economic consequences such as socio-political instability (Alesina and Perotti, 1996), lower rate of human capital accumulation (Perotti, 1993) and negative economic growth (Barro, 2000; Knowles, 2005). However, there is not any consensus about whether there is a need for policies to reduce inequality and if there is a need how it should be met (Alesina and Angeletos, 2005; Starmans et al., 2017).

In traditional welfare theory, where maximization frameworks are grounded in utility theory, redistribution aims to address inequality without accounting for the origins of gaining resources. However, recent research underscores the significance of how incomes are acquired, suggesting that the perceived fairness of its origins can influence redistributive behaviour. While substantial work has explored inequality and fairness, the role of strategic considerations in shaping redistribution preferences remains under-examined. This study seeks to fill this gap by addressing the research question: Do strategic considerations influence redistributive preferences?

The study of inequality typically follows two broad methodological approaches: normative, which focuses on what ought to be, and positive, which analyses what is. In the normative approach, there are different perspectives on the issue of inequality. At one end of the spectrum, egalitarian theorists argue that all forms of inequality are unjust and should be eliminated, advocating for an equal distribution of resources among individuals (e.g., Temkin, 1993; Nagel, 1991). At the other end, liberal thinkers maintain that inequality resulting from individual choices should not be corrected, as individuals are entitled to the outcomes of their free decisions (e.g., Nozick, 1974). Situated between these two extremes is the view of luck egalitarianism, or choice egalitarianism, which focuses on the sources of inequality, specifically, whether they stem from individual choice or circumstances beyond one's control.

This normative perspective, grounded in normative economics, distinguishes between option luck and brute luck (Dworkin, 1981*a*,*b*; Roemer, 1993, 1998). Option luck refers to outcomes that result from deliberate and informed choices under uncertainty—for instance, choosing to participate in a lottery. If someone chooses to enter the lottery and ends up worse off than someone who chose not to, the resulting inequality is attributed to their own decision and is considered justifiable. In contrast, brute luck refers to inequalities arising from circumstances beyond a person's control, such as being forced into a lottery where outcomes vary significantly. In such cases, individuals are not responsible for the resulting disparities. Luck egalitarians argue that individuals should be held accountable for inequalities arising from their choices (option luck), but not for those resulting from brute luck, which warrant compensation or redistribution.

In the positive approach, there is empirical research which seeks individuals' views about fair redistribution when the inequality is created from specific factors (including choice and luck)(Cappelen et al., 2007; Fong, 2007; Mollerstrom et al., 2015; Gee et al., 2017; Konow, 2000). In this line of research, there is a focus on the origins of inequality, with particular attention to factors within individuals' control (such as their choices) and those beyond their control (such as luck). Some works in the literature argue that inequality arising from individuals' choices is generally perceived as fair, leading to less support for extensive redistribution policies and when inequality is attributed to luck, most individuals tend to favor more substantial redistribution policies (Almås et al., 2010; Cappelen et al., 2007, 2010).

The research questions in these studies mostly investigate whether individuals' moral view in the above-mentioned normative concepts (where people are held responsible for their decisions but not for luck) shapes their redistributive preferences. In this line of research, individuals are assumed to have moral fairness ideals about inequality. Observations of redistributive decisions by spectators (or observers who allocate money between other individuals) can reveal individuals' pure moral fairness judgements. However, stakeholders (active participants who should allocate money between themselves and another person) mix their fairness ideals with self-interest, and so their redistributive decisions are biased measures of their moral judgements. That is why most of the studies in this line rely more on spectators (observers) rather than stakeholders (active participants).

It is important to clarify that, in this context, luck usually refers to situations that are based on a fair lottery. However, this conceptualisation of luck as solely lottery-based overlooks strategic situations where luck is not contingent upon a random lottery administered by a third party or a machine but rather on the actions of other individuals engaged in the same situation. For instance, in a competitive market environment individuals' earnings result not only from their own actions but also from the actions of other individuals engaged in this strategic context. In this scenario, one aspect beyond an individual's control is luck, which is predicated on the actions of others, rather than a random lottery draw.

Notably, the existing literature on fairness considerations regarding factors within individuals' control versus those beyond their control has largely overlooked interactions between individuals engaged in strategic situations. Specifically, it has not accounted for how one player's actions can influence another's payoffs. This gap in the literature motivates our investigation into the factors contributing to inequality arising from strategic choice versus strategic luck.

In this paper, our contribution is to introduce the concept of strategic choice and strategic luck, and based on that we design an experiment in which choice and luck are based on the interaction of individuals, and luck is not a lottery. This design enables us not only to study the effect of strategic choice and strategic luck on individuals' redistributive preferences, but also to compare the effect of strategic versus non-strategic situations (e.g. strategic choice vs. choice). Additionally, unlike most papers in the literature (e.g. Cappelen et al., 2007, 2013), we do not assume that individuals necessarily have fairness ideals and that their redistributive preferences are shaped by those ideals. Instead, we study preferences for both active participants and observers since both preferences are equally important for explaining real-world situations.

According to our results, participants do not exhibit different redistribution preferences when comparing strategic luck with strategic choice, strategic luck with non-strategic luck, or strategic choice with non-strategic choice.

The following content of the chapter is arranged as follows: In Section 2.2, we will introduce the seminal studies related to our research questions. In section 2.3 and Section 2.4 research questions and the details of our experimental design are explained respectively. Our hypotheses will be specified in Section 2.5. Section 2.6 reports data analysis and explains the results related to the hypotheses. Finally, Section 2.7 is the conclusion.

2.2 Literature Review

In the experimental literature on redistributive preferences, the central question is how the origins of individuals' incomes (specifically, whether they result from luck, choice, or effort) influence attitudes towards redistribution (e.g., Cappelen et al., 2007; Konow, 2000). Most studies follow a similar experimental structure: in the first phase, participants earn income through an income-generating task; in the second phase, they engage in a redistribution procedure where they can redistribute the incomes earned in the first phase. Within this framework, researchers study the role of luck versus choice (e.g. Almås et al., 2010), luck versus effort (e.g. Cappelen et al., 2010), luck versus merit (e.g. Cappelen et al., 2023) or luck versus talent (e.g. Bartling et al., 2025). Our experiment adopts this standard framework but introduces several important and conceptually significant modifications.

Our study builds on the seminal study by Cappelen et al. (2013), which has become a foundational work in the experimental literature on fairness and redistribution. In this section, we review key aspects of their experimental design, as many subsequent studies adopt similar elements. We then highlight the main differences between their approach and ours, and clarify the conceptual and methodological contributions of our experiment.

Cappelen et al. (2013) investigate how individuals' perceptions of the sources of inequality shape their preferences for redistribution. The study focuses on two key determinants: luck and choice. The authors design an experiment in which income inequality is deliberately generated either by random chance (luck) or by participants' choices. After being informed about the origin of the inequality, participants are asked to indicate their preferred level of redistribution.

The experimental design is structured into two stages. The first stage involves an income-generating procedure, where participants choose between two investment options: a safe option, offering a guaranteed fixed amount of money, or a risky option, offering a 50% chance of earning nothing and a 50% chance of earning a higher amount than the safe option. The incomes for the safe option are randomly chosen from 25, 200, 300, or 400 NOK[1], while the risky option always yields either 0 or 800 NOK. For simplicity, consider a player choosing the safe option (when the safe option is 200 NOK) and receiving 200 NOK. This player could have either stuck with the guaranteed 200 NOK or opted for the risky investment, resulting in a 50% chance of earning either 0 or 800 NOK. Before starting the experiment, participants were randomly assigned to be either stakeholders or spectators. Only stakeholders participate in this stage.

The second stage, the redistribution procedure, features a dictator game. Stakeholders are divided into pairs, with one individual in each pair randomly assigned the role of dictator. The dictator is tasked with reallocating the combined income of themselves and their partner. For example, one participant might have 200 NOK (having chosen the safe option), while their partner has 800 NOK (having chosen the risky option and been fortunate). The dictator can decide how to redistribute the total of 1,000 NOK between them. Each participant is paired with eight different individuals, and in each pairing, one is randomly assigned as the dictator to reveal their redistribution preferences. In this stage, spectators participate and their task is to reallocate incomes between two other participants (stakeholders).

The analysis focuses on two key types of pairs. The first comprises individuals who chose the safe option (e.g. 200 NOK) paired with those who chose the risky option and were either lucky (800 NOK) or unlucky (0). Here, inequality stems from differing choices. The second type includes pairs where both participants chose the risky option, but one experienced bad luck (0 NOK) while the other experienced good luck (800 NOK). In this case, inequality is due to luck. In both types, a dictator (a spectator or one of the stakeholders) proposes a reallocation of the total incomes

[1]NOK is the Norwegian Krone.

of the pair between them. By comparing redistribution preferences across these two scenarios, the study examines whether the source of inequality (choice or luck) affects redistribution behaviour.

The design incorporates two distinct participant roles. Stakeholders actively engage in the income-generating task, while spectators, who do not participate in the investment task, only contribute to the redistribution stage by reallocating money between two stakeholders. Stakeholders are referred to as active players, fully involved in all tasks, whereas spectators are observers who merely observe the active players' decisions and subsequent outcomes before making redistribution choices.

The paper's findings indicate that spectators in safe-risky pairs redistribute less money than those in risky-risky pairs. Cappelen et al. (2013) explain that in the former, spectators perceive inequality as a consequence of personal choices, believing that individuals deserve their incomes irrespective of the amounts involved. In the latter, spectators recognise that inequality arises purely from luck, as both individuals made the same choice, and the difference in outcomes is attributed to a random lottery. The results for stakeholders follow the same pattern, but they are not as strong as spectators. The authors' explanation of the weaker results for stakeholders is that although stakeholders generally adhere to the same fairness principles as spectators, they exhibit a bias towards self-interest. The authors therefore treat spectators' behaviour as a purer expression of individuals' fairness preferences. For this reason, many studies in the literature prefer to include spectators rather than stakeholders. This trend is well-documented in the literature (see the review by Cappelen et al., 2020).

Several features of this and similar experimental designs are noteworthy. First, the income-generating task is conducted individually, with no interaction between participants. Secondly, luck is modelled as a fair lottery determined by a random device. Finally, the design uses an econometric model that assumes individuals hold different fairness ideals, but participants in the role of stakeholder exhibit selfish biases that prevent them from acting in accordance with their ideal of fairness. However, these designs pay limited attention to strategic situations, which are pervasive in the real world. Strategic situations can involve competition or coordination where earnings depend not only on individual actions but also on the actions of others; examples include financial markets and job markets. Our contribution to the literature lies in introducing a game-theoretic framework that incorporates interactive tasks involving multiple participants. Furthermore, we broaden the definition of luck to include factors beyond individual control that are not necessarily determined by random lotteries. Finally, we enable a direct comparison between redistribution driven by luck and by choice, without relying on econometric assumptions about individuals' fairness ideals.

The approach of our research is related to the positive aspects which is similar to the studies in the literature. However, we do not assume individuals have specific moral fairness value. So, in our study, we will take a direct test of preferences for redistribution without considering individuals' fairness ideal. Therefore, active players are not assumed to be biased. For example, taking the whole pie (total incomes) of a pair may be simply a preference. In this paper, we wish to fill the explained gap in the literature with a direct test of the hypothesis without the mentioned assumptions for moral fairness value.

2.3 Research Question

This research seeks to explore how strategic luck and strategic choice influence individuals' preferences for redistribution. To clarify the research question, it is essential to define strategic luck and strategic choice.

2.3.1 Definition of Strategic Choice and Strategic Luck

In this part, firstly we introduce the formal definition of strategic choice and strategic luck and then explain simple examples of this definition. The formal definition of strategic luck and strategic choice is as follows.

Consider a game with 2 players $(1, 2)$: Player 1 (P_1) and Player 2 (P_2) . The strategy set of P_1 is $S_1 = \{s_{11}, \ldots, s_{1n}\}$ and the strategy set of P_2 is $S_2 = \{s_{21}, \ldots, s_{2m}\}$.

Let the set of strategy profiles be:

$$\Sigma = S_1 \times S_2 = \{(s_1, s_2)\}$$

Each player $i = 1, 2$ has a payoff function:

$$\pi_i(s) = \pi_i(s_1, s_2)$$

where s is a strategy combination of s_1 and s_2 .

If the game is played by persons A (as Player 1) and B (as Player 2), their payoffs are shown by $\pi_1(s), \pi_2(s)$ respectively.

Now consider we have four players A, B, C, D who play this game in which

- A plays as P1 vs B as P2
- C plays as P1 vs D as P2

then *Strategic luck* and *Strategic choice* between A and C (who have played the same role as P1) are defined as follows:

- A's strategy: $s_A \in S_1$
- B's strategy: $s_B \in S_2$

$\Rightarrow$ A gets $\pi_1(s_A, s_B)$

- C's strategy: $s_C \in S_1$
- D's strategy: $s_D \in S_2$

$\Rightarrow$ C gets $\pi_1(s_C, s_D)$

Strategic Luck

This is a strategic luck for the pair of A and C if

$$s_A = s_C = s', \quad s_B \neq s_D$$

The resulting strategic luck inequality in favour of player A is as follows:

$$\text{Strategic luck inequality in favour of A} = \pi_1(s', s_B) - \pi_1(s', s_D)$$

Definition: Inequality between players i and j arises from *strategic luck* if both make the same decisions, but one gains an advantage due to differing choices made by their co-players, factors beyond the control of i and j.

Strategic Choice

This is a strategic choice for the pair of A and C if

$$s_A \neq s_C, \quad s_B = s_D = s''$$

The resulting strategic choice inequality in favor of player A is as follows:

$$\text{Strategic choice inequality in favour of A} = \pi_1(s_A, s'') - \pi_1(s_C, s'')$$

Definition: Inequality between players i and j arises from *strategic choice* if both face the same options, but one gains an advantage due to making a different decision from the other, reflecting a difference in the choices of i and j rather than external factors.

2.3.2 Concept of Strategic Luck

Strategic luck refers to circumstances beyond individuals' control, but unlike conventional notions of luck, it is not determined by a lottery. Instead, it arises from the interactions between individuals and others engaged in the same games.

Consider a pure coordination game (employed in our experimental design) where both players choose between two options, e.g., Rome (R) or Paris (P)[2]. Paris and Rome are labels that are seen by both players. If both select the same option, they each receive £20; otherwise, they earn £0. It is well established (Since Schelling, 1960) that for many label pairs, ordinary human players coordinate with probabilities significantly greater than 0.5. That is, achieving the higher payoff in such coordination games is not pure luck. The strategic form of this game is presented in Table 2.3.1.

Table 2.3.1: PC Game for Pair 1

	P	R
P	(20,20)	(0,0)
R	(0,0)	(20,20)

Suppose Player 1 and Player 2 play this game, and both choose P. In this case, they each earn £20. Now consider a similar game played by Player 3 and Player 4, as shown in Table 2.3.2.

Table 2.3.2: PC Game for Pair 2

	P	R
P	(20,20)	(0,0)
R	(0,0)	(20,20)

In this game, if Player 3 chooses P and Player 4 chooses R, both earn £0. Comparing Player 1 (from Table 2.3.1) and Player 3 (from Table 2.3.2), we observe inequality in their earnings (£20 vs £0), even though both made the same choice (P). The source of this inequality is not their choices but the choices of their co-players, which are beyond their control. This is an instance of luck, not luck arising from a random lottery, but from the decisions of others within the strategic environment. Thus, strategic luck refers to situations where outcomes are determined by the interplay of individuals' actions and those of others, rather than by external random mechanisms.

[2]P and R are just labels for strategies that are isomorphic, and they are payoff-irrelevant.

2.3.3 Concept of Strategic Choice

Strategic choice, by contrast, arises when outcomes are determined by individuals' own decisions within a strategic environment involving others.

Returning to Table 2.3.1, suppose Player 1 chooses P, and Player 2 also chooses P, resulting in earnings of £20 each. Now consider Table 2.3.2, where Player 3 chooses R, and Player 4 chooses P, resulting in £0 for both. Comparing Player 1 and Player 3, the inequality (£20 vs £0) stems entirely from their different choices, while their co-players made the same choice (P). This inequality is thus attributed to strategic choice.

It is important to note that strategic choice requires that the source of inequality is solely the individuals' own decisions. If co-players' choices also contribute, the inequality involves both choice and luck. The same principle applies to strategic luck.

2.3.4 Research Question and Contribution

Our research examines individuals' redistribution preferences when inequalities arise from either strategic luck or strategic choice. To explore this, we employ a pure coordination game (PC), which, to our knowledge, has not been utilised in the existing literature.

We use the pure coordination (PC) game for the following reasons:

(i) it allows for only two possible outcomes (e.g., 0 and 1), making the task simple and the payoff structure easy to interpret;

(ii) it can generate inequality in outcomes and the inequalities are the same in all situations, which allows more control for studying redistributive preferences.

(iii) it cannot be solved by iterated elimination of weakly dominated strategies, which preserves meaningful strategic uncertainty and coordination challenges in the experiment.

For these requirements, only two possible 2×2 game matrices satisfy the criteria: pure coordination and matching pennies. These conditions are difficult to simultaneously satisfy in many game forms, but they are particularly useful in an experimental setting where the goal is to study fairness and coordination without the interference of payoff complexity or role-based advantages. While our analysis is based on the PC game, the conceptual distinction between *strategic choice* and *strategic luck* is not limited to this setting and can be extended to other classes of games that involve interdependent decisions and outcome uncertainty.

Proposition 1. *The only 2×2 games that satisfy the three above-mentioned criteria are either pure coordination games or matching pennies.*

Proof. Consider the payoffs for one of the players, say Row. There must be at least one cell with a payoff of 1 and at least one with a payoff of 0. If both payoffs in the same row are 1, or if both are 0, the game can be solved through dominance. Therefore, the 1s must be on one diagonal and the 0s on the other. Now consider Column's payoffs. There are only two strategically distinct possibilities: (i) Column's 1s payoffs align with Row's 1s on the main diagonal, representing pure coordination, or (ii) Column's 1s align with Row's 0s on the opposite diagonal, representing matching pennies. □

In either game, the two strategies available to a given player are isomorphic in the standard game theory sense, differing only in terms of labelling. However, as shown in the literature (e.g. Mehta et al., 1994*b*; Hargreaves Heap et al., 2017), players can use the salience of labels to facilitate coordination. Intuitively (and as confirmed in our experiment by response times), choosing the more salient label requires no additional effort but represents a conscious choice rather than just 'luck'. The reason we do not use the matching pennies game is that players in this game cannot use the salience of the labels to improve their cooperation.

