THE UNIVERSITY *of* EDINBURGH

Edinburgh Research Explorer

Are people equally other-regarding when selecting a match vs choosing an allocation?

OPEN ACCESS

Are People Equally Other-Regarding When Selecting a Match vs Choosing

an Allocation?

Michèle Belot (Corresponding author)

Department of Economics, European University Institute, Villa La
Fonte, Via delle Fontanelle 18, 50014 San Domenico di Fiesole
(Italy). Tel. [+39] 055 4685 417/982. Email: michele.belot@eui.eu.

Marcel Fafchamps

Freeman Spogli Institute for International Studies, Stanford
University, Encina Hall, 616 Serra Street Stanford, CA 94305-6055
(USA). Email: fafchamp@stanford.edu

Abstract

There are many assignment processes in which agents are given the opportunity to unilaterally select a match. Resulting allocations can be inefficient if agents do not internalize the consequences of their choice on others. To test this formally, we study how other-regarding behavior vary across two decision contexts: when subjects make a pure allocation decision; and when they select a partner. In both settings each subject's decision is final and it affects their payoff and that of other subjects in the same way. We find that that subjects are more likely to sacrifice their own material well-being to increase that of others when dividing a pie than when selecting a partner in a large anonymous setting — even though the consequences on the material payoffs of others are identical. These findings suggest that, in assignment processes with unilateral selection, efficiency can be improved by presenting the selection process as a choice between outcomes involving multiple individuals, instead of simply selecting a match for themselves.

1 Motivation

Economists and sociologists have long been interested in assignment processes in which agents are matched without a pairwise price mechanism, starting with the early work by Becker (1973). Examples of such processes include: the matching of organ donors with recipients (e.g., Baccara et al. 2016); the assignment of students to schools, dorms, or work groups (e.g., Agarwal and Somaini 2016, Sacerdote 2001, Fafchamps and Mo 2016); and marriage markets without dowry, bride price or pre-nuptial agreement (e.g., Becker 1973). Even in markets with a price mechanism, the assignment of goods and services to individual buyers often is subject to random variation due to fixed pricing or limited supply – e.g., queuing at the emergency room, or lining up for fresh bread at the baker's. In all these cases, match selection plays a crucial role in the efficiency and equity of the resulting allocation of resources – e.g., are goods or services going to those with the largest consumer surplus.

In matching markets, the choices that people make often have consequences on others because they effectively reduce the choice set of others. This happens for instance when someone picks a product from the shop shelf, sets an appointment with a dentist, or picks a child for adoption when they reach the top of the queue. In all these examples, assignments typically differ in aggregate efficiency and they generate different distributions of welfare gains. Hence in the absence of a mechanism to adjudicate matches in an efficient or equitable manner, the quality of the resulting allocation depends on whether people internalize the externalities they impose on others. For instance: do you take the last croissant on the shelf, or should you leave it for the next consumer; do you take that early morning dentist slot or should you leave it for someone with a more constrained schedule; do you pick that young healthy kidney for yourself, or do you leave it for a younger recipient further down the queue for organ transplants.

In this paper we examine whether we can nudge people to make choices that internalize their impact on others. To this effect, we compare individual behavior in decisions that have identical consequences on payoffs for self and others, but take different forms: either a pure allocation decision; or match selection. We hypothesize that these decision environments trigger different behavior. Pure allocation decisions resemble those taken within families or organizations, and have been studied extensively in the experimental literature. There is considerable evidence that experimental subjects behave pro-socially in dictator games and other pie division games. This has been interpreted as suggesting that people have other-regarding preferences (e.g., Fehr and Schmidt 1999). There is also abundant experimental evidence that those who share less than fairly are subject to second or third-party punishment – i.e., either by the recipient, as in the ultimatum game (e.g., Guth et al., 1982), or by a third-party observer (e.g., Fehr and Fischbacher, 2004). This suggests that, when making an allocation decision, people may be influenced by norms of fairness, not just innate altruistic preferences.