In the game, players select one option, with their payoffs determined by the interaction of their own and their co-players' choices. Then, for a redistribution stage, they are re-matched with different players, resulting in income inequalities from PC game.

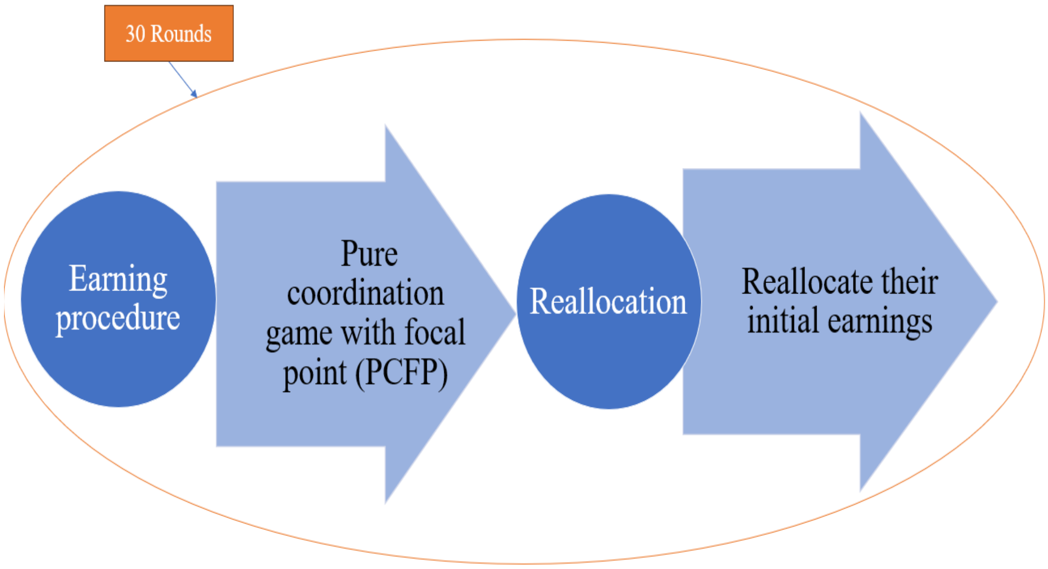
Participants are informed of the source of these inequalities—either strategic luck or strategic choice—and are asked to propose redistribution. There are also Observers who did not play the game and just reallocate initial incomes of two Game Players between them. Our key contribution is the use of a strategic game in a unique setting, where groups differ between the game and redistribution stages. This design enables us to elicit participants' redistribution preferences based on their perception of the source of inequality, whether it is strategic luck or strategic choice. By addressing this specific context, we advance the literature on redistribution preferences in strategic environments.

In addition to this research objective, we are also interested in comparing the effect of strategic choice and strategic luck versus non-strategic choice and luck. To enable this comparison, we include Block 2, in which the same participants from Block 1 also take part. The whole design for this paper which includes two blocks, bridges the gap between strategic and non-strategic environments by allowing a direct comparison of redistribution preferences. Specifically, it juxtaposes the effects of strategic choice and strategic luck, as examined in block 1, with the effects of choice and luck in a non-strategic framework, as explored by Cappelen et al. (2013). By integrating these dimensions, we can calibrate individuals' preferences for redistribution across contexts, providing a more nuanced understanding of how strategic and non-strategic factors shape fairness considerations. This comparative approach helps to situate strategic redistribution preferences within a broader theoretical and empirical framework, advancing the literature on fairness and decision-making.

2.4 Experimental design

To address the research question, we conducted two independent blocks, explained in the following sections.

Figure 2.4.1: Experimental Design For Block 1



2.4.1 Block 1: Strategic choice versus strategic luck

The first block of the experiment is designed to address key aspects of the research question and consists of a series of rounds. Each round in this block has two stages. Stage 1 is an income-generating procedure where participants engage in a task to earn money. Stage 2 is a redistribution procedure where participants reallocate their initial incomes.

There are two roles in this block: Game Players and Observers. Game Players participate in both stages, while Observers only participate in Stage 2. The experiment comprises 30 rounds. In each round:

- Game Players make decisions in a Pure Coordination (PC) game and receive feedback on their own and their co-player's choices, as well as their income.

- Participants are then randomly rematched with a new partner (different from their co-player) and reallocate their initial incomes.
- Observers do not play the PC game in stage 1 and they allocate the total incomes of a pair of Game Players in stage 2.

The experimental design is described in detail below.

2.4.1.1 Block 1- Stage 1: Income-Generating Procedure

In Stage 1, only Game Players are involved. Participants are informed that they will be randomly matched with another Game Player in each round. Game Player are presented with two options and must select one. If their choices match, they earn a higher income than if they do not. In the experiment, the payoff structure varies across 30 rounds:

- 10 rounds: £20 for matching choices, £0 for non-matching;
- 10 rounds: £20 for matching choices, £5 for non-matching;
- 10 rounds: £5 for matching choices, £0 for non-matching

The order of rounds is different in different sessions. If the players' choices match, they earn £20 or £5 depending on the round, and if the choices do not match, they earn either £5 or £0, depending on the round. The reason why we chose only three payoffs during all rounds is to allow the PC game to be matched with an investment game in Block 2. In Block 1 with these three payoffs, our possible inequalities are £20-£0, £20-£5 and £5-£0 which are matched with inequalities in the investment game in Block 2. In this way, we can compare preferences for redistribution for the same individuals in both Block 1 (influenced by strategic luck/choice) and Block 2 (influenced by luck/choice).

The two options in each round are different. The options in each round are selected from 10 predefined categories. For each category, a ranking from some external source is used. Categories are as follows: "Clothing" [3], "Car"[4], "Football"[5],

[3] Most valuable clothing companies in the world, by brand value 2021-2023 listed in https://www.statista.com/statistics/1207840/top-apparel-clothes-brands-worldwide/

[4] Best-selling car brands 2022 in the United States listed in https://www.statista.com/statistics/264362/leading-car-brands-in-the-us-based-on-vehicle-sales/.

[5] the best Premier League clubs according to the number of wins in all seasons from 1992 until April of 2024 listed in https://www.premierleague.com/stats/top/clubs/wins?co=1&se=-1.

”City”[6], ”Actors”[7], ”Actresses”[8], ”Films”[9], ”Fruits”[10], ”Books”[11] and ”Sports”[12].

Each category contains the six items that are ranked between 4 and 9. In total, participants play 30 rounds, divided into three blocks of 10 rounds. Each block consists of one round per category, using two items from that category as labels. The random selection of items is carried out independently in each experimental session to avoid systematic patterns across sessions.

There are several reasons why we use this method for pairing items. Firstly, the items ranked 4 to 9 in each category were chosen to ensure that the options are neither too obvious nor too difficult for participants to identify. This balance is intended to discourage random choice and promote sense of strategic decision-making. Secondly, randomly selecting item pairs for each round helps prevent participants from inferring experimenters’ expectations and reduces potential biases across sessions. Finally, we want to ensure that all items are accessible to participants and that all cues are easily understood, making the cues effectively neutral.

The structure of item randomisation and selection in different rounds of the pure coordination game is as follows:

- Rounds 1–10: One round per category, using two randomly selected distinct items without replacement from each category .
- Rounds 11–20: One round per category, using two new randomly selected distinct items without replacement (not used in the first block).
- Rounds 21–30: One round per category, using the remaining two distinct items from each category.

In each round, after playing the game, participants are informed of their own and their co-players’ choices, as well as the resulting incomes.

[6]Leading city destinations worldwide in 2018, by number of overnight visitors in millions listed in https://www.statista.com/statistics/310355/overnight-visitors-to-top-city-destinations-worldwide.

[7]Top 100 greatest actors of all time ranked according to their lifetime success (awards & nominations), along with their acting skills, versatility and role transformation by ChrisWalczyk55 listed in https://www.imdb.com/list.

[8]Top 50 greatest actors of all time ranked according to their acting abilities, their success, their versatility, and the difficulty of the roles they’ve played through out their careers by ChrisWalczyk55 listed in https://www.imdb.com/list.

[9]Top movies based on IMDB ranking in https://www.imdb.com/chart/top.

[10]The healthiest fruits based on BBC food in www.bbcgoodfood.com.

[11]The greatest books of all time based on thegreatestbooks.org.

[12]The most popular sports for 16-24 year old based on participation size on caytoo.co.uk which is based on Sport England’s Active Lives Survey.

2.4.1.2 Block 1- Stage 2: Redistribution Procedure

In each round, after Stage 1, Game Players are informed that they are re-matched with another Game Player who is not their co-player in Stage 1. Then Game Players and Observers make decisions in Stage 2.

Each Game Player acts as a dictator for their re-matched Game Player, referred to as the ”recipient.” The Game Player knows what they choose, what their coplayer chose and their own payoffs but does not know what the recipient did or the recipient’s payoffs. There are four scenarios based on the recipient’s actions and the recipient’s coplayer’s actions in Stage 1. The payoffs in these scenarios may vary each round. For example, in a round of the coordination game where the labels are Paris and Rome, and payoffs are £20-0, the four scenarios are as follows:

- Scenario 1: The recipient and their co-player both chose Paris, so each of them has £20 in their account;
- Scenario 2: The recipient chose Paris and their co-player chose Rome, so each of them has £0 in their account;
- Scenario 3: The recipient chose Rome and their co-player chose Paris, so each of them has £0 in their account;
- Scenario 4: The recipient and their co-player both chose Rome, so each of them has £20 in their account.

Each Game Player must decide how much money to redistribute between themselves and their recipient in each scenario. These decisions can affect the two players’ payoffs only if the scenario corresponds with what actually happened in the recipient’s game. Note that the Game Player’s recipient is not their dictator, meaning that the recipient makes redistribution decisions for a different Game Player who is neither their co-player in stage 1 nor their dictator. This design prevents potential reciprocity between Game Players.

After completing the redistribution of initial incomes, participants do not receive any feedback until the end of the experiment. They then proceed to the next round, which includes both stages.

The Observers’ task is to divide the initial incomes between two Game Players, referred to as Player A and Player B. The Observer knows what happened to Player A but does not know about Player B. Thus, there are four scenarios for Player B. The incomes in these scenarios may vary each round. For example, in a round of the coordination game where payoffs are £20-0, the four scenarios are as follows:

- Scenario 1: Player B and their co-player both chose Paris, so each of them has £20 in their account in Stage 1;
- Scenario 2: Player B chose Paris and their co-player chose Rome, so each of them has £0 in their account;
- Scenario 3: Player B chose Rome and their co-player chose Paris, so each of them has £0 in their account;
- Scenario 4: Player B and their co-player both chose Rome, so each of them has £20 in their account.

Based on each of these four scenarios for Player B, the Observer decides how to redistribute money between Player A and Player B. Thus, for each Game Player in each round, there is one Observer for that round. Consequently, the number of Game Players and Observers is equal.

This is a within-subject design since each player encounters both strategic luck and strategic choice situations. Game Players know that one round will be randomly selected for payment[13], but they do not know which one. Payments for Observers are fixed and do not depend on their performance.

This design enables us to test the effects of strategic luck and strategic choice on participants' redistribution preferences. In each round, each participant (both Game Players and Observers) faces one strategic choice and one strategic luck situation with the same inequality among the four scenarios. This provides a clear test for comparing the effects of strategic luck and strategic choice on participants' redistribution preferences.

2.4.2 Block 2: Choice Versus Luck

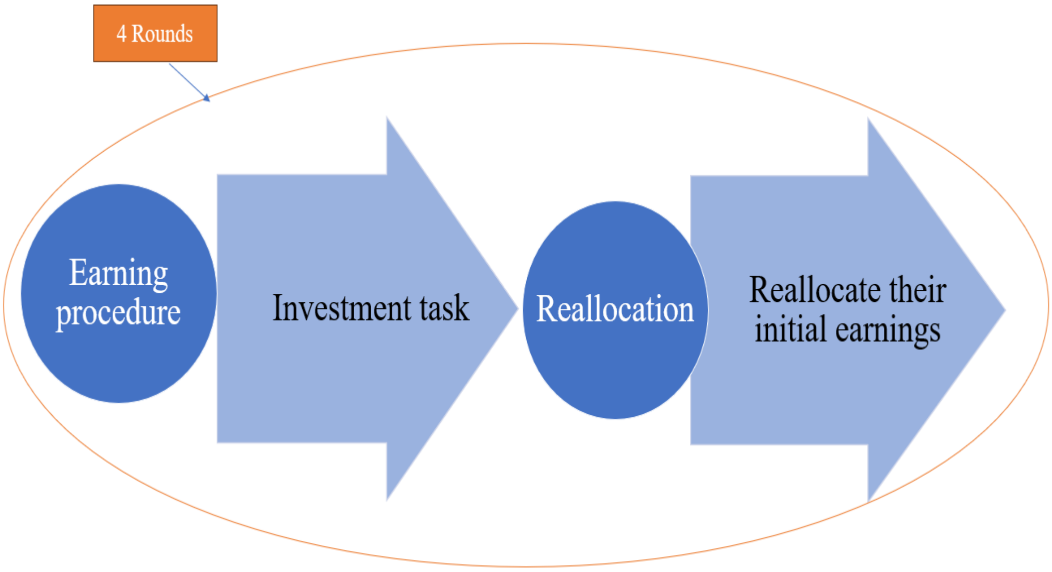
This block of the experiment builds on the design of Cappelen et al. (2013), with some modifications to align it with the block 1 of the experiment. Like that block, it comprises two stages: Stage 1, an income-generating procedure, and Stage 2, a redistribution procedure.

The roles in this Block are Investors and Observers. Investors participate in both stages, whereas Observers are only involved in Stage 2.

There are four rounds. In each round, Investors first make decisions in an investment task, choosing between two options: a safe option and a risky option. Subsequently, they learn their own and their co-players' outcomes. Then, they are

[13]One round from all sessions, including both experiments, is randomly selected for payment. In total, one of the 34 rounds is chosen for payment.

Figure 2.4.2: Experimental Design For Block 2



randomly matched with another participant and proceed to reallocate initial rewards between themselves.

2.4.2.1 Block 2 - Stage 1: Income-Generating Procedure

In Stage 1, only Investors participate in an investment task. The task requires choosing between two investment options:

- **Safe option**: Provides a fixed amount of income.
- **Risky option**: Has a 50% chance of providing a higher return than the safe option but a 50% chance of yielding nothing.

For three rounds, the safe investment yields £5, while the risky investment offers either £20 or £0 with equal probability. These rounds can produce inequality that is due to luck or choice.

In one round, the safe option yields £0, while the risky investment offers either £20 or £0 with equal probability. This additional round is designed to distinguish outcomes driven by option luck from those driven by brute luck. Brute luck refers to circumstances in which individuals have no meaningful choice and are instead subject to luck. By setting the safe option to £0 (a weakly dominated choice) we effectively eliminate any meaningful decision, as there are no reasons to select it.[14] Thus, the resulting £20–£0 outcome can be interpreted as driven by brute luck. Including the £0 safe option, rather than presenting only the risky £20–£0 outcome, allows us to

[14]This is in line with observed behaviour in the lab, where no participant chose the £0 option.

preserve a consistent structure and set of instructions across all rounds in Block 2, and enables randomisation across the four rounds of this block.

After making their decisions, Investors are informed of their choices and resulting incomes.

2.4.2.2 Block 2 - Stage 2: Redistribution Procedure

In Stage 2, Investors are matched with a new participant, and both Investors and Observers make redistribution decisions. Each Investor acts as a dictator for their matched recipient. The Investor knows their own income but has no information about their recipient's actions or income. The recipient's outcomes from Stage 1 fall into one of three scenarios. For example, in a round where the safe option yields £5 and the risky option yields £20 or £0, the scenarios are:

- **Scenario 1**: The recipient chose the safe investment and earned £5.
- **Scenario 2**: The recipient chose the risky investment, and it yielded a high return, resulting in £20.
- **Scenario 3**: The recipient chose the risky investment, and it yielded nothing, resulting in £0.

Investors decide how to redistribute money between themselves and their recipients in each scenario. Recipients do not make decisions about their dictators, ensuring no reciprocity between matched participants.

Observers, on the other hand, redistribute the initial incomes between two Investors: Investor A and Investor B. Observers know the outcomes for Investor A but not for Investor B. For Investor B, the outcomes align with the same three scenarios as above.

Observers' redistribution decisions are made based on these scenarios, and there is one Observer for each Investor in every round. Thus, the number of Investors equals the number of Observers.

Pairs of safe-risky choices represent inequalities based on choice, while pairs of risky-risky choices (if one is lucky and the other unlucky) represent inequalities arising from luck. This within-subject design ensures that each Investor and Observer encounters both luck-based and choice-based inequalities.

Investors are aware that one round will be selected randomly for payment[15], but they do not know which round. Observer payments are fixed and independent of their performance.

[15] One round from the entire session, which includes both experiments, is selected randomly for payment. In total, one of the 34 rounds is selected for payment.

This experiment allows us to compare the effects of strategic choice and strategic luck (as studied in Block 1) with choice and luck in a non-strategic environment, as explored in the existing literature, particularly Cappelen et al. (2013). By doing so, we can scale individuals' redistribution preferences in strategic contexts against those in non-strategic contexts, helping to position strategic redistribution preferences within a broader framework.

2.4.3 Implementation

We utilised oTree (Chen and Wickens, 2016) to conduct the experiment, implementing a total of 15 sessions with 12 participants in each. The sessions took place in May and October 2024 at the Laboratory for Economic and Decision Research (LEDR) at the University of East Anglia. In each session, 6 participants were randomly assigned as game players in Block 1 and as investors in Block 2, while the remaining 6 participants acted as observers in both experiments. Participants received an average payment of £25.50 for approximately 100 minutes of participation.

2.5 Hypotheses

The first hypothesis compares individuals' redistribution behaviour following outcomes determined by strategic choice versus strategic luck. The literature does not yet offer a direct comparison between strategic choice and strategic luck. However, the analogy with non-strategic luck/choice suggests that there might be such an effect which means that, for a given initial inequality, preferences for redistribution differ according to whether the inequality resulted from luck or from choice. The related null hypothesis is 'no difference' between individuals' preferences for redistribution in the two cases, and we investigate whether there is a difference between them.

Hypothesis 1. (Strategic Choice vs Strategic Luck): There is a difference between redistribution following strategic choice and following strategic luck.

- **H1a (Game Players as Active Players):** Game Players' redistribution following strategic luck is different from that following strategic choice.
- **H1b (Observers):** Observers' redistribution following strategic luck is different from that following strategic choice.

The second and third hypotheses examine the differences between strategic and non-strategic settings. There is no existing literature directly comparing these conditions, so we investigate whether individuals' redistribution differ across strategic and non-strategic contexts.

Hypothesis 2. (Strategic Choice vs Non-Strategic Choice): Individuals' redistribution following strategic choice differs from redistribution following non-strategic choice.

- **H2a:** Redistribution by Active Players' (Game Players in strategic and Investors in non-strategic settings) following strategic choice differs from their redistribution following non-strategic choice.

- **H2b:** Redistribution by Observers following strategic choice differs from their redistribution following non-strategic choice.

Hypothesis 3. (Strategic Luck vs Non-Strategic Luck): Individuals' redistribution following strategic luck differs from redistribution following non-strategic luck.

- **H3a:** Redistribution by Active Players' (Game Players in strategic and Investors in non-strategic settings) redistribution following strategic luck differs from their redistribution following non-strategic luck.

- **H3b:** Redistribution by Observers following strategic luck differs from their redistribution following non-strategic luck.

Since our design is within-subject, comparisons between strategic and non-strategic conditions are made within the same individuals. This sharpens the test of our hypotheses[16].

The final hypothesis focuses on the distinction between two types of non-strategic luck: option luck and brute luck. There are some theories (e.g. Dworkin (1981*b*)) expecting individuals to be more inclined towards redistribution when outcomes are determined by brute luck, as individuals have no control and are forced into that situation. So, according to these theories, the expectation is to have redistribution from brute luck rather than option luck. But as a researcher, we do not know what our data can be and just wish to test the null hypothesis whether these sources of luck make a difference to individuals' redistribution preferences.

Hypothesis 4. (Option Luck vs Brute Luck): Redistribution following from brute luck is different from that following option luck.

- **H4a (Investors as Active Players):** Investors redistribute differently when the outcome is due to brute luck rather than option luck.

- **H4b (Observers):** Observers redistribute differently when the outcome is due to brute luck rather than option luck.

[16]For each participant, we observe redistribution choices in all conditions: strategic choice, strategic luck, and at least one non-strategic condition (choice, luck, or both).

Note that for all comparisons, we control for participants' payoffs (i.e. whether they have higher or lower initial incomes) and their roles (player or observer).

2.6 Results

Since our hypotheses focus on inequality, we restrict our analysis to cases involving unequal initial outcomes. To do so, we define a "winner" as the individual who initially (after game or task and before redistribution) earns more money than their paired counterpart, and a "loser" as the individual who earns less. Although these two individuals did not participate in the same game, one emerged with a higher income. Dictators (if they are active players), who can be either the winner or the loser, decide on the redistribution of money. Our measure is the share allocated[17] to the winner of the total income of the two players in each pair, regardless of whether the winner is the dictator or the recipient.

Dictators (who can be Active Players who are winners, Active Players who are losers, or Observers) decide on the redistribution of money between the winner and the loser. Our measure of redistribution is the share of the total income of the two players that is allocated to the winner by the dictator, regardless of whether the dictator is a winner, loser or Observer.

When a dictator decides on redistribution for each scenario, the winner's initial income is denoted as x_w , and the loser's initial income is x_l, with their combined total being X. After the dictator's redistribution decision, the final incomes of the winner and loser are y_w and y_l, respectively.

To measure redistribution, we define the following index:[18]

[17] Throughout the paper, we use the term "allocate" to refer to the act of a dictator dividing a pie (total income of a pair) between two players, that is, determining how much each player receives in the final outcome (y_w for winners or y_l for losers). We use "give" to describe the transfer of income made by a dictator who is an active player. If the dictator ends up with less than their initial payoff, the amount they have "given" is the reduction in their payoff. If the dictator ends up with more than their initial payoff, the amount they have "taken" is the increase in their payoff.

[18] The index used for data description in Cappelen et al. (2013), which is more commonly found in the existing literature, differs slightly. It is defined as the share given, as follows:

$$\text{ShareGiven} = \frac{y_r}{X} \tag{2.1}$$

where:

- y_r: The amount of money the dictator allocates to the other player
- X: The total amount of money the dictator and the other player have together

This index does not reflect the initial situations of dictators when dictators are active players and mostly informative about that whether dictators are selfish or not. Once dictators are observers, the recipient is not unique among different pairs. In contrast, the index we use in this paper shows the redistribution of initial incomes of winners and losers. This captures the notion that the challenge lies in redistributing initial incomes. We believe our measure is clearer as it is more informative about the

$$\text{ShareGiven} = \frac{y_w}{X} \tag{2.2}$$

where:

- y_w: The amount of money the dictator allocates to the winner
- X: The total amount of money held by the winner and the loser, defined as $x_w + x_l$

We use this index for all participants– Game Players and Observers in Block 1 and Investors and Observers in Block 2.

2.6.1 Descriptive Statistics and statistical tests

Table 2.6.1 reports the average proportions of total payoffs allocated to winners by the three types of dictator (winners, losers and observers) for each initial payoff pair (20–0, 20–5 and 5–0) in each of Block 1 and Block 2. For Block 1, the experiment provided data for both strategic choice and strategic luck for each of the three payoff pairs. For Block 2, it provided data for (i) non-strategic luck in the 20-0 payoff pair (i.e., Risky Winner-Risky Loser), (ii) non-strategic choice in the 20-5 payoff pair (i.e., Risky Winner-Safe), and (iii) non-strategic choice in the 5-0 payoff pair (i.e., Safe-Risky Loser).

The ”Test” column in Block 1 shows p-values from t-tests comparing strategic luck and strategic choice at the individual level. These t-tests examine whether there is a significant difference in the average amount participants transferred depending on strategic luck and strategic choice. For each participant, the difference in transfers between these two conditions was calculated. The t-tests then assess whether the average of these differences across all participants is significantly different from zero.

Null hypotheses for these tests state that there is no difference in dictators' allocations between the two conditions, i.e., that participants' redistributive decisions do not depend on whether the relevant inequality resulted from strategic choice or strategic luck.

The t-tests reported in Block 2 compare transfer amounts between strategic choice and non-strategic choice, or strategic luck and non-strategic luck, across different games, using a within-subject design. Each participant who played as an active player in Block 1 faced both strategic luck and strategic choice conditions in different rounds. In Block 2, the same participants faced either non-strategic luck or non-strategic choice in different rounds. This design allows us to directly compare,

initial positions of winners and losers in the redistribution. Nevertheless, either index can be validly employed to examine differences between choice and luck.

Table 2.6.1: Basic Descriptive Statistics for Active Players and Observers.

	Game Players (Block 1)						Observers (Block 1)		
	Winners			Losers			–		
Payoffs Pair	St. Choice	St. Luck	Test	St. Choice	St. Luck	Test	St. Choice	St. Luck	Test
20–0	.895	.891	0.333	.328	.332	0.784	.645	.650	0.406
	(588)	(588)	(90)	(312)	(312)	(88)	(900)	(900)	(90)
20–5	.872	.859	0.068*	.277	.287	0.474	.630	.630	0.952
	(544)	(544)	(90)	(356)	(356)	(89)	(900)	(900)	(90)
5–0	.916	.913	0.841	.276	.294	0.372	.686	.671	0.115
	(548)	(548)	(90)	(352)	(352)	(90)	(900)	(900)	(90)
Aggregate	.894	.888	0.159	.292	.303	0.317	.653	.651	0.5891
	(1680)	(1680)	(90)	(1020)	(1020)	(90)	(2700)	(2700)	(90)
	Investors (Block 2)						Observers (Block 2)		
	Winners			Losers			–		
Payoffs Pair	Choice	Luck	Test	Choice	Luck	Test	Choice	Luck	Test
20–0	–	.925	0.350	–	.274	0.758	–	.719	0.024**
	(–)	(106)	(71)	(–)	(97)	(63)	(–)	(203)	(90)
20–5	.881	–	0.872	.371	–	0.162	.663	–	0.095*
	(106)	(–)	(71)	(67)	(–)	(38)	(173)	(–)	(90)
5–0	.880	–	0.988	.313	–	0.038**	.765	–	0.023**
	(67)	(–)	(38)	(97)	(–)	(64)	(164)	(–)	(82)
Aggregate	.881	.925		.337	.274		.730	.719	–
	(173)	(106)		(164)	(97)		(270)	(203)	(–)
	Investors (Block 2)						Observers (Block 2)		
	Winners			Losers			–		
Payoffs Pair	Option Luck	Brute Luck	Test	Option Luck	Brute Luck	Test	Option Luck	Brute Luck	Test
20–0	.927	.887	0.286	.295	.295	0.758	.684	.656	0.059*
	(71)	(46)	(41)	(64)	(44)	(29)	(67)	(44)	(35)

Notes: This table reports the proportion of total payoffs allocated to share given to the winners by dictators in different tasks. The upper panel (Block 1) reports data for game players, subdivided by whether the dictator was a winner or a loser, and for observers. The lower panel (Block 2) reports data for investors, subdivided by whether the dictator was a winner or a loser, and for observers. Numbers of observations in are shown in parentheses. The 'tests' are explained in the main text. Significance levels are indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Aggregate values are averages across all payoff pairs.

for each participant, their average transfers made under strategic choice with those made under non-strategic choice, and similarly, their transfers under strategic luck with those under non-strategic luck.

The t-tests examine whether these within-subject differences in transfer amounts are statistically significant. Essentially, for each participant, the average transfer in one condition (e.g., strategic choice) in block 1 is compared to the average transfer in the corresponding condition (e.g., non-strategic choice) in Block 2. The analysis then tests whether the average of these differences across all participants is different from zero.

The null hypotheses for these tests state that there is no difference in dictators' allocations between the two conditions. In relation to the 20-0 payoff pair, the null hypotheses are that participants behave in the same way regardless of whether the inequality resulted from strategic luck or non-strategic luck. In relation to the 20-5 and 5-0 payoff pairs, the null hypotheses are that participants behave the same regardless of whether the inequality resulted from strategic choice or non-strategic choice.

For Block 1, there are 900 observations for each inequality level (20–0, 20–5, 5–0). Observations of tests in Block 1 are 90 (except for losers in the game 20–0 which are 88 and losers in game 20–5 which are 89). Observations for Block 2 vary and are reported in parentheses.

Significant results are indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Aggregate values are averages across all payoff pairs.

Block 1: It can be seen in Table 2.6.1 that winners allocate around 89% to winners while losers allocate around 29%-30% of total payoff to winners. Observers allocate around 65% to winners. This shows substantial differences between dictators' redistribution preferences in the three situations.

To formally test the hypothesis related to block 1, we need to run some t-tests. Before reporting the results, it is important to explain the method used for the t-tests, the results of which are reported in the "Test" columns. Each Game Player participates in 30 rounds, with each set of 10 rounds corresponding to one of three payoff pairs: 20–0, 20–5, or 5–0. In every round, participants redistribute initial incomes between themselves and the recipient in two scenarios: strategic choice and strategic luck.

To perform valid t-tests, we calculate an individual-level index that captures preferences for redistribution. In each round, we initially calculate differences between the amounts allocated to winners under strategic luck and strategic choice. Then, we take an average for all related rounds. For example, in the 20–0 payoff pair, we calculate differences between strategic luck and strategic choice for each of the 10 rounds. To obtain the index for the t-test, we take the average of these round-level differences at the individual level. For the aggregate level, the index is similarly calculated, but by averaging the differences across all 30 rounds for each participant.

In all cases, whether for individual payoff pairs or the aggregate level, there are 90 observations for the t-tests, as there are 90 Game Players across the sessions, each contributing one index value. This index reflects each Game Player's preferences for redistribution. The t-tests then compare these 90 index values across strategic luck and strategic choice scenarios for each payoff pair, as well as for the aggregate level. The tests are carried out separately for cases in which the dictator is a winner and for those in which they are a loser. Based on the explanation about the tests, the results are as follows.

For Game Players (the left and middle part of Table 2.6.1 for Block 1), the relevant tests for winners compare strategic luck and strategic choice for payoff pairs 20–0, 20–5 and 5–0 separately and at the aggregate level. There are corresponding tests for losers. The table shows that none of these tests is at the 5% level[19].Therefore, we can conclude as follows for H1a:

Result 1. Contrary to Hypothesis H1a, the difference in redistribution by Game Players between outcomes resulting from strategic luck and those resulting from strategic choice is not statistically significant at the 5% level.

We carry out similar tests for Observers (the right part of the Table 2.6.1 for Block 1). The index is calculated for Observers in the same way as for Game Players, i.e., by calculating differences in Observers' allocations between strategic luck and strategic choice averaged at the individual level (Since observers did not play the games, their behaviour as dictators cannot be classified as that of 'winners' or 'losers'). As for Game Players, we run a separate test for each payoff pair, together with an aggregate tests. As can be seen in Table 2.6.1, none of the t-tests is significant. Therefore, we can conclude as follows for H1b:

Result 2. Contrary to Hypothesis H1b, the difference in redistribution by Observers between outcomes resulting from strategic luck and those resulting from strategic choice is not statistically significant at the %5 level.

Block2: Now we will report the results from the investment task, based on the experiment of Cappelen et al. (2013). Unlike Block 1, both choice and luck situations are not present for each payoff pair. Specifically, the 20–0 winners occur when lucky dictators meet unlucky recipients, and losers vice versa. The 20–5 winners arise when lucky dictators meet safe recipients, and losers vice versa. Finally, the 5–0 winners result when safe dictators meet unlucky recipients, and losers vice versa. Consequently, the number of observations differs across situations. Once again, we observe substantial

[19]In the 20–5 payoff pair, in which winners allocate 87.2% to winners in strategic choice and 89.1% in strategic luck, the difference in winners' behaviour is significance at the 10% level.

differences between the allocations of winners, losers and observers. Winners allocate around 88%-93% to winners, losers allocate around 27%-34% to winners, and observers allocate around 72%-73% to winners.

In the "Test" columns, we test the differences between non-strategic choice and non-strategic luck and their corresponding strategic choice or strategic luck situations from Block 1 for the same participants. Since the same participants completed both blocks, we calculate an individual-level index to reflect redistribution preferences. This index is the average of all differences in redistribution between non-strategic choice or non-strategic luck and the corresponding strategic choice and strategic luck situations for each participant. For instance, the test for winners in the 20–0 condition of Block 2, which reports 0.350 with 71 observations, is computed as follows: First, the difference in redistribution between non-strategic luck and strategic luck in Block 1 for the same participants is calculated (e.g., 20–0 winners in Block 1 for Game Players). This is then divided by the number of observations. This is the same for all other situations. It is important to note that Game Players in Block 1 are Investors in Block 2. This index serves as a measure of participants' willingness to redistribute when faced with strategic choice or strategic luck compared respectively to non-strategic choice or non-strategic luck. Due to the overlap of certain choice situations, calculating this index at the aggregate level is not feasible.

For Active Players (Game Players in Block 1 and Investors in Block 2), the relevant tests (the left part of Table 2.6.1 for Block 2) for winners compare strategic choice and non-strategic choice for payoff pairs 20–5 and 5–0 separately. The table shows that none of these tests is significant at the 5% level. There are corresponding tests for losers (the middle part of Table 2.6.1 for Block 2). The table shows that the test for losers for payoff pairs 20–5 is not significant but for payoff pairs 5–0 is significant at the 5% level. Therefore, we can conclude as follows for H2a:

Result 3. Related to Hypothesis H2a, Active Players' redistribution following strategic choice is not significantly different at the 5% level from redistribution following non-strategic choice in three out of four pair payoffs, and is significantly different in one out of four pair payoffs.

For Active Players (Game Players in Block 1 and Investors in Block 2), the relevant tests (the left part of Table 2.6.1 for Block 2) for winners compare strategic luck and non-strategic luck for payoff pairs 20–0. The table shows that the test is not significant at the 5% level. There is corresponding test for losers (the middle part of Table 2.6.1 for Block 2). The table shows that the test for losers for payoff pairs 20–0 is not significant at the 5% level. Therefore, we can conclude as follows for H3a:

Result 4. Contrary to Hypothesis H3a, Active Players' redistribution following strategic luck is not significantly different at the 5% level from redistribution following non-strategic luck.

For Observers (the right part of Table 2.6.1 for Block 2), the relevant tests compare strategic choice and non-strategic choice for payoff pairs 20–5 and 5–0 separately. The table shows that none of these tests is significant at the 5% level. Therefore, we can conclude as follows for H2b:

Result 5. Consistently with Hypothesis H2b, Observers redistribute differently when the outcome is based on a strategic choice compared to a non-strategic choice.

For Observers (the right part of Table 2.6.1 for Block 2), the relevant tests compare strategic luck and non-strategic luck for payoff pairs 20–0. The table shows the test is significant at the 5% level. Therefore, we can conclude as follows for H3b:

Result 6. Consistently with Hypothesis H3b, Observers redistribute differently when the outcome is based on a strategic luck compared to a non-strategic luck.

The last hypothesis H4 is related to the comparison of option luck and brute luck. To remind the part of experiment related to this, in the investment task, we have four rounds in which risky option incomes are 20–0 (one of them based on random). However, incomes of safe option in 3 rounds are 5 and in one round is 0. In those three rounds, the game is similar to Cappelen et al. (2013) and we call them option luck but we call the other round with safe option of zero brute luck.

For Investors and Observers (the bottom part of Table 2.6.1 for Block 2), the relevant tests compare option luck and brute luck for payoff pairs 20–0[20]. The table shows the tests for Investors and Observers are not significant at the 5% level. Therefore, we can conclude as follows for H4:

Result 7. Contrary to Hypothesis H4, participants (both Investors related to H4a and Observers related to H4b) do not redistribute differently when the outcome is based on an option luck compared to a brute luck.

Apart from the hypothesis tests, Table 2.6.1 provides important insights. Block 1 games with 20–0 and 5–0 payoff pairs, Game Players who are winners can only give, while losers can only take. In the 20–5 game, in contrast, both winners and losers are able to choose between giving and taking. Some papers in the literature (e.g., Bardsley et al., 2010) conclude that individuals' preferences over final allocations vary according to whether those allocations are reached by giving or taking. We test this here by comparing the behaviour of winners when just giving, just taking or both are possible.