In contrast, match selection puts individuals in competition with each other. In such decisions, people often behave in a selfish or even rival manner. The literature on decentralized matching describes match formation as a market-like process. For markets, the original insight goes back to Adam Smith's shoemaker parable, and it has been verified in numerous market experiments (e.g., Smith 1962, Roth et al. (1991), or more recently Falk and Szech (2013), Bartling, Weber and Yao (2015), Kirchler et al. (2016), Harbring (2010), Schwieren and Weichselbaumer (2010)). Becker (1973) offers a similar result in the context of competition for mates. In a review of the experimental evidence, Bowles (1998) observes that the more the experimental situation approximates a competitive (and complete contracts) market with many anonymous buyers and sellers, the less other-regarding behavior is observed.[1] Smith (1998) points out that these two aspects of human nature are not contradictory but apply to different contexts. Selfish

---

[1]This finding fits with the two apparently opposite views of Adam Smith who argues in the *Wealth of Nations* (1776) that self-interest prevails in markets, while acknowledging the pro-sociality of human nature in the *Theory of Moral Sentiments* (1759).

behavior maximizes the gains from impersonal market exchange, while cooperative behavior maximizes the gains from non-market personal exchange. Little is known as to what role other-regarding preferences play in *match selection*. The question is: do people apply competitive heuristics when selecting a match; or do they behave pro-socially, as they do once partnerships are formed?

To investigate this question, we design a novel experiment in which subjects unilaterally choose between two alternatives. The choice they make directly or indirectly determines their payoff as well as that of three other participants. The purpose of the experiment is to test whether final payoff allocations depend on the type of decision subjects make. To this effect, the experiment is designed to ensure full information and to eliminate any strategic interaction between subjects. This allows us to study behavior in situations where people make unilateral assignment decisions.[2]

The main experimental variation across treatments is the way in which the decision is framed. This places our paper within the broader literature on nudging (e.g., Thaler and Sustein 2008). Most contributions to this literature have sought to improve individual decision making. Our focus is on inducing people to internalize the effect of their choices on others. To our knowledge, this type of nudging has never been studied before. We conjecture that framing a decision in a competitive and anonymous setting induces people to behave in a competitive, and possibly rival manner. In contrast, framing a decision like an allocation decision – similar to those taken within the household – may nudge people to behave less selfishly.

Each experimental session involves 24 people equally divided into categories and types. Participants are first randomly assigned to one of two neutrally labelled categories that are intended to capture the two sides of an assignment process – e.g., organ donor and recipient. Within each category, each subjects is assigned to one of two types – e.g., high and low – based

---

[2]Comola and Fafchamps (2016) implement a partner selection experiment with competition. They show that experimental subjects are quite adept at competing for a good partner and almost always converge to a stable match. Here the focus is on the role of other-regarding preferences in selecting a match or choosing a pie allocation.

on their performance in a real effort task. Type partly determine payoffs. In each session, we have 6 participants of each combination of type and category. The payoff matrices used in the experiment are chosen to enable us to discriminate between different types of other-regarding preferences: pro-social preferences (Fehr and Schmidt 1999; Bolton and Ockenfels 2006); invidious preferences (Blanchflower and Oswald 2004; Fafchamps and Shilpi 2008); and preferences for efficiency (Charness and Rabin 2002). To achieve this objective, payoff vectors are designed such that some participants receive higher average payoffs than others. If participants are inequality averse, the behavior of high payoff subjects reveals variation in expressed altruistic preferences across treatments, while the behavior of low payoff subjects reveals variation in expressed invidious preferences.

The first treatment we introduce is a *dictator allocation decision* involving the dictator and three other subjects. Each group of four subjects includes one high and one low type from each category. Participants are asked to choose between two divisions of payoffs among the four of them, with no reference to partner selection. The second treatment revolves around the same allocation problem, but the decision is framed as a *choice between a high or low partner* from the other category. The unchosen partner is then automatically matched with the remaining participant from the decision maker's category. The match pattern determines payoffs. In the third treatment the allocation problem is also framed as the choice of a high or low partner, except that each potential partner is not a uniquely (but anonymously) identified subject but is selected at random among the six participants in that type-category cell. In all three treatments, the decision of one of four players is selected at random and implemented in a unilateral manner to determine the payoff of all four. Going from Treatment 3 to Treatment 2 changes the size of the group (large group vs. small group) but keeps the decision as a partner selection decision. Going from Treatment 2 to Treatment 1 changes the type of decision from partner selection to allocation decision. In the first treatment, the implications that decisions have on others' material payoffs are obvious. Although they are the same in

4

Treatments 2 and 3, they become increasingly less salient as we move from Treatment 1 to Treatment 3.