In the 20–0 game, where winners can only give, they give approximately 10% of the pie (total payoffs of a pair). In contrast, in the 20–5 game, where both giving and

[20] In brute luck, the safe option is zero. So, the only possible pair payoffs is 20-0. In the experiment, 89 out of 90 Investors chose risky option since safe option gives 0 payoffs and just one person chose safe option.

taking are possible, winners take around 6%–7%. A similar pattern is observed for the 5–0 and 20–5 games, where winners give 8% in the former but take 6% in the latter. However, the final share allocated to the winners is almost identical across all games. For losers, preferences over final allocations are also not shaped by the choice set. In the 20–0, 20–5, 5–0 games, where losers can only take, they take an average of 33% and 28%–29% and 28%–29% respectively. This pattern indicates that, conditional on a given role as winner, loser or observer, participants have broadly consistent views about the proportion of the total payoff to be allocated to the winner, regardless of whether they have to give or take to achieve that outcome.

However, there is a notable difference between the redistribution preferences of winners and losers. In all games, the share allocated to winners when they are dictators ranges from 86% to 92%, while the share allocated to winners when losers are dictators ranges from 28% to 33%. These findings demonstrate that initial relative incomes (which determine whether the dictator is a winner or loser) play a significant role in shaping redistribution preferences. A related (but less strong) effect can be seen also for observers where, despite the fact that observers have no self-interested motivation to favour winners, the share allocated to winners is 65% to 75%, not 50%.

2.6.2 Regression Analysis

In the previous section, we tested hypotheses using simple methods. In this section, we use a regression model to check the robustness of our findings and to investigate additional explanatory factors.

In addition to choice and luck, there are other variables that might affect individuals' redistribution preferences including the total incomes (the whole pie), relative initial incomes (individual's incomes before redistribution relative to the whole pie) and whether they are winner, loser (if they are active players) or observer. We define the following variables for any given dictator in the redistribution phase of a Block 1 or Block 2 task:

- x_w is the initial income of the winner (i.e., the player in the matched pair who has the higher initial income).
- x_l is the initial income of the loser (i.e., the player in the matched pair who has the lower initial income).
- y_w is the final income of the winner, as allocated by the dictator.
- y_l is the final income of the loser, as allocated by the dictator.

- X is the sum of the initial incomes of the winner and loser i.e., $x_w + x_l$. This is equivalent of final earnings to the sum of the the final incomes of the two players i.e., $y_w + y_l$.

Since redistributive preferences are clearly different for winners, losers and observers, we estimate separate regression models for each role, separately for Block 1 and Block 2. Each model has the following form:

$$y = \alpha + \beta_1 \cdot x_1 + \beta_2 \cdot x_2 + \beta_3 \cdot x_3$$

where:

- y: Share allocated to the winner by the dictator
- α: Intercept of the model
- x_1: 1 if incomes are based on choice, 0 if incomes are based on luck (depending on the dataset, this can be a comparison between strategic choice and strategic luck or between non-strategic choice and non-strategic luck). We will sometimes refer to this variable as 'choice'.
- x_2: X, the total of winners' and losers' incomes , before and after redistribution.
- x_3: $\frac{x_w}{X}$, the share of total income held by winners after the initial income-generating phase, where x_w is the sum of winners' incomes and X is the total income of the pairs.

The results for Block 1 are reported in Table 2.6.2.

The results indicate that the source of inequality (luck or choice) has no significant effect on redistribution for winners, losers or observers, as the coefficient x_1 i.e., choice is not significant in any of the regression models. The coefficient x_2 (total payoff in each pair i.e., X) is significant and negative in the models for losers and observers (when selfish dictators are included). Furthermore, the coefficients on x_3 reveal that among active players, redistribution is strongly influenced by relative income shares. Active players who are winners tend to retain a greater share as their relative income increases, whereas losers are significantly less generous toward winners who already hold a larger proportion of the joint income. However, this variable for observers is not significant.

In relation to our research questions, the important message from this table is the insignificance of luck and choice on players' redistribution preferences. The significant effects of X and $\frac{x_w}{X}$ on transfers are interesting and might give clues for further investigation in this area.

Table 2.6.2: OLS Regression of Share Given on Treatment and Role Variables in Block 1

	Sample						
	Incl. Self-interest				**Excl. Self-interest**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
x1 (choice)	0.0014	0.0014	0.0014	0.0014	-0.0020	-0.0015	-0.0021
	(0.0033)	(0.0033)	(0.0033)	(0.0033)	(0.0034)	(0.0034)	(0.0034)
x2 (X)		-0.0009	-0.0024***	-0.0017***		-0.0003	-0.0002
		(0.0006)	(0.0006)	(0.0006)		(0.0006)	(0.0007)
x3 ($\frac{x_w}{X}$)		0.3560***	-0.4740***	-0.0144		0.0989***	-0.0362
		(0.0271)	(0.0414)	(0.0231)		(0.0206)	(0.0305)
Constant	0.6590***	0.5700***	0.7830***	0.6930***	0.6320***	0.6210***	0.6410***
	(0.0107)	(0.0247)	(0.0194)	(0.0166)	(0.0109)	(0.0200)	(0.0201)
Observations	10800	10800	10800	10800	6720	6720	6720
R-squared	0.0000	0.2200	0.2740	0.0021	0.0000	0.0252	0.0030
F-test (p-value)	0.6789	0.0000	0.0000	0.0911	0.5510	0.0000	0.5590

Standard errors in parentheses. Robust standard errors clustered by participant.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regressions (1) to (4) use the full dataset, while regressions (5) to (7) exclude selfish dictators (i.e., active players who took the entire pie). Regression (1) is the baseline, testing only the effect of choice versus luck on redistribution. Regressions (2) to (4) focus separately on dictators who were winners, losers, and observers (outside the pair), respectively. Regressions (5) to (7) repeat the same structure as (1) to (3), but exclude selfish dictators from the analysis. Observers cannot be selfish by definition since they cannot take money for themselves and just allocate money between the pairs.

Regression models 1 to 4 use data from all dictators. In models 5 to 7, dictators who are active players who always allocate the whole of the initial payoffs to themselves are excluded. We use this approach to make sure that the insignificance of luck or choice on players' preferences is not because many players want to take all the initial payoffs for themselves. When selfish dictators are excluded, the regression models still report insignificant results for luck and choice.

Table 2.6.3 shows the results for the same series of regressions for Block 2.

Table 2.6.3: OLS Regression of Share Given on Treatment and Role Variables in Block 2

	Incl. Self-interest				Excl. Self-interest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
x1 (choice)	-0.0024	0.0108	-0.0222	0.0105	0.0250	0.0108	0.0086
	(0.0148)	(0.0182)	(0.0146)	(0.0174)	(0.0171)	(0.0172)	(0.0169)
x2 (X)		0.0002	-0.0026**	0.0015		-0.0041***	-0.0045***
		(0.0015)	(0.0013)	(0.0019)		(0.0012)	(0.0012)
x3 ($\frac{x_w}{X}$)		0.3370***	-0.4950***	0.1150***		0.0870***	-0.1360***
		(0.0299)	(0.0418)	(0.0306)		(0.0246)	(0.0324)
Constant	0.6670***	0.5750***	0.8370***	0.5800***	0.6660***	0.7370***	0.7840***
	(0.0190)	(0.0479)	(0.0339)	(0.0513)	(0.0173)	(0.0339)	(0.0332)
Observations	1080	1080	1080	1080	699	699	699
R-squared	0.0000	0.1662	0.3552	0.0261	0.0030	0.0404	0.0774
F-test (p-value)	0.8724	0.0000	0.0000	0.0028	0.1478	0.0000	0.0000

Standard errors in parentheses. Robust standard errors clustered by participant.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regressions (1) to (4) use the full dataset, while regressions (5) to (7) exclude selfish dictators (i.e., active players who took the entire pie). Regression (1) is the baseline, testing only the effect of choice versus luck on redistribution. Regressions (2) to (4) focus separately on dictators who were winners, losers, and observers (outside the pair), respectively. Regressions (5) to (7) repeat the same structure as (1) to (3), but exclude selfish dictators from the analysis. Observers cannot be selfish by definition since they cannot take money for themselves and just allocate money between the pairs.

In relation to our research questions, the main message from this table is the same as the previous one for Block 1: the insignificance of luck and choice on players' redistribution preference. Again, X and $\frac{x_w}{X}$ have significant effects in some situations that may give clues for further investigation.

2.7 Conclusion

In the literature on inequality and fairness within experimental economics, the relationship between individuals' preferences for redistribution and the effects of choice and luck has been widely explored. However, much of the existing research focuses on non-strategic environments, where decisions are based solely on individual tasks. Furthermore, luck is typically conceptualised as a lottery which does not cover factors beyond individuals' control that are not based on lotteries. To date, there has been little attention to strategic environments, in which individuals' outcomes are determined not only by their own choices but also by the choices of others. This paper aims to build up on the literature by considering strategic context, where outcomes are influenced by both individual and others' decisions. Additionally, we redefine luck as a variable that lies beyond the individual's control, determined by the choices of others, rather than a random lottery. This way, luck is more general concept. Given the results of the literature that luck and choice affect individuals' preferences for redistribution, we aim to improve the designs in the literature and test the factors in the improved design.

The primary research question addressed in this study is the impact of strategic choice and strategic luck on individuals' preferences for redistribution. We wish also to compare the effects of strategic choice and strategic luck with those of non-strategic choice and luck. To examine this, we designed an experiment consisting of two blocks. In Block 1, we introduced a novel strategic environment based on a pure coordination game (PC), followed by a redistribution phase across 30 rounds. In Block 2, we used the experiment from Cappelen et al. (2013), with adjustments to standardise it with Block 1. The purpose of Block 2 was to elicit individuals' preferences for redistribution based on non-strategic choice and non-strategic luck, allowing us to compare them with preferences shaped by strategic luck and strategic choice.

According to our analysis, participants do not exhibit different redistribution preferences based on strategic luck and strategic choice at the 5% significance level. To explore potential reasons for the lack of significant results, we compare our findings with those of Cappelen et al. (2013). Table 2.7.1 presents Cappelen et al. (2013)'s results using our measurement approach (i.e., the share allocated to winners in each pair) for comparability. 'Stakeholder' and 'Spectator' correspond with our concepts of 'active player' and 'observer' respectively.

Table 2.7.1: Basic Descriptive Statistics for Cappelen et al. (2013).

		Stakeholders						Spectators		
		Winners			Losers			–		
Payoffs Pair	Matched Pairs	Choice	Luck	Test	Choice	Luck	Test	Choice	Luck	Test
800–0	Risky Winner, Risky Loser		.887			.309			.671	
		(–)	(92)	(–)	(–)	(92)	(–)	(–)	(95)	(–)
800–200	Risky Winner - Safe(200)	.891		0.968	.500		0.285	.700		0.826
		(3)	(–)	(–)	(3)	(–)	(–)	(4)	(–)	(99)
800–300	Risky Winner - Safe(300)	0.790		0.045**	.323		0.876	.760		0.260
		(13)	(–)	(–)	(13)	(–)	(–)	(11)	(–)	(106)
800–400	Risky Winner - Safe(400)	1.000		0.096*	.194		0.361	.592		0.362
		(6)	(–)	(–)	(6)	(–)	(–)	(9)	(–)	(104)
Aggregate	Risky Winner - Safe(Aggregate)	0.861	.887	0.488	0.312	.309	0.9710	.687	.671	0.773
		(22)	(92)	(114)	(22)	(92)	(114)	(24)	(95)	(156)
0–200	Risky Loser - Safe(200)	.925		0.6169	.500		0.174	.625		0.520
		(5)	(–)	(–)	(5)	(–)	(–)	(8)	(–)	(103)
0–300	Risky Loser - Safe(300)	.861		0.525	.412		0.149	.762		0.8182
		(21)	(–)	(–)	(21)	(–)	(–)	(20)	(–)	(115)
0–400	Risky Loser - Safe(400)	.898		0.8623	.562		0.029**	.784		0.872
		(8)	(–)	(–)	(8)	(–)	(–)	(9)	(–)	(104)
Aggregate	Risky Loser - Safe(Aggregate)	.879	.887	0.803	.460	.309	0.014 **	.738	.671	0.194
		(34)	(92)	(126)	(34)	(92)	(126)	(37)	(95)	(132)

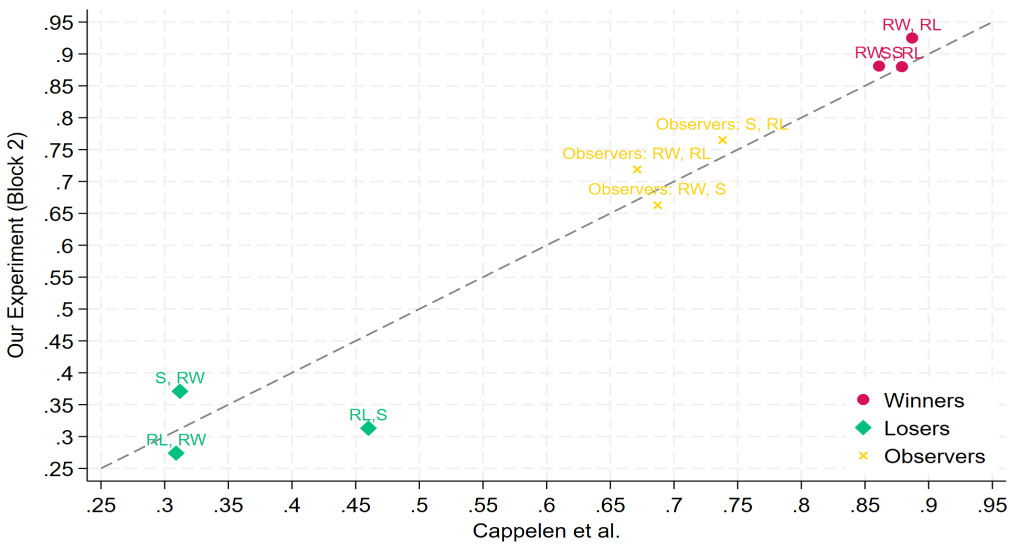
Notes: This table reports the proportion of total payoffs allocated to winners by dictators in different tasks. Numbers of observations are shown in parentheses. ”Winners” and ”Losers” indicate groups based on dictators’ outcomes: In winners, dictators have higher outcomes than their recipients, while in losers, dictators have lower outcomes. For spectators, these categories are not applicable and just the inequality level is important.
The ”Test” column shows p-values from t-tests comparing luck and choice at the individual level. Each initial payoff pair for choice is compared with the 800-0 initial payoff pair for luck. E.g., winners’ behaviour when the initial payoff pair is 800-200 and due to choice (an average allocation of 0.891 to winners) is compared with winners’ behaviour when the initial payoff pair is 800-0 and due to choice (an average allocation of 0.887); the p-value of the t-test for that comparison in 0.887.
Significant results are indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Aggregate values are averages across all payoff pairs.

As shown in Table 2.7.1, initial payoff pairs for comparisons between choice and luck situations are not identical. However, given the experimental design, this is the most suitable test we can conduct. The average share allocated to winners does not differ greatly across different payoff pairs. Among stakeholders, three out of four overall tests between choice and luck show no significant differences. The only exception is for dictators with lower initial incomes and in pairs in which one of the players is unlucky. In this pair, dictators allocate 46% to winners after choice inequality and 31% after luck inequality, a difference significant at the 5% level. For spectators, both of the tests that refer to overall values indicate no significant differences.

Comparing Table 2.7.1 with Table 2.6.1, the average share allocated to winners by active participants (i.e., game players and investors in our experiment, and stakeholders in Cappelen et al. (2013)’s experiment) is quite similar. Both tables reveal a consistent pattern and closely aligned average shares, broadly in line with the results of our t-tests, which show no significant differences in most cases.

Figure 2.7.1 gives a better perspective on this similarity. The horizontal axis measures allocations to winners in different tasks in Capellen et al.'s experiment. The vertical axis measures corresponding allocations in the investment tasks in our experiment. The marked points show matched pairs of observations from the two experiments.

Figure 2.7.1: Comparison of Winners, Losers, and Observers in the Investment task Experiment with Cappelen et al. (2013)



Note: Dots represent the share assigned to the winner in each pair. In each pair, first player is the dictator and the second one is recipient. Winners mean that the dictator is winner, losers mean the dictator is loser and observers mean the dictator is an observer who is not in the pair. RW = Risky Winner, RL = Risky Loser, S = Safe. The 45-degree dashed line indicates equal values between our experiment and Cappelen et al. (2013)

All but one of these points, in figure 2.7.1, are close to the 45 degree, showing similarity between the data from the two experiments[21]. We conclude that, the source of insignificant results in our paper is not a difference between our data and those of Cappelen et al. (2013).

One interesting aspect of this similarity is related to the observers. Our measurements of Cappelen et al. (2013)'s data show that observers give between 69% and 74% of of total payoffs to winners. This tendency for observers to favour winners, found in both experiments, shows that observers respect initial incomes.

Given the similarity between the two data sets, it is surprising that we find little evidence that active players' redistributive preferences differ between choice and luck scenarios, while Cappelen et al. (2013) estimate that around 30% of their participants have 'choice egalitarian' fairness views (i.e., think it fair to equalise

[21]The only exception is for the pair of "RL,S" which refers to the risky losers as dictator and safe as recipeint and in our experiment the share of winner is 0.31 but this in Cappelen et al. (2013) is 0.46.

inequalities due to luck, but not to redistribute if inequalities are due to choice). We conjecture that this contrast reflects differences in methods of data analysis. Most of the papers in the literature, including Cappelen et al. (2007, 2013), are based on the assumption that individuals are of different types, classified by their fairness views, and that they cannot switch type between tasks (Conte and Moffatt, 2014). However, we use a simple direct parametric test in which we test redistribution coming from (strategic or non-strategic) choice with that of (strategic or non-strategic) luck. We think simple direct statistical test might be more suitable for addressing the research question of differences between redistribution after luck and choice. This simpler approach does not require assumptions about individuals' types and is not greatly sensitive to econometrics method. In the more complicated econometric approach used by Cappelen et al. (2007), assumptions about types and details of the econometric method play important roles in determining results (Conte and Moffatt, 2014).

The next step for future research is to investigate why this divergence in results occurs under different econometric approaches. Specifically, future work should aim to understand why redistribution preferences based on luck and choice (both in strategic and non-strategic contexts) are so sensitive to the type of econometric analysis employed.

Chapter 3

An Experimental Investigation of Players' Choice Strategies in Pure Coordination Games

A key puzzle in coordination game literature is understanding how decision-makers achieve success in pure coordination games. In standard game-theoretical models, strategies do not have labels. Experimental research findings show that in pure coordination games with labels, players coordinate significantly better than they would with random choice. This improved performance arises because some labels are chosen more frequently than others, which is unexplained by game theory. This paper investigates what makes labels more salient. We conducted an experiment where participants played 30 rounds of pure coordination games using labels from 10 categories (e.g., Actors and Fruits). These labels were selected based on high rankings on specialised websites. Coordination success exceeded random chance. While label frequencies on websites and in Google searches showed no significant effects, label frequency in English-language books significantly influenced success.

3.1 Introduction

We ask whether focality is historically and culturally grounded; how do individuals coordinate when communication is not possible and there are no payoff-related cues? This is the central question in the study of pure coordination games where all equilibria yield the same payoffs and success depends entirely on mutual expectations. Such games are not only theoretically interesting but also practically relevant in environments ranging from social conventions and traffic systems to language use and online interactions. Understanding the mechanisms through which coordination occurs in these games is crucial for economics, psychology, and the design of institutions.

Classical game theory, in its standard form, offers no resolution to coordination in symmetric games with multiple equilibria: it predicts that players should randomise among options. However, experimental evidence shows otherwise; Participants consistently perform better than random choice would suggest, particularly in pure coordination games[1] (Mehta et al., 1994*b*; Bacharach and Bernasconi, 1997; Isoni et al., 2013). This discrepancy has led scholars to explore the role of focal points, a term introduced by Schelling (1960) to describe salient features or labels that individuals naturally converge on without prior agreement. These features act as coordination devices, helping players to anticipate one another's choices.

While the existence of focal points is now well documented, an important question remains: What determines the salience of a label? Why do some words, images, or references become focal while others do not, especially in the absence of any payoff-related cues?

Previous research has explored how individuals select focal points by studying the heuristics and cognitive rules they use in coordination games, both unsystematically and in more structured frameworks. Notably, Hargreaves Heap et al. (2017) provide a systematic classification of such rules. They find that the most commonly applied rules in pure coordination games with 'restricted options' (i.e., a pre-specified finite set of alternative labels) are choosing the best-known label within a category (Typicality I), selecting the item "most frequently mentioned" (Typicality II) and "Choose the top of the most natural ranking (Prominence)". This is in line with cognitive fluency theory (Alter and Oppenheimer, 2009), shared exposure theory (Clark, 1996), availability heuristics (Tversky and Kahneman, 1973) and normative theory of focal points (Sugden, 1995).

However, existing literature has not yet clearly distinguished between two features

[1]Once there is a conflict of interest (e.g. the battle of the sexes game which is a coordination game with symmetry between the players) the coordination rate is lower.

of these rules: long-standing cultural familiarity (e.g., repeated exposure over time, best football player ever) and recent prominence (e.g., online search frequency, best football player now). The reason this distinction is important is considering the theory of cultural entrenchment (Mesoudi, 2011) which argus that ideas transmitted across generations shape more stable and widely shared forms of cognition.

We investigate the significance of this distinction by examining whether all forms of familiarity are equally effective. In particular, we explore the hypothesis that deep, culturally entrenched knowledge (reflected in the frequency of labels in books) is more likely to generate shared mental representations than more recent or domain-specific prominence.

To evaluate this, we systematically compare three sources of salience, each representing a different temporal and cognitive pathway:

(i) frequency in books, which captures long-term cultural exposure;

(ii) search engine trends that capture the recent exposure;

(iii) recognisability from specialised websites (such as IMDb), which mostly reflect the prominence rule ("Choose the top of the most natural ranking").

To explore this, we conducted a lab experiment in which participants played 30 rounds of pure coordination games. In each round, players were presented with two labels drawn from ten predefined categories (e.g. cities, fruits). The labels were drawn randomly from rankings on specialised websites (domain-specific recognisability). This design allowed us to assess whether different forms of familiarity (long-lasting, recent, and domain-specific) differentially predict coordination success, thereby testing the explanatory power of each source in shaping salience.

Our results show that participants consistently coordinate above random levels when choosing between labelled options. The tests of three plausible indicators of salience reveal that individuals are more likely to coordinate on labels that are more frequently used in books, while recent online popularity and domain-specific rankings showed comparatively weaker explanatory power.

Our study contributes to the literature in several key ways. It introduces a novel approach to studying focal points by systematically comparing different forms of familiarity (long-lasting, recent, and domain-specific) as predictors of coordination success. Furthermore, it tests whether long-term cultural exposure (captured via frequency in books) is a more robust driver of salience than recent online popularity or domain-specific visibility.

The following content of the chapter is arranged as follows: In Section 3.2, we will introduce the seminal studies related to our research questions. In Section 3.3 the details of our experimental design are explained. Our hypotheses will be mentioned in Section 3.4. Section 3.5 reports data analysis and explain the results related to the hypotheses. Finally, Section 3.6 is the conclusion.

3.2 Literature Review

A pure coordination game is a symmetric 2-player game in which players receive the highest payoffs when they select the same strategy, and a lower payoff when they choose different strategies. The version "with labels" refers to the presence of arbitrary identifiers attached to strategies (such as "Label 1" and "Label 2"), which may influence players' choices despite being payoff-irrelevant.

Formally, a 2×2 pure coordination game with labels can be represented in strategic (normal) form as shown in Table 3.2.1. Each player chooses one of two labelled strategies, and both receive the same payoff depending on whether their choices match:

Table 3.2.1: Strategic form of a 2×2 pure coordination game

	Label 1	**Label 2**
Label 1	$(x,\ x)$	$(y,\ y)$
Label 2	$(y,\ y)$	$(x,\ x)$

In table 3.2.1, both players have the same strategy set $A = \{\text{Label 1}, \text{Label 2}\}$. The game satisfies the following conditions:

- **Symmetry:** The payoffs are identical for both players at each outcome.
- **Coordination structure:** Players receive the higher payoff x when they coordinate on the same label, and the lower payoff y when they fail to coordinate, where $x > y$.

Thus, the game captures the essential property of pure coordination: players are incentivised solely to match their actions, and the labels (while payoff-irrelevant) may serve as focal points for equilibrium selection.

According to game theoretical modelling, the game has two pure-strategy Nash equilibria, at $(\text{Label 1}, \text{Label 1})$ and $(\text{Label 2}, \text{Label 2})$, each yielding the higher payoff pair (x, x) through mutual coordination on the same action. In addition to these, the game admits a symmetric mixed-strategy equilibrium in which both players randomise over their strategies with equal probability, which yields the payoff of $(0.5x + 0.5y, 0.5x + 0.5y)$. While the pure-strategy equilibria are Pareto optimal (Mas-Colell et al., 1995), the mixed-strategy equilibrium represents an equilibrium outcome (which is Pareto dominated by the pure strategy equilibria) in which players are indifferent between their strategies due to the symmetric structure of the game.

However, Schelling (1960) proposes that in games with multiple equilibria, individuals often rely on focal points which are salient labels or features that guide mutual expectations. These focal points may arise from culturally shared associations, conventions, or prominence derived from language, geography, or social norms. Mehta et al. (1994*b*,*a*) run some experiments to test Schelling's theoretical model of focal points. Participants are asked to solve 20 coordination problems without communication, where success depends on matching another player's answer. Their results show that participants often select the same obvious options (such as "red" for colour or "apple" for fruit) indicating the psychological power of primary salience[2]. By comparing these responses with answers given in an equivalent non-strategic setting, the authors identify behavioural rules such as "closeness", "accession", and "equality" that appear to guide focal reasoning. These findings provide experimental support for Schelling's insights into how individuals identify focal points in games with multiple equilibria.

Sugden (1995) offers a theoretical foundation for interpreting these findings. He introduces the idea of the "existential game," which separates the formal game structure (players, strategies, payoffs) from the descriptions or labels that players use to conceptualize their choices. The paper shows that when players share a mental model of the game through common labels, these labels create asymmetries in otherwise symmetric games, making coordination possible. The model supports experimental evidence by formalising how salience can be both subjective (depending on individual framing) and inter subjectively rational (when players have common knowledge about language or category structure). In particular, labels that are "most frequently mentioned" gain prominence as focal points because their popularity creates mutual expectations about others' choices, reinforcing successful coordination possibilities.

Bacharach and Bernasconi (1997) introduce the Variable Frame Theory, which provides a cognitive foundation for understanding how players identify focal points in coordination games. In their model, a frame is a shared mental perspective that categorizes the available options in terms of concepts such as colour, shape, typicality, or uniqueness. Crucially, frames are not strategically chosen by individuals but are assumed to be common knowledge among players. Once a frame is established, players apply standard game-theoretic reasoning (such as payoff

[2] A label is said to be primarily salient if it is likely to be chosen in a non-strategic context simply because it is more prominent or accessible than alternatives. A label is secondarily salient if it is the best response to the behaviour of players who act on primary salience. See Mehta et al. (1994*b*) for more details.

dominance) within that frame to select strategies. Within a given frame, items that are rare or distinctive (e.g., the only red circle among many white ones) are predicted by the theory to be focal points. Thus, salience is not a fixed property of an object but emerges from its distinctiveness relative to a commonly understood frame. This theory enriches the focal point literature by showing how shared cognitive structures shape players' expectations and guide coordination.

Bardsley et al. (2010) compares some major theoretical approaches including the level-k theory[3] (Stahl and Wilson, 1995; Nagel, 1995), Cognitive Hierarchy Theory (CHT)[4] (Camerer et al., 2004*b*), and Team Reasoning Theory[5] (Sugden, 1993, 2000; Bacharach, 1999, 2006) to explain focal point behaviour in an experimental coordination setting. They find that team reasoning can explain high coordination rates in symmetric pure coordination games, while CHT and level-k theory are sometimes insufficient in explaining consistent focal behaviour when higher-level reasoning should negate simple salience. Their results indicate that in certain coordination contexts, participants are not merely guessing but are actively seeking mutual understanding. This suggests that salience is not necessarily a level-0 heuristic, but can be rationalised as part of cooperative or team-based reasoning. This view strengthens the theoretical legitimacy of salience-based coordination under appropriate conditions and supports the idea that players can engage in normative coordination without communication. Supporting evidence comes from Faillo et al. (2017), who demonstrate experimentally that reasoning as a team enhances coordination success in symmetric games.

Further insight is provided by Crawford et al. (2008) that experimentally investigate the limits of focal point power in coordination games by introducing minute payoff asymmetries in two quite different sets of games. Their results show that while salient labels can facilitate coordination when payoffs are symmetric, even small asymmetries can significantly undermine this effect, leading to sharp drops in coordination success. In the coordination "X–Y Game"[6], with asymmetric payoffs

[3]Level-k theory assumes that players are categorized into discrete levels of strategic reasoning. Level-0 players act non-strategically (e.g., randomly), Level-1 players best respond to Level-0 behavior, Level-2 players best respond to Level-1 behavior, and so on, with each player assuming others reason one level below themselves.

[4]Cognitive Hierarchy Theory (CHT) assumes that players differ in their levels of strategic reasoning: some play non-strategically (level-0), often modeled as random or simple behavior, while higher-level players best respond to a distribution of lower-level types rather than to a single fixed type.

[5]Team Reasoning Theory posits that players may reason as members of a team, choosing the strategy that would best achieve the group's objective if everyone reasoned similarly.

[6]The X–Y Game is a 2-player pure coordination game in which each player chooses either "X" or "Y." Players receive a positive payoff only if they both select the same label; mismatched choices yield zero. In symmetric treatments, both matching outcomes yield equal payoffs. In asymmetric treatments, one matching choice yields a slightly higher payoff for one player, creating a trade-off

(i.e., a battle of the sexes game in which the strategies are labelled 'X' and 'Y'), the observed behaviour is more consistent with level-k reasoning: players appear to best respond to expectations about less sophisticated opponents. In contrast, the "Pie Game"[7] provides stronger support for team reasoning: when payoff asymmetries are introduced, coordination deteriorates in ways that level-k models cannot explain, whereas team reasoning offers a more plausible account of player behaviour.

Isoni et al. (2013) further study the X-Y type of game but in a bargaining frame. They investigate whether relational cues (such as proximity or perceived ownership) can sustain focal-point coordination when mutual interest is uncertain or contested. They use tacit bargaining scenarios where coordination success depends on players avoiding overlapping claims. They introduce relational cues (e.g., proximity or ownership-like associations) and found that these cues do indeed function as focal points, especially when no clear efficiency-based solution exists. Yet their later work (Isoni et al., 2019) introduced payoff information asymmetries and demonstrated that when players are aware of potential conflicts of interest, their reliance on salience weakens. This shift occurs even if the salient label would lead to mutual benefit, suggesting that focal point reasoning is more fragile under perceived conflict of interests or inequality. In short, salience is dependent not just on label features, but also on the perceived social logic of the game.

Isoni et al. (2020) offer an important contribution to the literature on coordination and focal-point reasoning by disentangling two often conflated factors in Battle of the Sexes (BoS) games: conflict of interest and payoff inequality. While both elements are typically present in BoS, their study introduces the pizza night game to isolate the effects of payoff inequality alone. In this framed coordination setting, players face two equilibria with identical distributions of payoffs, but with a known asymmetry in who receives the higher payoff, thereby eliminating conflicting preferences over outcomes. Their findings show that although extreme payoff inequality dampens coordination somewhat, agreement rates in the pizza night game are significantly higher than in BoS games. This suggests that conflict of interest, rather than inequality per se, is the more disruptive factor in coordination.

The study most closely related to our experiment is Hargreaves Heap et al. (2017). They investigate how individuals coordinate in environments where there is a finite set of labels to choose from (restricted options treatment) and where there is not a finite

between label salience and payoff salience.

[7]The Pie Game is a 2-player coordination game in which players independently select one of three pie slices illustrated in a diagram. If both choose the same slice, they receive a payoff determined by that option; otherwise they receive nothing. Treatments vary in visual framing and payoff structure. In symmetric versions, matching on any slice yields equal payoffs; in asymmetric versions, one option benefits one player more, introducing conflict between fairness and payoff dominance.

set of labels to choose from (unrestricted options treatment). Their primary focus is on how people identify salient options for coordination when faced with varying contextual constraints. While the natural expectation is that the success rate (i.e., the frequency with which players coordinate) in the restricted options treatment is higher than in the unrestricted options treatment, their results show the opposite. To explain the reason of this unexpected result, they develop a method for eliciting the general rules (captured from experimental and broader psychology literature) that subjects use to determine salience, allowing for systematic identification of coordination heuristics across different choice sets.

The study comprises two experiments. In Experiment 1, each participant plays 16 coordination games, each defined by a different category such as flowers, car brands, or colours. In the unrestricted options treatment, participants are free to choose any label within the category (e.g., "Choose the same colour as the other person"). In the restricted options treatment, participants choose from five pre-selected labels, which were determined based on the most frequent responses in a pilot study. In Experiment 2, participants engage with the same 16 categories in two stages. In the first stage (naïve stage), they are asked to respond to non-strategic prompts under seven different rules (e.g., "Choose your favourite flower" or "Choose a random color"). In the second stage (strategic stage), participants are asked to guess what a randomly chosen participant from the first stage selected (e.g., "Choose the other person's favourite flower"). The blocks of categories are swapped between stages, and the sessions differ depending on whether the labels are drawn from an unrestricted options treatment or from the same restricted sets used in Experiment 1.

The above-mentioned rules include:

- *Favourite* ("Choose your favourite"), grounded in Mehta et al. (1994*b*), where coordination may occur through personal preferences that are assumed to be shared.
- *Odd-one-out* ("Choose the least similar"), originally discussed by Schelling (1960) and further developed as a rarity heuristic by Bacharach and Bernasconi (1997); Bacharach (2006).
- *Prominence* ("Choose the top of the most natural ranking"), linked to natural category hierarchies—e.g., Everest for mountains or Maradona for footballers.
- *Similarity* ("Choose the most similar"), based on feature overlap among alternatives (Tversky, 1977).
- *Typicality I* ("Choose the best known"), and *Typicality II* ("Choose the most frequently mentioned"), both of which reflect availability heuristics (Tversky and

Kahneman, 1973) and are consistent with Sugden (1995)'s normative theory of focal points.

- *proto-typicality* ("Choose an example") is an availability heuristic based on the idea that both the external world and our cognitive systems are highly structured (Rosch, 1977). The idea is that because the world is highly structured, we store and retrieve its most typical members (prototypes) more easily. This rule is applicable just for the unrestricted options treatment.

These heuristics suggest that coordination behaviour is often driven by subjective perceptions of salience (what stands out, feels familiar, or seems typical) rather than strict payoff dominance or rational choice. Hargreaves Heap et al. (2017) also examine how the application of these rules varies between restricted and unrestricted choice sets. In restricted choice sets, the most applied rules (from highest to lowest frequency) are typicality I best (best known), typicality II (most frequent), prominence, favourite, odd-one-out, similarity[8]. The authors find that there is evidence that different rules are triggered when options are restricted as compared with when they are unrestricted. Although individual rules, if used by both players, are more likely to induce coordination in restricted choice sets, players facing unrestricted choice sets are more likely to use the same rule. This contributes to the explanation of why we observe higher coordination when options are unrestricted.

Recent work by Gneezy and Rottenstreich (2024) examines the limits of salience as a coordination device, focusing on the tension between prominence and distinctiveness. Their study asks whether people are able to coordinate on options that are distinct but not widely known, particularly when competing options are highly prominent in popular culture or historical memory. Using a series of coordination experiments, they show that when all options are relatively obscure, participants often successfully coordinate on the most distinctive label (e.g. "Tianjin" among a list of unfamiliar city names). However, when some options are highly prominent (such as the names of U.S. presidents) they tend to overshadow more distinctive choices, even when those are better suited to coordination. The presence of prominent alternatives leads to systematic coordination failure, suggesting that salience derived from prominence can hinder the use of more effective focal points. These findings challenge the assumption that more familiar or prominent labels always facilitate coordination and point instead to a more nuanced understanding: while prominence can guide attention, it may also override cognitive strategies that rely on distinctiveness, leading individuals to converge on options that are mutually salient but ultimately unhelpful for coordination.