In our three experimental treatments we find that participants mostly follow their material self-interest. This is in line with numerous studies of behavior in market games. We nonetheless find that the propensity of high payoff subjects to behave altruistically is significantly lower in the large group Treatment 3 than in the small group Treatment 2. We find a similar, albeit smaller difference between Treatments 2 and 1. This is consistent with the idea that subjects are more altruistic in a pure allocation decision than in a match selection decision – possibly because, in the former, the impact of their decision on others is more salient. We also uncover some evidence that, in Treatment 2 and especially Treatment 3, low payoff subjects are more likely to behave in an invidious manner – i.e., they are more likely to select a partner that reduces others' payoff at the expense of a reduction in their own payoff. As a result of the combination of these two sources of behavioral variation, aggregate efficiency is lowest in Treatment 3 and highest in Treatment 1.

The paper is structured as follows. Section 2 presents the experimental design and the different treatments. The testing strategy and empirical results are presented in Section 3 and we conclude in Section 4.

2   Experimental Design

Each experimental session includes 24 subjects. To avoid contamination from one treatment to another, participants in a session only play one treatment – i.e., we use a 'between subject' design. Subjecting participants to multiple treatments would have exposed them to multiple frames, thereby revealing the object of the experiment. This would have allowed contamination across frames and probably triggered experimenter-demand effects – hence making experimental results much harder to interpret. For this reason we opted for a 'between subject' design instead of a 'within subject'

design. Participants do, however, play the same treatment several times, with a different payoff matrix each time.

2.1 Stages

Within each session the experiment is divided into two stages. In the first stage, the pool of participants is divided equally and randomly into two categories A and M.[3] The two categories are intended to represent, in an uncontextualized way, the two sides of a matching game, e.g., bride-groom, employer-employee, or hospital-intern. Having been assigned to a category, each participant individually completes a computerized task. Subjects are asked to do simple calculations for a period of 3 minutes – additions or multiplications depending on the category, A or M, to which they have been randomly allocated. Based on their performance in the task relative to other participants in the same session and category, they are assigned one of two types – bottom 50% or top 50%. Here we denote the two types simply as 'high' and 'low'.

In the second stage of the experiment, participants are first informed of their type (high or low) and then play six consecutive rounds of an assignment game. No feedback is provided to participants about the choices of others until the end of the experiment, at which time they are only told their final payoff. Since payoffs are based on one randomly selected round, it is impossible for participants to infer the choices of other participants. The purpose of this is to rule out repeated games and strategic play: each round is de facto a dictator game with anonymous others and no feedback.

We consider three different versions of the second stage. In the first two treatments, participants are assigned to groups of four. Each group consists of a low and high type participant from each category.For each round, each participant in each group selects his favorite pie/allocation. At the end of the session, one subject from each group and one round are selected at

---

[3]In the experiment these categories are refereed to as 'Addition' and 'Multiplication' – see below.

random, and his/her choice determines the payoff of all four participants in the group. The structure of the game thus resembles a dictator game with four players.

The scenarios participants are confronted are perfectly equivalent across treatments in terms of their implications for efficiency and income distribution. The only variation is the salience of these implications. Importantly, we provide the same information in all treatments.

We now turn to the details of the protocol for each treatment.

Treatment T1 - Earnings division: In this treatment participants are told that they are randomly allocated in groups of 4, composed of 2 M participants (one high and one low) and 2 A participants (one high and one low). They are asked to choose between two distributions of earnings that correspond to the earnings distribution in T3 and T2 (see Appendix 1). In Treatment 1 (T1) choices are presented as a selection between two 'pies' divided into four possibly unequal slices. Each choice is associated a single pie that represents the division of earnings between the *four* people in the group. Thus, the implication of the decision for efficiency and income distribution is obvious and salient in this treatment. This treatment is also the closest to the dictator game designs used in the literature on social preferences (for a review, see Fehr and Schmidt 1999).

Treatment T2 - Partnership formation - small groups: In the second treatment, participants are told that they are randomly allocated in groups of 4, composed of 2 people who did the multiplication task (one high, one low) and 2 people who did the addition task (one high, one low). They are asked to indicate whether they would prefer to form a partnership with the low or high type participant from the other category (they are asked to tick one of two boxes (top 50% or bottom 50%) at the bottom of the answer sheet). They are shown how the payoffs would be distributed for each possible partnership (high A/high M, high A/low M, low A/low M, low A/high M). They are also made aware of the implications of their choice for the 2 other participants in their group (see Instructions for Treatment 2 in Appendix 1). The scenarios are presented on an answer sheet (see

7

Appendix 1) in a manner similar to Treatment 3, except that we now write explicitly the implication of the partnership decision for the other people in the group. For example, if a participant in category M, say, selects the high type in category A as partner, this also determines the payoff of the two remaining players since they can now only be matched with each other. As in Treatment 3, final payoffs are determined by randomly selecting one round and one subject from each group, and letting his/her choice of partner determine the payoff of all four participants in the group.

Treatment T3 - Partnership formation - large groups: In Treatment 3 (T3), all 24 participants in a session play a partner selection game together as follows. Participants are organized in groups of 24 divided equally into A and M categories. Each participant is asked to select one of two possible types of partners, high or low, among players of the other category (see Instructions for Treatment 3 in Appendix 1). If their choice is implemented, we randomly match them with one of the 12 people from the other category. The scenarios are presented on an answer sheet (see Appendix 1). They are shown the distribution of earnings associated with the four different types of partnership (high A - high M, low A - low M, high A - low M, and low A - high M), including those that do not involve them. These choices are illustrated graphically with colored pies. The size of the pie represents the total earnings to be shared. Each earnings division corresponding to each partnership is represented with a pie division. Participants are asked to tick one of two boxes (top 50% or bottom 50%) at the bottom of the answer sheet.

Since the 24 participants are divided equally between categories A and M, and subsequently divided equally between low and high type within each category, there are six participants in each category $\times$ type[4]. Because players are anonymous and there is no feedback, the choice of each player resembles Treatment 2 except for the larger number of players. They are also shown how the payoffs would be distributed for each possible partnership,

---

[4]If only 16 or 20 participants showed up to the session, there were 4 or 5 people of each group (category and type).

as in Treatment 2. But they are not told anything explicitly about the implications of their choice for others. To understand these implications, they need to understand that by choosing a partner of a certain type, they prevent one person from their own category to be matched to a partner of that particular type.

Payoffs in Treatment 3 are determined as follows. We first select one category (A or M) at random as well as one of six rounds. We then aggregate the choices of the participants in the selected category. If there is excess demand for one type, then the scarce type is allocated in a random manner between those who have expressed a preference for it. For instance, say category A is selected. Of the 12 participants in category A, 8 have selected to match with a high type. Since there are only 6 high types in category M, each of the 8 participants is allocated to a high type match with a probability equal to 6/8, and a low type match with probability 2/8. The 4 participants who have selected to match with a low type get their choice of type. The payoff determination process is explained in detail to participants before the experiment (see Instructions in Appendix 1 for details).

In treatment T1, it is obvious by design that selecting a pie affects one's payoff and that of three other players. In treatment T2, it is clear to each player that their choice affects the payoff of the partner they choose. Given the payoff determination rule, they can also deduce that selecting one partner de facto forces the other two players together. Alternatively they may follow a market logic and convince themselves that their choice directly affects only one other player, and hence that their other-regarding preferences only apply to that player.