[8]Proto-typicality is used for unrestricted options condition since in the restricted options condition all of the items are examples within the category so that this type is not ranked for restricted options in the mentioned paper.

Overall, the above-mentioned studies highlight that the success of coordination depends not only on what labels are used but also on how they are perceived and contextualised by players. They also suggest that salience is more than a surface-level heuristic: it is deeply connected to social cognition, shared expectations, and the framing of the game environment. They have also explored the heuristics and cognitive rules individuals use in coordination games, both unsystematically and in more structured frameworks. Notably, Hargreaves Heap et al. (2017) provide a systematic classification of such rules. They find that the most commonly applied was choosing the best-known label within a category. While they did not operationalise this rule using domain-specific rankings, we interpret specialised websites (e.g., IMDb for films) as natural proxies for recognisability within interest-based communities. Their second most applied rule was typicality II, selecting the item "most frequently mentioned."

However, existing literature has not yet distinguished between two key sources of typicality: long-standing familiarity (e.g., repeated exposure over time) and recent exposure (e.g., online search frequency).

This distinction represents a gap in the coordination literature. Our aim is to address it by systematically comparing these two forms of familiarity (long-term and recent) alongside domain-specific recognisability which captures choosing top of the most natural ranking (prominence). Building on this framework, we examine whether the frequency with which labels are chosen in pure coordination games is more strongly shaped by long-term exposure (which may reflect deep-rooted cultural exposure), short-term salience, or visibility within natural domains. The next section describes how we design our experiment to test these effects.

3.3 Experimental design

We conducted an experiment[9] to address the research question. The details of the experiment are as follows.

[9]This experiment is part of a larger two-block design. Each session includes both blocks, and all participants complete both. In the Pure Coordination Block, participants play a coordination game followed by a redistribution stage. In the Investment Task Block, they complete an investment task, again followed by redistribution. Block order is random across sessions, and participants are only informed about the second block's content after they finish the first. Although both blocks follow the same structure (an income-generating task followed by redistribution) they are independent, as the nature of the income-generating tasks differs substantially. This paper focuses exclusively on the coordination block, which is independent and directly relevant to our research question. Although participants know that each coordination round is followed by redistribution, their aim in the coordination game is to maximise their earnings which happens by successful coordination. This ensures that choices in the coordination stage are not driven by redistribution incentives. This is because players' initial payoffs are always equal, so maximising own initial payoffs is equivalent to maximising the pie for redistribution.

3.3.1 Pure Coordination Game with labels

Our experimental design is based on the following formal structure. Participants are matched in pairs and asked to choose between two labelled options. The labels vary in salience across treatments to examine the extent to which focal points (inherent in labels) influence coordination success. Since the payoffs only depend on whether the players match, and not which label is chosen, any systematic preference for one equilibrium over the other can be attributed to label-driven coordination. This allows us to isolate the behavioural role of labels in facilitating coordination. We called participants Game Players[10]. Game Players are informed that they will be randomly matched with another Game Player in each round. They have two options and must select one. If their choices match, they earn a higher income than if they do not. The payoff structure varies across 30 rounds as follows:

- 10 rounds: £20 for matching choices, £0 for non-matching;
- 10 rounds: £20 for matching choices, £5 for non-matching;
- 10 rounds: £5 for matching choices, £0 for non-matching

The order of rounds is different in different sessions. So, if game players' choices match, they earn £20 or £5 depending on the round, and if their choices do not match, they earn either £5 or £0, depending on the round[11].

So far, the design is to make a pure coordination game in which each game player has two options that lead to some payoffs. What makes this experiment novel is the way we create labels for options in this pure coordination game.

In each round, all game players in the session play the same coordination game. In each round, there are two labels for the options which are not repeated in other rounds. The labels in each round are selected from 10 predefined categories as follows:

[10] In the whole experiment, we have two roles of Game player and Observer. Game Players participate in the pure coordination game and reallocation stage but Observers just participate in the reallocation stage. That is why in this paper, we just need the data of Game Players, becasue they just participate in the pure coordination game.

[11] The reason why we chose only three payoffs during all rounds is to allow the coordination games to be matched with 'investment' tasks faced in block 2, for the purposes of a different investigation. In block 1 with these three payoffs our possible inequalities are £20-£0, £20-£5 and £5-£0 which are matched with inequalities in investment game in block 2.

”Clothing” [12], ”Car”[13], ”Football”[14], ”City”[15], ”Actors”[16], ”Actresses”[17], ”Films”[18], ”Fruits”[19], ”Books”[20] and ”Sports”[21].

In each category, six items are used which are ranked between 4 and 9 in real-world popularity lists. These rankings are chosen to ensure that: (i) all items are recognisable, so participants can associate each label with shared content, a necessary condition for salience; and (ii) the tasks are neither too easy (which would lead to trivial focal points) nor too difficult (which would result in random choices), allowing for variation in coordination success. By avoiding the very top-ranked items, we reduce the risk of obvious focal points, and by excluding very low-ranked items, we ensure sufficient familiarity. The categories themselves, such as films, cities, or movies, are chosen to reflect domains that most participants are likely to know. Rankings within each category were drawn from publicly available lists (e.g., IMDb) to provide a neutral, hands-off method of label selection. Because label pairs are randomly combined in different games, this design allows us to explore whether and how far coordination success depends on the relative ranking and familiarity of labels obtained from external sources.

Items are selected randomly for labels as the following procedure:

- Rounds 1–10: Two items from each category are randomly selected for that round and removed from the list;
- Rounds 11–20: Two more items from each category are randomly selected and removed;
- Rounds 21–30: The final two items from each category are used.

In each round, after playing the game, game players know what they and their co-players chose and what their incomes are.

[12]The most valuable clothing companies in the world, by brand value 2021-2023 based on statista.com.

[13]Best-selling car brands 2022 in the United States based on statista.com.

[14]The best Premier League clubs according to the number of wins in all seasons from 1992 until April of 2024 based on premierleague.com.

[15]Leading city destinations worldwide in 2018, by number of overnight visitors in millions based on statista.com.

[16]Top 100 greatest actors of all time ranked according to their lifetime success (awards & nominations), along with their acting skills, versatility and role transformation by ChrisWalczyk55 listed in https://www.imdb.com/list.

[17]Top 50 greatest actors of all time ranked according to their acting abilities, their success, their versatility, and the difficulty of the roles they’ve played through out their careers by ChrisWalczyk55 listed in https://www.imdb.com/list.

[18]Top movies based on IMDB ranking in www.imdb.com.

[19]The healthiest fruits based on BBC food in www.bbcgoodfood.com.

[20]The greatest books of all time based on thegreatestbooks.org.

[21]The most popular sports for 16-24 year old based on participation size on caytoo.co.uk which is based on Sport England’s Active Lives Survey.

This experiment allows us to measure the extent of coordination between players in games with different label pairs. If players coordinate more successfully than if they had selected labels at random, we can investigate what makes some labels more salient than others. Since the labels are chosen and matched for each round of the game randomly (not based on the experimenters' selection) from diverse categories, we can appropriately investigate the reasons of selection of some more salient ones.

3.3.2 Implementation

We utilised oTree (Chen and Wickens, 2016) to conduct the experiment, implementing a total of 15 sessions with 12 participants in each in which 6 of them were randomly assigned as game players. So, our sample is made of 90 participants. The sessions took place in May and October 2024 at the Laboratory for Economic and Decision Research (LEDR) at the University of East Anglia. Participants received an average payment of £25.50 for approximately 100 minutes of participation for the whole experiment. Since the payment is related to the whole experiment not just the coordination game block, which is used for this paper, we do not have the average payment data solely for this block. Having said that, the average of participants' payoffs for the coordination game block was £15.53. Since the redistribution stage does not burn any incomes, average payoffs after the first stage (pure coordination game) remain unchanged after redistribution. The mentioned amounts include both active players (who played the pure coordination game) and observers (who did not play the game and just participated in a redistribution stage to redistribute incomes between the other two active players and earned a fixed £18 irrespective of their performance). In this paper, we just consider the participants who played the pure coordination game, so we just consider active players. The active players' average payoffs in this block was £13.05. In addition, each active player received a £10 participant fee for taking part in both blocks (£13.05 plus £10 participation fee) [22].

3.4 Hypothesis

There are two main hypotheses for this paper. The first one is related to our expectation following the literature that the successful rate of coordination for participants are significantly higher than the successful rate once participants choose their options randomly.

[22] Participants' average payoffs for the Investment task block is £15.66. Since one of the rounds in either of these two blocks in each lab session is selected randomly for payment, the payments from each block are not completely separable.

Hypothesis 1. The successful rate of participants is significantly higher than 50% which is the successful rate based on participants' random selection.

Since this expectation is well-documented by the literature, this is natural to expect our experiment to follow the same pattern.

The second hypothesis, our main focus in this paper, depends on the first: if participants are able to coordinate successfully on some labels, the next question is what makes certain labels more salient than others. Following Hargreaves Heap et al. (2017), we consider several observable variables that might capture different pathways to salience.

First, domain-specific recognisability is captured by the label's ranking on specialised websites (e.g., IMDb for films). These rankings reflect what is prominent within interest-based communities and correspond to the heuristic of choosing the "best-known" item in the interest-based community.

Second, we measure recent digital prominence through the frequency of Google searches in the year preceding the experiment. This indicator reflects current public attention and active curiosity (how often people recently sought out the labels) even if they already recognise it.

Third, long-term cultural familiarity is proxied by the frequency of the label in English-language books, using the Google Books Ngram corpus. This provides a measure of entrenched, cumulative exposure and aligns with the "most frequently mentioned" heuristic.

Hypothesis 2 (H2): At least one of the three variables (web rank, Google search rank, book frequency rank) significantly explains salience of the labels.

These three variables (representing domain-specific rank, recent digital attention, and long-term cultural exposure) allow us to compare the relative explanatory power of different mechanisms for why some labels become focal points.

- **H2a:** Coordination success is significantly explained by the rank of labels in specialised websites.
- **H2b:** Coordination success is significantly explained by the frequency of Google searches for the labels.
- **H2c:** Coordination success is significantly explained by the frequency of the labels in English-language books.

We investigate the effect of each of the variables separately and jointly together to assess not only whether each contributes to explaining salience, but also which source of influence has greater explanatory power.

In addition to these hypotheses, we are interested in response times and success rates in the coordination game, aiming to shed light on the cognitive processes behind focal point selection. Theoretical accounts differ in how much deliberation they assume. Some models (e.g., Bacharach, 1997) emphasise interpretive context and reasoning, suggesting that individuals reflect on what others are likely to choose. Others suggest that coordination may rely on fast, intuitive judgments based on shared cultural knowledge or common experiences (Schelling, 1960; Sugden, 1995; Hargreaves Heap et al., 2020).

Drawing on the dual-process theory of cognition developed by Kahneman and Frederick (2002), this distinction can be understood as a contrast between System 1 (fast, automatic, intuitive processing) and System 2 (slow, effortful, deliberative reasoning). If focal point selection operates primarily through System 1 (the first thing that comes to mind), we would expect short response times accompanied by high success rates in the coordination task. Quick, accurate decisions would suggest reliance on intuitive recognition of salient options rather than on deliberate reasoning.

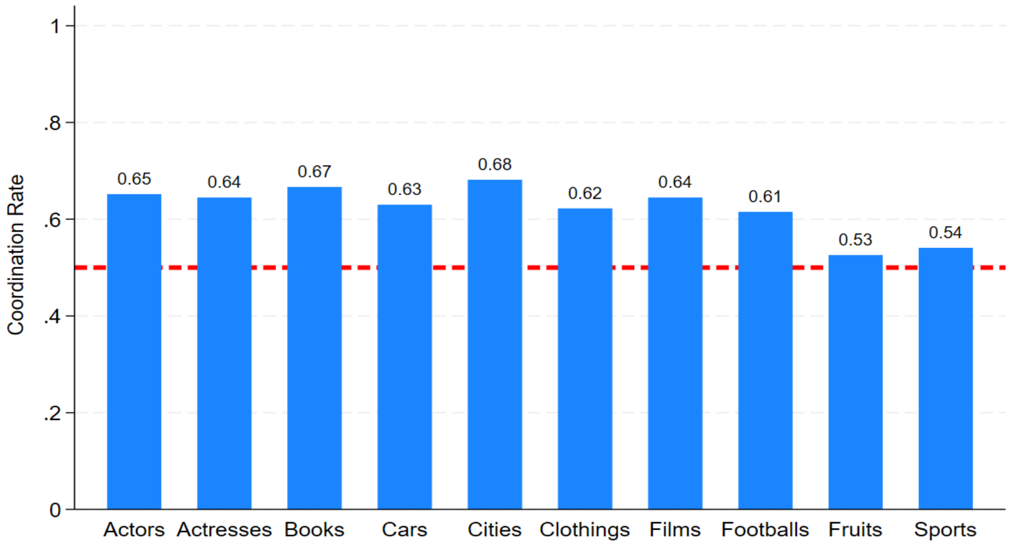
We test this by examining both how quickly participants make their choices and how often they successfully coordinate. Although we did not have prior quantitative predictions about response times under "System 1" versus "System 2" reasoning, we can make informal inferences: if participants predominantly use System 1, then after the initial learning rounds, response times should remain relatively stable, and shorter response times should be associated with higher success rates.

Response time is a useful (though indirect) indicator of cognitive effort, particularly when interpreted alongside success rates. Methods such as eye-tracking (Hayhoe and Ballard, 2005), think-aloud protocols (Ericsson and Simon, 1993), or neuroimaging (Rilling et al., 2004) could offer richer insights into how focal points are processed cognitively, and these could be explored in future studies. However, in this study, we rely on response time as a pragmatic proxy to distinguish between intuitive and deliberative processes.

3.5 Results

We begin by reporting the overall coordination rate, broken down by label categories. In most coordination game experiments, the same games are played by many participants. It is standard to define the coordination rate for a given game as the expected proportion of games in which coordination would be achieved if each participant's actual choice was matched in turn with each other participant's actual choice. This removes the arbitrariness of actual pairings. In our design, however, each category generates 15 possible games (i.e. pairings of two distinct labels from a set of 6); each participant plays 3 games, randomly selected from the set of 15. We define the coordination rate for a category as the proportion of games in that category in which the two players chose the same label. The coordination rate for any given category averages over 135 interactions between pairs of participants (i.e., 90 participants, each playing 3 games). Figure 3.5.2 shows the coordination rate in each category and the average for the all categories taken together.

Figure 3.5.1: Coordination Rate by Category



We can see that the average coordination is 62%. The coordination rate is different for different categories ranging from 53% in ”Fruits” to 68% in ”Cities”.

Result 1. Consistently with Hypothesis H1, the average coordination rate is significantly higher than 50% at the 1% level. In all categories, the coordination rate is higher than 50%; this difference is significant at the 5% level for 8 of the 10 categories (the exceptions are ”Sports” with 54% and ”Fruits with 53%).

It is important to note that the relationship between individual choice probabilities and observed coordination success is non-linear. Specifically, if each participant independently chooses a particular label with probability p, the expected coordination rate is given by $p^2 + (1-p)^2$. This means that quite large asymmetries in choice probabilities can produce coordination rates that are not far from 0.5. For example:

- If $p = 0.5$ (i.e., random choice), the expected coordination rate is 0.5.
- If $p = 0.6$, the expected coordination rate is 0.52 (e.g. ”Fruits” category in our experiment).

- If $p = 0.8$, the expected coordination rate is 0.68 (e.g. "City" category in our experiment).

In our case, the observed average coordination rate is around 0.62, which corresponds to an individual choice probability of approximately $p = 0.75$. This suggests that the observed coordination success is not merely marginally above chance, but instead reflects a meaningful degree of convergence in participants' expectations, consistent with the presence of moderately strong shared focal points, even in the absence of communication.

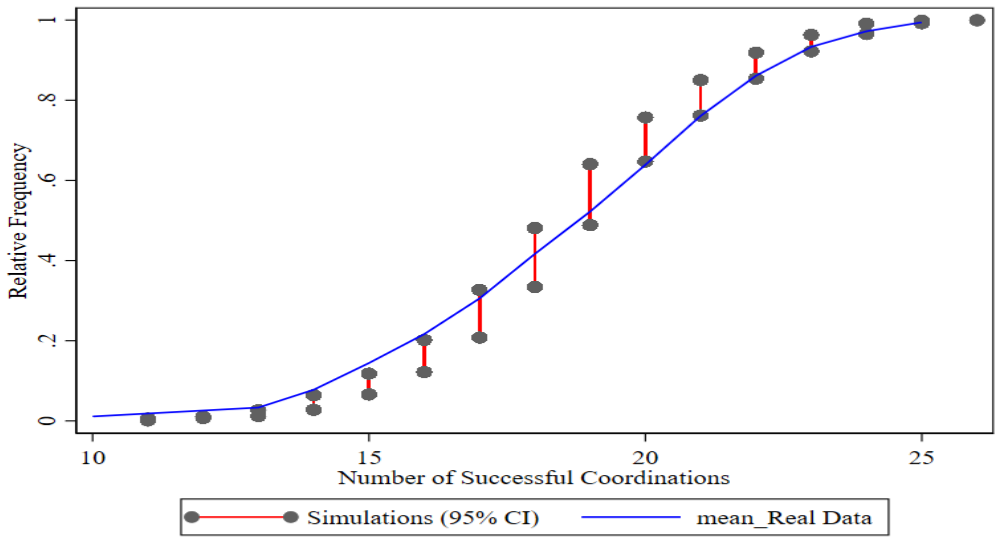
To investigate individual-level heterogeneity in achieving success in the pure coordination games, we constructed a simulated dataset in which the overall structure of success was preserved but individual choices were randomised. Specifically, for each of the ten categories, we fixed the empirical coordination rate observed in the real data and generated simulated player choices based on these fixed rates. This method maintained the marginal probabilities of coordination while stripping away any individual-level heterogeneity in perceptions of salience, cultural knowledge, or focal-point intuition. In essence, the simulation represents what coordination would look like if participants' decisions were entirely driven by independent randomisation, conditional on the empirical success rates.

We then compared the real and simulated data both graphically (through cumulative distribution functions of winning per person) and statistically. The graph illustrates that the distribution of wins among real participants is wider and more dispersed than the simulated data, suggesting more individual-level variation in coordination performance in the real dataset. This visual pattern indicates that while some individuals coordinated very successfully, others did not, consistent with individual-level strategies or underlying intuitions influencing choices.