In treatment T3 players can, as in treatment T2, clearly see the effect of their choice on their – yet to be determined – *partner*. By taking one possible partner away from the choice set of other participants, they also de facto limit the choices of other players. This effect is less salient than in treatment T3, however, an issue that we discuss in detail in Appendix 2. Because several rounds of randomization are needed to assign payoffs, Treatment 3 may blur the sense of responsibility that participants associate

with their actions.

2.2 Payoffs

Payoffs in the second stage of treatments 2 and 3 represent how gains from matching are shared between matched partners. It is important to realize that in each T2 and T3 scenario participants ultimately choose between two possible pairings: (1) high A-high M / low A-low M or (2) high A-low M / low A - high M. The first corresponds to positive assorting (i.e., high-high or low-low), the second to negative assorting (i.e., high-low or low-high). Each of the pairings has an associated total payoff for the four participants affected by someone's choice. Hence to each scenario is associated two possible efficiency values. In some scenarios positive assorting is efficient; in others negative assorting is efficient. Treatment T1 mimics these differences albeit without explicit partner choice and assorting.

As shown in Table 1, whether positive or negative assorting maximizes someone's material payoff varies by category and type, and according to the sharing rule associated with a particular scenario. Gains are always shared equally if both partners are of the same type (both high or both low). If partners are of different types, the division of payoffs differs from game to game. We call each payoff matrix a scenario.

In a session, all participants play 6 different scenarios. The scenarios are the same for all the participants in the same session. The same set of scenarios/payoff matrices is used for the three treatments. To facilitate understanding, let us take the 4 people partnership selection setup as a benchmark (Treatment 2). Players of each group and type receive a scenario where they are asked if they would like to partner with a high or low type player of the other group. The payoffs associated with this choice are represented graphically (see Appendix 1 for screen shots). We use a set of 17 different scenarios. In 10 scenarios, payoffs are such that negative assorting (e.g., low A with high M) is more efficient. In 6 scenarios, payoffs are such that positive assorting is efficient. We also have one scenario where positive

10

and negative assorting are both efficient.

We also vary how the gains from a match are divided. Continuing with the 4 people partnership selection treatment as benchmark, we implemented three different division rules. These are summarized in Table 1, which shows how the joint payoff of two types of participants are divided between them. As mentioned earlier, the sharing rule is always equal if both types are the same. We only vary this sharing rule when the participants are of different types (one high and the other low). That is, a low M type will be asked if she wants to pair with (1) a low A or (2) a high A type and the implications of her choice will be that in case (1) the joint payoff is divided equally (but the two high types may receive a much higher payoff), while in case (2) the division varies across different scenarios (either she gets 1/2, 1/3, or 1/6 of the value of the match). We implement the same payoff structure in the other treatments, but the way it is presented differs (see Appendix 1 for screen shots).

Table 1. Possible sharing rules		
	Low A	High A
Low M	1/2,1/2	1/2,1/2 1/3, 2/3 1/6, 5/6
High M	1/2,1/2 2/3, 1/3 5/6, 1/6	1/2,1/2

The scenarios are summarized in Table 2. The session-specific six different scenarios were chosen among the set of 17 so as to permit as unambiguously as possible the assignment of an individual subject to a specific preference archetype. Correct assignment nonetheless requires that a subject play consistently according to a single archetype. We come back to this in Section 3.2.

High types have higher payoffs on average, and low types have low payoffs. To help identify other-regarding preferences, we introduce a further payoff differentiation between low A and low M payoffs such that low M types get, on average, lower payoffs than low A types. Throughout the analysis we present results broken down by these three payoff categories.

2.3 Research questions and hypotheses

As mentioned earlier, the main difference across treatments is the framing of the decision. To our knowledge, this experiment is the first to rely on framing to induce subjects to internalize the effect of their choice on others. We conjecture that, when faced with allocation decision, people are more likely to apply heuristics taken from of choices made within the household or family where norms of behavior usually involve redistributive considerations. As a result, subjects may act in a more altruistic manner and thus pick a more efficient solution. In contrast, when a decision looks like something subjects would do in a competitive setting, people are more likely to behave in a more competitive – and thus more selfish if not more rival – manner. This is because norms of behavior allow self-interest in competing for mates and for goods.