For a formal comparison of coordination performance between the real and simulated datasets, we employ the robust variance (robvar) test to formally assess whether the variance in success rates differed significantly between the real and simulated datasets. While mean comparisons are useful for detecting overall differences in performance, they may conceal important distributional characteristics (such as heterogeneity in coordination ability that are better captured by differences in variance). The robvar test is particularly appropriate here because it does not assume equal variances across groups and is robust to deviations of data distribution from normality.

The robvar test indicates that the variance in the real data is significantly higher than in the simulated data ($p < 0.05$ across all comparisons). This suggests that there is heterogeneity in skills or structures guiding individuals' choices. One plausible source of this heterogeneity is variation in cultural familiarity, differences in

Figure 3.5.2: Cumulative Distribution of Successful Rate (Simulation and Real Data)



Note: The horizontal axis represents the number of successful coordinations out of 30 rounds. The scale begins at 10, as no individual achieved fewer than 10 successful coordinations in either the real or simulated data. The vertical axis displays the cumulative relative frequency of individuals according to their number of successful coordinations.

Table 3.5.1: Summary Statistics and F-tests

Treatment	Mean	Std. Dev.	Freq.
Real	18.67	3.08	90
Simulate	18.63	2.67	1,000
Total	18.63	2.70	1,090

Test Name	F-statistic	Degrees of Freedom	P-value
W0	5.14	df(1, 1088)	0.02
W50	4.55	df(1, 1088)	0.03
W10	4.99	df(1, 1088)	0.03

Note: The top panel reports descriptive statistics for each treatment group. The bottom panel presents F-tests for treatment effects across different weighting schemes.

what participants have been exposed to or consider salient. An alternative explanation is variation in coordination skill. Given our findings on response times (see later), this skill is unlikely to be rooted in conscious deliberation, but may reflect an intuitive, socially learned ability to anticipate others' expectations in common knowledge situations. Both mechanisms (shared cultural exposure and learned intuitions from social interaction) likely contribute to the observed differences in coordination success.

Thus far, we have found that coordination rates in our games exceed those predicted by random choice, and that individuals vary substantially in their ability to coordinate successfully. This suggests that certain intuitions, skills, or cognitive strategies may underlie successful coordination, and that these vary across individuals. These findings provide the basis for investigating Hypothesis 2, which concerns the sources of such successful coordination.

To test Hypothesis 2, we examine whether three external measures of familiarity could predict the salience of labels, as they are peroxided by their frequency of selection in the coordination task. To do so, we estimated the following linear regression model.

$$\text{count\_item}_i = \beta_0 + \beta_1 \,\text{web\_rank}_i + \beta_2 \,\text{search\_google\_rank}_i + \beta_3 \,\text{frequency\_english\_books\_rank}_i + \varepsilon_i \quad (3.1)$$

Where:

- **count_item$_\mathbf{i}$**: The frequency that each item i is chosen by participants which is a non-negative counting variable.

- **web_rank$_\mathbf{i}$**: The rank of item i in the curated online list for the relevant category. Lower values indicate higher ranks.

- **search_count_rank$_\mathbf{i}$**: The rank of item i defined by the frequency with which each label in the relevant category was searched on Google during a defined period. Lower values indicate higher ranks.

- **frequency_books_rank$_\mathbf{i}$**: The rank of item i defined by frequency with which each label in the relevant category appeared in English-language books. Lower values indicate higher ranks.

- $\varepsilon_\mathbf{i}$: The error term accounting for unobserved factors affecting the frequency usages of items in the pure coordination game i.

In the Equation 3.1, the dependent variable, which captures the salience of labels, is derived from the experimental data. While the procedure for assigning website-based ranks was outlined earlier, we will describe how the other two independent variables are constructed using publicly available online sources.

The search popularity of each label was obtained using Google Trends[23]. English language frequency data were retrieved from the Google Books Ngram Viewer[24]. All

[23]We obtain the data on the date 12/09/2024 from https://trends.google.com/trends website. Weekly data were collected for each item between 26 March 2023 and 28 April 2024 (before the first lab session started), and total search counts across this period were calculated to represent cumulative interest.

[24]`https://books.google.com/ngrams` This tool analyses the occurrence of words and phrases appears in the corpus of digitised published books during the specific year. This is across a wide

rankings (search count rank, books frequency rank, and web-based rank) were computed separately within each category, resulting in ordinal measures ranging from 1 (most frequent) to 6 (least frequent).

For our regression, the dependent variable (count_item) is a frequency with which items were chosen by participants. The independent variables are category-specific rank-based indicators of item familiarity derived from three sources: web popularity, Google search frequency, and book frequency, each calculated within item categories.

The dependent variable in this study (count_item) is a non-negative integer count variable, which violates the assumptions of ordinary least squares (OLS) regression that require continuous, normally distributed errors. OLS can produce inefficient and biased estimates when applied to count data, especially when the variance is not constant (heteroskedasticity) and the data are skewed. Count models like Poisson regression and Negative Binomial regression are more appropriate for such data. While the Poisson model assumes that the mean and variance of the count variable are equal, this assumption is often unrealistic in practice. In this case, the data show overdispersion (variance greater than the mean), as indicated by the dispersion parameter (alpha) being significantly different from zero. Therefore, the Negative Binomial (NB) regression model, which accounts for overdispersion by introducing an additional parameter, provides a better fit and more reliable estimates than the Poisson model.

To ensure the robustness of the main results obtained from the Negative Binomial model, additional regressions were conducted using OLS with the original count item, and a rank-transformed dependent variable (count_item_rank) analysed via rank-ordered logistic regression (ROL). Although these alternative methods rely on different assumptions and model specifications, the results consistently produce the result that frequency books rank has a positive effect on the frequency of choice, significant at the 5% level, while the effects of web rank and search count rank are not statistically significant. This consistency across different modelling approaches strengthens the confidence in the key findings and suggests that the significance of rank of frequency of items used in books is not an artifact of the modelling choice[25].

The result of the regression are reported can in Table 3.5.2.

range of published books such as fiction, non-fiction, academic texts, and other printed materials dating from the 1500s to recent years. However, it is important to note that ‘books’ in this context refers to printed and digitised texts in the Google Books database, which may not fully represent all forms of language exposure today. Despite this limitation, the Ngram Viewer provides a valuable long-term measure of cultural and linguistic familiarity within English-speaking contexts. For each item, we used the most recent available rank corresponding to the year 2022.

[25] The use of the rank-ordered Logit model allowed us to capture the ordinal nature of the data while accounting for differences between item categories, thereby strengthening the robustness of the findings.

Table 3.5.2: Regression of participants' usage of labels on Different Variables

	(1) count_item (NB)	(2) count_item (OLS)	(3) count_item_rank (ROL)
web_rank1	-0.0282	-1.3564	0.0521
	(0.0230)	(1.0819)	(0.0930)
search_count_rank	-0.0256	-1.2006	-0.0089
	(0.0270)	(1.3109)	(0.1013)
freq_book_rank	-0.0807***	-3.6013***	0.3159***
	(0.0258)	(1.2350)	(0.1103)
Observations	60	60	60
Log likelihood	-246.2248		-65.7925
Pseudo R^2	0.0259	0.2068	0.084

Robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The negative binomial model is the main regression and the other two OLS and rank-ordered logit (ROL) model are for robustness check.

Among the predictors, `freq_book_rank` was the only variable to reach conventional levels of statistical significance, indicating it has greater explanatory power compared to the other two measures to explain item usage. Specifically, a one-rank increase in `freq_book_rank` (i.e., the item appearing less frequently in books) was associated with a statistically significant increase in the likelihood of that item receiving a worse usage rank in the experiment (coefficient = 0.080, p = 0.002). This suggests that participants were more likely to use labels that are more frequently found in books. In contrast, web-based and search-based familiarity measures were not statistically significant in this dataset, suggesting their explanatory power is weaker than that of book frequency. Based on this, we can report the result for hypothesis 2 and its sub hypotheses.

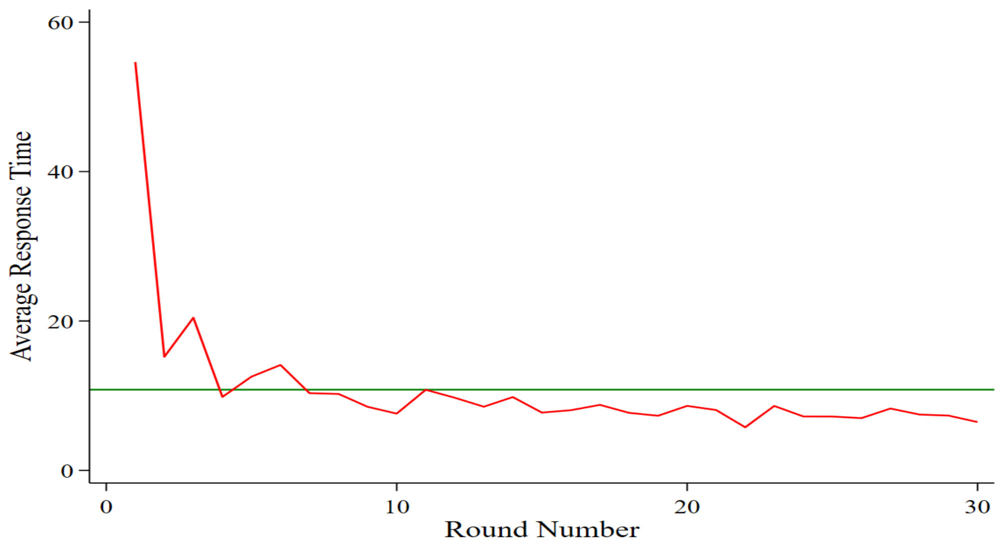
Result 2. Consistent with Hypothesis 2 (H2), the negative binomial regression model including web rank, Google search rank, and English book frequency rank significantly explains the frequency usage of items in the pure coordination game (i.e., salience) $\chi^2(3) = 18.74, p = 0.0003$. Within this model, `freq_book_rank` emerged as the strongest predictor, with a statistically significant negative effect (coef $= -0.081, p = 0.002$), indicating that higher frequency in English books is strongly associated with increased item usage frequency, while the coefficients for `web_rank` and `search_google_rank` were not statistically significant.

The results support Hypothesis 2 by showing that salience in coordination is most strongly associated with long-term cultural familiarity, as measured by book frequency. In comparison, the explanatory contributions of recent public interest (Google search rank) and domain-specific visibility (website rank) were comparatively weaker and did not reach statistical significance in this analysis. This suggests that labels people have been repeatedly exposed to over time are more likely to become focal points in coordination tasks.

In addition to the hypotheses we tested, we are interested in understanding individuals' response time in different rounds which are related to the pure coordination game with different labels.

We analyse the average time participants took to make their choices across all 30 rounds. Individuals' average response times for pure coordination games in different rounds can be seen in figure 3.5.3.

Figure 3.5.3: Average Participants' Responses Time in different rounds in Seconds



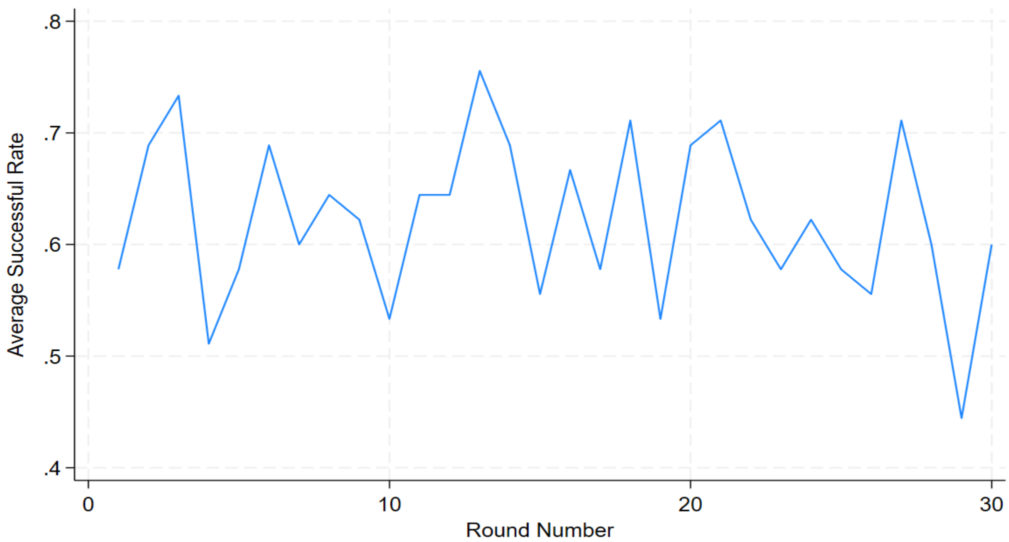
We begin with a visual inspection of participants' response times across rounds. The data show that response times drop sharply during the initial rounds, particularly in rounds 1 to 5, which likely reflect a learning phase. After this early adjustment period, response times stabilise: they decrease to approximately 10–11 seconds by round 6 and remain in the range of 8–9 seconds up to round 10 (end of the first block). From round 11 onward, response times fluctuate slightly but stay within a relatively narrow and stable range of 8–10 seconds. Additionally, the overall mean response time is 10.7 seconds, indicating quick decision-making. This supports the idea that focal point recognition is often intuitive rather than deliberative. That is, individuals do not calculate salience in real-time; rather, they draw on cultural knowledge or habits that allow them to make rapid, shared judgments.

To formally assess whether response times systematically decline beyond the initial learning phase, we estimate a fixed-effects regression using only rounds greater than 5[26]. This model captures within-subject changes over time, effectively controlling for any constant individual differences (e.g., general response speed). We do a regression estimating an exponential function.

The results show a significant negative effect (= −0.011, p = 0.000) which means that for every additional round, the response time decreases by 1.1%. Although the qualitative effect is highly significant, the quantitative effect size is very small.

The next step to see if the average rate changes over the rounds. To investigate this, graphically we can see figure 3.5.4. This shows that there are a lot of fluctuations in different rounds and it seems there is not any pattern for changing of average successful rates in different rounds.

Figure 3.5.4: Coordination Success in different Rounds



For a formal test, we regress individuals' coordination success (i.e. the frequency with which they coordinate with their co-player) in different rounds (after round 5) against round number. The results show that the effect of round number on individuals' coordination rates is not significant at the 5% level. There is a marginally significant effect at the 10% level ($\beta = -0.0027$, $p = 0.052$), but the sign is negative, indicating a slight decline in coordination success over the 30 rounds.

Combining the above-mentioned two results is interesting. First we find that, after the first five rounds, participants' response times decrease very slightly over further rounds (1.1% decrease in each round). Then we find that participants' coordination

[26]The reason is that individuals use the first 5 rounds to learn the rules of the game and related instructions and that is why time response in this 5 rounds are hugely higher than the later rounds.

rates do not improve over rounds. The combination of these two findings suggests that participants understand the rules of the game after the first few rounds, but do not learn more about how to achieve coordination from then on.

To gain further insight to participants' response times, we examine the relationship between response time and coordination success. We analysed the data at both the participant and round levels. First, we computed each participant's average response time and average coordination success rate across the 30 rounds for each participant. A linear regression using these participant-level averages revealed a marginally significant negative relationship ($\beta = -0.0046$, $p = 0.059$), suggesting that participants who responded more quickly on average tended to achieve higher coordination success. This finding is consistent with the notion that salience recognition is intuitive and may operate more efficiently among participants who respond more quickly.

However, when we turned to the round-level data (analysing all observations while accounting for within-subject clustering) no significant relationship was found. Neither a simple logistic regression with clustered standard errors nor a random-effects logistic model showed any effect of response time on coordination success ($p > 0.80$ in all specifications). These findings indicate that although faster-responding individuals may, on average, be more successful, fluctuations in decision speed within individuals across rounds do not systematically predict coordination outcomes. Thus, the relationship appears to reflect between-subject differences (e.g., individual style or cognitive tendency), rather than within-subject which is based on rounds for all participants.

3.6 Conclusion

This study aims to explore a fundamental question in coordination theory: What makes certain labels more effective focal points than others in pure coordination games with labels? According to our experimental evidence, individuals consistently coordinate above random levels when choosing between labeled options.

Crucially, we tested three plausible indicators of salience, popularity rankings on websites, frequency in Google searches, and frequency in books, and found that book frequency was the measure to reach conventional levels of statistical significance. Participants were more likely to coordinate on labels that are more frequently used in books, while recent online popularity and domain-specific rankings showed comparatively weaker explanatory power. This finding suggests that salience in coordination is more strongly associated with long-term cultural familiarity than with measures of recent public interest or domain-specific visibility. Frequency in books corresponds with 'most frequently mentioned' labels (those that people have been repeatedly exposed to over time) and may therefore be more readily brought to mind in coordination tasks (see Sugden (1995)). In contrast, Google search rank captures more active and recent interest, while website rank reflects prominent within natural domains. These distinctions align with broader accounts of salience mechanisms discussed by Hargreaves Heap et al. (2017).

We also demonstrated that players made their decisions rapidly (the median response time is 7 seconds). This is in line with findings in Isoni et al. (2020). Isoni et al. (2020) find short response times for a variety of different coordination games. In their pure coordination games (in which labelling cues are induced by the spatial layout of discs on a 'bargaining table'), the median response time is 4.06 seconds in the presence of labelling cues and 5.70 seconds in their absence. Furthermore, we show that response times decrease slightly over rounds, but coordination does not improve (and even slightly decreases) over rounds. This provides support for the idea that focal point selection is intuitive and shaped by accumulated cognitive and cultural experience, rather than being the product of deliberative strategic reasoning.

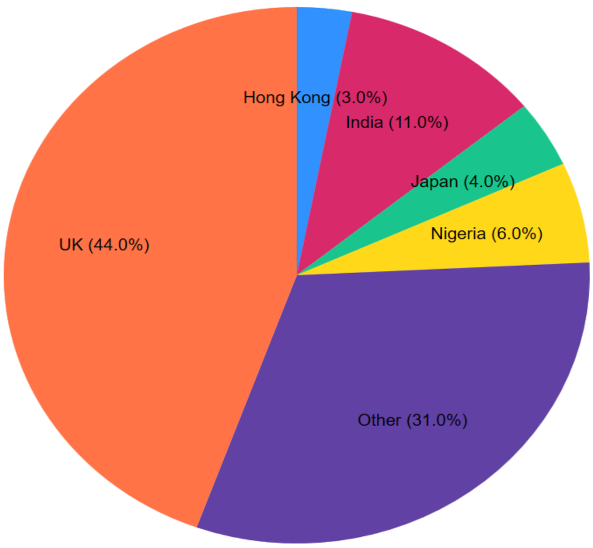
Our findings contribute to this literature by offering empirical support for the distinction between long-term and recent familiarity in shaping focal point selection. While previous studies have often treated frequency of exposure as a unified mechanism, our analysis shows that only frequency in books, not websites or online searches, robustly predicts successful coordination. This suggests that focal salience cannot be fully explained by short-term exposure or natural website visibility.

The most commonly applied coordination rules identified by Hargreaves Heap et al. (2017), Typicality I, Typicality II, and Prominence, are consistent with frameworks such as cognitive fluency (Alter and Oppenheimer, 2009), shared exposure (Clark, 1996), and the availability heuristic (Tversky and Kahneman, 1973), which imply that frequently encountered or familiar labels should facilitate coordination. However, our results show that not all forms of frequency are equal: only long-term (culturally) embedded labels, as reflected in book frequency, consistently guide coordination.

This supports the view that long-term, culturally transmitted knowledge exerts a stronger influence on collective reasoning than more ephemeral, context-specific exposure. The theory of cultural entrenchment (Mesoudi, 2011) helps explain this by suggesting that ideas embedded in books are more likely to be transmitted across generations and become widely shared.

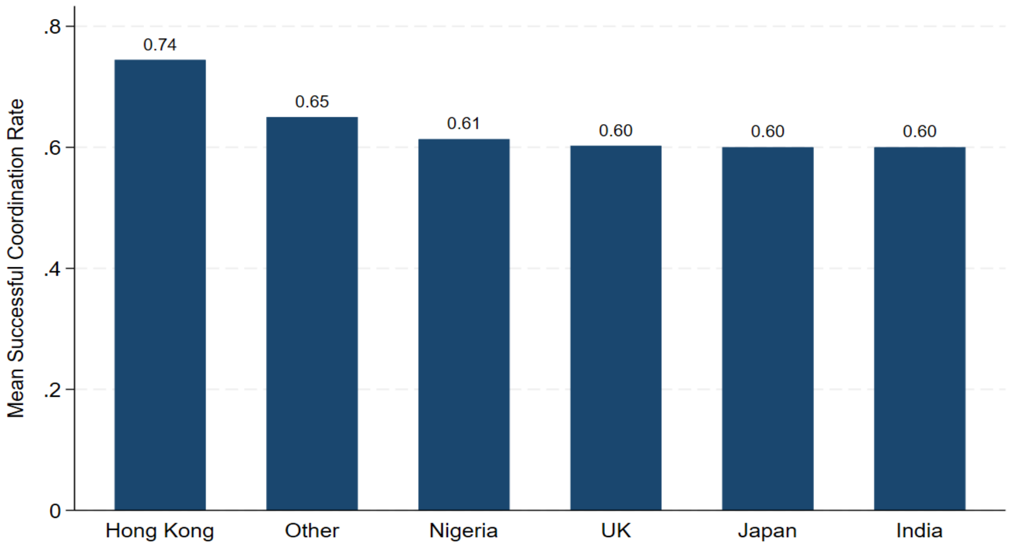
One might hypothesise that cultural background, proxied by country of birth, enhances cooperation such that individuals from the same country (and therefore similar cultural norms) coordinate more successfully. However, our findings do not support this view. Our sample consists of 90 participants and the details of their country of birth are shown in figure 3.6.1.

Figure 3.6.1: Participants by Country born



The average successful coordination rates by country group are shown in Figure 3.6.2.

Figure 3.6.2: Average Successful Coordination by Country



The key takeaway from this graph is that the success rate does not significantly differ across participants from different countries.[27]

The finding that quicker decisions led to better coordination suggests that people

[27]One might argue that the average successful coordination rate for participants from Hong Kong is statistically different from other countries. However, this result is based on only three participants, which weakens the strength of this conclusion. If multiple countries exhibited similar patterns, it would warrant further consideration.

relied more on fast, intuitive thinking than on slow, deliberate reasoning. This supports the idea of System 1 thinking (automatic and effortless) rather than System 2, which is slower and more analytical (Kahneman and Frederick, 2002). It challenges models like Cognitive Hierarchy Theory (Camerer et al., 2004*a*), which assume players engage in complex strategic thinking about others' decisions.

Bibliography

Alesina, A. and Angeletos, G.-M. (2005), ‘Fairness and redistribution’, *American economic review* **95**(4), 960–980.

Alesina, A. and Perotti, R. (1996), ‘Income distribution, political instability, and investment’, *European Economic Review* **40**(6), 1203–1228.

Almås, I., Cappelen, A. W., Sørensen, E. and Tungodden, B. (2010), ‘Fairness and the development of inequality acceptance’, *Science* **328**(5982), 1176–1178.

Alter, A. L. and Oppenheimer, D. M. (2009), ‘Uniting the tribes of fluency to form a metacognitive nation’, *Personality and Social Psychology Review* **13**(3), 219–235.

Bacharach, M. (1999), ‘Interactive team reasoning: A contribution to the theory of cooperation’, *Research in Economics* **53**(2), 117–147.

Bacharach, M. (2006), *Beyond Individual Choice: Teams and Frames in Game Theory*, Princeton University Press.

Bacharach, M. and Bernasconi, M. (1997), ‘The variable frame theory of focal points: An experimental study’, *Games and Economic Behavior* **19**(1), 1–45.

Bardsley, N., Mehta, J., Starmer, C. and Sugden, R. (2010), ‘Explaining focal points: Cognitive hierarchy theory versus team reasoning’, *The Economic Journal* **120**(543), 40–79.

Barro, R. J. (2000), ‘Inequality and growth in a panel of countries’, *Journal of Economic Growth* **5**, 5–32.

Bartling, B., Cappelen, A. W., Skarpeid, I. L., Sørensen, E. and Tungodden, B. (2025), The talent paradox: why is it fair to reward talent but not luck?, Working Paper 464, Department of Economics.

Blount, S. (1995), ‘When social outcomes aren’t fair: The effect of causal attributions on preferences’, *Organizational Behavior and Human Decision Processes* **63**(2), 131–144.

Bolton, G. E. (1991), 'A comparative model of bargaining: Theory and evidence', *American Economic Review* **81**(5), 1096–1136.

Bolton, G. E., Brandts, J. and Ockenfels, A. (2005), 'Fair procedures: Evidence from games involving lotteries', *The Economic Journal* **115**(506), 1054–1076.

Bolton, G. E. and Ockenfels, A. (2000), 'Erc: A theory of equity, reciprocity, and competition', *American Economic Review* **90**(1), 166–193.

Buchanan, J. M. (1964), 'What should economists do?', *Southern Economic Journal* **30**(3), 213–222.

Camerer, C. F., Ho, T.-H. and Chong, J.-K. (2004*a*), 'A cognitive hierarchy model of games', *The Quarterly Journal of Economics* **119**(3), 861–898.

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

Camerer, C., Ho, T.-H. and Chong, J.-K. (2004*b*), 'Cognitive hierarchy: A limited thinking theory of games', *The Quarterly Journal of Economics* **119**(3), 861–898.

Cappelen, A. W., Falch, R. and Tungodden, B. (2020), Fair and unfair income inequality, *in* 'Handbook of Labor, Human Resources and Population Economics', Springer, pp. 1–25.

Cappelen, A. W., Hole, A. D., Sørensen, E. Ø. and Tungodden, B. (2007), 'The pluralism of fairness ideals: An experimental approach', *American Economic Review* **97**(3), 818–827.

Cappelen, A. W., Konow, J., Sørensen, E. Ø. and Tungodden, B. (2013), 'Just luck: An experimental study of risk-taking and fairness', *American Economic Review* **103**(4), 1398–1413.

Cappelen, A. W., Liu, Y., Nielsen, H. and Tungodden, B. (2024), Fairness in a society of unequal opportunities. Forthcoming or manuscript.

Cappelen, A. W., Moene, K. O., Skjelbred, S. E. and Tungodden, B. (2023), 'The merit primacy effect', *The Economic Journal* **133**(651), 951–970.

Cappelen, A. W., Sørensen, E. Ø. and Tungodden, B. (2010), 'Responsibility for what? fairness and individual responsibility', *European Economic Review* **54**(3), 429–441.

Chancel, L., Piketty, T., Saez, E. and Zucman, G. (2021), *World inequality report 2022*, World Inequality Lab.

Chen, Daniel L., M. S. and Wickens, C. (2016), 'otree—an open-source platform for laboratory, online, and field experiments', *Journal of Behavioral and Experimental Finance* **9**, 88–97.

Chen, Y. and Li, S. X. (2009), 'Group identity and social preferences', *American Economic Review* **99**(1), 431–457.

Chlaß, N., Güth, W. and Miettinen, T. (2019), 'Purely procedural preferences—beyond procedural equity and reciprocity', *European Journal of Political Economy* **59**, 108–128.

Clark, H. H. (1996), *Using Language*, Cambridge University Press.

Conte, A. and Moffatt, P. G. (2014), 'The econometric modelling of social preferences', *Theory and Decision* **76**, 119–145.

Corder, G. W. and Foreman, D. I. (2009), *Nonparametric statistics for non-statisticians: A step-by-step approach*, Wiley.

Crawford, V. P., Gneezy, U. and Rottenstreich, Y. (2008), 'The power of focal points is limited: Even minute payoff asymmetry may yield large coordination failures', *American Economic Review* **98**(4), 1443–1458.

Dworkin, R. (1981*a*), 'What is equality? part 1: Equality of welfare', *Philosophy & Public Affairs* **10**(3), 185–246.

Dworkin, R. (1981*b*), 'What is equality? part 2: Equality of resources', *Philosophy & Public Affairs* **10**(4), 283–345.

Ericsson, K. A. and Simon, H. A. (1993), *Protocol analysis: Verbal reports as data*, rev. ed. edn, MIT Press.

Faillo, M., Ottone, S. and Zarri, L. (2017), 'Beyond the hypothesis of the internalization of social norms: Evidence from a coordination game experiment', *Journal of Economic Behavior & Organization* **144**, 114–124.

Fehr, E. and Schmidt, K. M. (1999), 'A theory of fairness, competition, and cooperation', *The Quarterly Journal of Economics* **114**(3), 817–868.

Fong, C. M. (2007), 'Evidence from an experiment on charity to welfare recipients: Reciprocity, altruism, and the empathic responsiveness hypothesis', *The Economic Journal* **117**(522), 1008–1024.

Gee, L. K., Migueis, M. and Parsa, S. (2017), 'Redistributive choices and increasing income inequality: Experimental evidence for income as a signal of deservingness', *Experimental Economics* **20**(4), 894–923.

Gneezy, U. and Rottenstreich, Y. (2024), 'Failing to utilize potentially effective focal points: Prominence can stymie coordination on distinct actions', *Games and Behavior* **1**(148).

Hansen, B. E. (2022), *Econometrics*, Princeton University Press.

Hargreaves Heap, S. P., Rojo Arjona, D. and Sugden, R. (2017), 'Coordination when there are restricted and unrestricted options', *Theory and Decision* **83**(1), 107–129.

Hayek, F. A. (1976), *Law, Legislation and Liberty. Vol. 2: The Mirage of Social Justice*, University of Chicago Press.

Hayhoe, M. and Ballard, D. (2005), 'Eye movements in natural behavior', *Trends in Cognitive Sciences* **9**(4), 188–194.

Hoffman, E., McCabe, K., Shachat, K. and Smith, V. (1994), 'Preferences, property rights, and anonymity in bargaining games', *Games and Economic Behavior* **7**(3), 346–380.

Hoffman, E. and Spitzer, M. L. (1985), 'Entitlements, rights, and fairness: An experimental examination of subjects' concepts of distributive justice', *The Journal of Legal Studies* **14**(2), 259–297.

Isoni, A., Poulsen, A., Sugden, R. and Tsutsui, K. (2013), 'Focal points in tacit bargaining problems: Experimental evidence', *European Economic Review* **59**, 167–188.

Isoni, A., Poulsen, A., Sugden, R. and Tsutsui, K. (2019), 'Focal points and payoff information in tacit bargaining', *Games and Economic Behavior* **114**, 193–214.

Isoni, A., Sugden, R. and Zheng, J. (2020), 'The pizza night game: Conflict of interest and payoff inequality in tacit bargaining games with focal points', *European Economic Review* **127**, 103428.

Kahneman, D. and Frederick, S. (2002), Representativeness revisited: Attribute substitution in intuitive judgment, *in* T. Gilovich, D. Griffin and D. Kahneman, eds, 'Heuristics and Biases: The Psychology of Intuitive Judgment', Cambridge University Press, pp. 49–81.

Knowles, S. (2005), 'Inequality and economic growth: The empirical relationship reconsidered in the light of comparable data', *The Journal of Development Studies* **41**(1), 135–159.

Konow, J. (2000), 'Fair shares: Accountability and cognitive dissonance in allocation decisions', *American Economic Review* **90**(4), 1072–1091.

Krawczyk, M. W. (2011), 'A model of procedural and distributive fairness', *Theory and Decision* **70**(1), 111–128.

Mas-Colell, A., Whinston, M. D. and Green, J. R. (1995), *Microeconomic Theory*, Oxford University Press.

Mehta, J., Starmer, C. and Sugden, R. (1994*a*), 'Focal points in pure coordination games: An experimental investigation', *Theory and Decision* **36**(2), 163–185.

Mehta, J., Starmer, C. and Sugden, R. (1994*b*), 'The nature of salience: An experimental investigation of pure coordination games', *American Economic Review* **84**(3), 658–673.
URL: *https://www.jstor.org/stable/2118074*

Mesoudi, A. (2011), *Cultural Evolution: How Darwinian Theory Can Explain Human Culture and Synthesize the Social Sciences*, University of Chicago Press.

Mirrlees, J. A. (1971), 'An exploration in the theory of optimum income taxation', *The Review of Economic Studies* **38**(2), 175–208.

Moffatt, P. G. (2015), *Experimetrics: Econometrics for Experimental Economics*, Macmillan International Higher Education.

Mollerstrom, J., Reme, B.-A. and Sørensen, E. (2015), 'Luck, choice and responsibility — an experimental study of fairness views', *Journal of Public Economics* **131**, 33–40.

Nagel, R. (1995), 'Unraveling in guessing games: An experimental study', *The American Economic Review* **85**(5), 1313–1326.

Nagel, T. (1991), *Equality and Partiality*, Oxford University Press.

Nunnari, S. and Pozzi, M. (2022), Meta-analysis of inequality aversion estimates, CESifo Working Paper 10009, CESifo.
URL: *https://www.cesifo.org/en/publications/2022/working-paper/meta-analysis-inequality-aversion-estimates*

Perotti, R. (1993), 'Political equilibrium, income distribution, and growth', *The Review of Economic Studies* **60**(4), 755–776.

Rabin, M. (1993), 'Incorporating fairness into game theory and economics', *The American Economic Review* **83**(5), 1281–1302.

Rawls, J. (1958), 'Justice as fairness', *Philosophical Review* **67**(2), 164–194.

Rawls, J. (1971), *A Theory of Justice*, Harvard University Press, Cambridge, MA and London.

Rilling, J. K., Sanfey, A. G., Aronson, J. A., Nystrom, L. E. and Cohen, J. D. (2004), 'The neural correlates of theory of mind within interpersonal interactions', *NeuroImage* **22**(4), 1694–1703.

Roemer, J. E. (1993), 'A pragmatic theory of responsibility for the egalitarian planner', *Philosophy & Public Affairs* **22**(2), 146–166.

Roemer, J. E. (1998), *Equality of Opportunity*, Harvard University Press.

Rosch, E. (1977), Classification of real-world objects: Origins and representations in cognition, *in* P. N. Johnson-Laird and P. C. Wason, eds, 'Thinking: Readings in Cognitive Science', Cambridge University Press, Cambridge, pp. 212–222.

Saito, K. (2013), 'Social preferences under risk: Equality of opportunity versus equality of outcome', *American Economic Review* **103**(7), 3084–3101.

Schelling, T. C. (1960), *The Strategy of Conflict*, Harvard University Press.

Sebald, A. (2010), 'Attribution and reciprocity', *Games and Economic Behavior* **68**(1), 339–352.

Stahl, D. O. and Wilson, P. W. (1995), 'On players' models of other players: Theory and experimental evidence', *Games and Economic Behavior* **10**(1), 218–254.

Starmans, C., Sheskin, M. and Bloom, P. (2017), 'Why people prefer unequal societies', *Nature Human Behaviour* **1**(4), 1–7.

Sugden, R. (1993), 'Thinking as a team: Towards an explanation of nonselfish behavior', *Social Philosophy and Policy* **10**(1), 69–89.

Sugden, R. (1995), 'A theory of focal points', *The Economic Journal* **105**(430), 533–550.

Sugden, R. (2000), 'Team preferences', *Economics and Philosophy* **16**(2), 175–204.

Sugden, R. and Wang, M. (2020), 'Equality of opportunity and the acceptability of outcome inequality', *European Economic Review* **130**, 103597.

Tajfel, H., Billig, M. G., Bundy, R. P. and Flament, C. (1971), 'Social categorization and intergroup behaviour', *European Journal of Social Psychology* **1**(2), 149–178.

Tajfel, H., Turner, J. C., Austin, W. G. and Worchel, S. (1979), An integrative theory of intergroup conflict, *in* 'Organizational Identity: A Reader', Oxford: Oxford University Press, pp. 56–65. Reprinted in 2004, ed. Whetten Godfrey.

Temkin, L. S. (1993), *Inequality*, Oxford University Press.

Trautmann, S. T. (2009), 'A tractable model of process fairness under risk', *Journal of Economic Psychology* **30**(5), 803–813.

Tversky, A. (1977), 'Features of similarity', *Psychological Review* **84**(4), 327–352.

Tversky, A. and Kahneman, D. (1973), 'Availability: A heuristic for judging frequency and probability', *Cognitive Psychology* **5**(2), 207–232.

United Nations, D. o. E. and Affairs, S. (2020), 'World social report 2020: Inequality in a rapidly changing world'.
URL: *https://www.un.org/development/desa/dspd/world-social-report/2020-2.html*