Treatment T1 is couched as an allocation process while Treatment T3 is presented as a match selection choice in an anonymous environment. We are mainly interested in contrasting T1 and T3, but going from one to the other varies two features at the same time: (1) the decision is either an allocation decision or a match selection decision; and (2) the consequences of one's actions on others are easier to realize in T1 than T3. To distinguish between these two possible effects, we introduce intermediate treatment T2: as in T3, T2 involves the choice of a match; but as in T1 the implications of the subject's choice is on a specific set of individuals. The latter feature makes it harder for the subject to avoid noticing the effect of their choice on others.[5]

Comparing T2 to T1 gives the pure effect of framing a decision as a match selection instead of a pure allocation choice. The main channel of influence here is a heuristic effect: people recognize a familiar situation and behave accordingly. Comparing T3 to T2 gives the additional effect of making

---

[5]We considered running a fourth treatment framed as an allocation decision involving self and three randomly selected subjects, but concluded that the artificiality of the setup would confuse subjects and yield unusable results.

the effect of the subject choice on others less salient. There are several possible reasons why salience may affect choices. Although the experiment is not designed to distinguish among these explanations, it is nonetheless of interest to identify their underlying mechanism, so as to enable the reader to form an opinion on their possible relevance in our setting.

The first possibility is bounded rationality: the harder it is for people to realize some of the consequences of their decisions, the more they focus on the consequences they can most easily identify. This idea is related to the literature on base-neglect (Grether, 1980), narrow-backeting (Rabin and Weiszacker, 2009), and correlation neglect (Enke and Zimmermann, 2013).

A second possibility is a moral wiggle room effect (Dana et al. 2007 and Grossman, 2014): participants do realize the consequences of their actions on others, but the fact that they are less salient provides them with an excuse for behaving more opportunistically. In the context of our experiment, subjects may for instance expect (or pretend to expect) others to act altruistically so as to accommodate their own selfish preferences. We call this the accommodation hypothesis. We show in Appendix 2 that the accommodation hypothesis is largely a fallacy – i.e., it is an excuse to act opportunistically.

The literature on trolley experiments (e.g., Green 2011) provides a third possible channel of influence: subjects who have clear preferences on outcomes may nonetheless deviate from them to avoid becoming personally implicated in the selection of an outcome. This typically occurs because they regard certain choices as morally repugnant. Yet, if multiple layers of intervening steps are added between their own action and the final outcome, subjects are more likely to select the outcome they prefer. This reasoning can be applied to our setup by noting that, in T3, several randomization steps are required to select a final outcome. Applied to the contrast between T2 and T3, the reasoning is that selfish subjects may nonetheless act altruistically in T2 because acting selfishly is morally reprehensible. But in T3 the transformation of a selfish choice into a selfish outcome is mediated by several randomization steps which exonerate the subject. We call this

the dilution hypothesis and develop it formally in Appendix 2 to compare it to the accommodation hypothesis. The main conceptual difference between the dilution hypothesis and moral wiggle room is opportunistic intent, which is present in the latter but not in the former.

3 Analysis and results

The experiment took place in May and November 2011 at the experimental laboratory of the Nuffield Centre for Experimental Social Sciences (CESS) of the University of Oxford.[6] 284 participants took part in total. We ran 13 sessions in all (5 for T1 and 4 for T2 and T3). If less than 24 participants showed up, we ran the session with either 20 (4 sessions) or 16 participants (one session) and we divided participants in types and groups equally. Participants earned £10.28 on average. No pre-selection criterion was used, all participants were recruited through the CESS subject pool database using ORSEE. The experiment was implemented with pen and paper.

The first objective of the experiment is to test whether play is systematically different between treatments and, in particular, whether decisions are more sensitive to the payoffs of others and the saliency of the implications of choices on others. The second objective is to test what type of preference is most able to account for observed behavior. We report on these two objectives in turn.

3.1 Sensitivity to the payoffs of others

Throughout we denote by π_i the payoff of subject i and N_i be the set of four players affected by i' choice.[7] We define efficiency as the sum of the payoffs of the four participants affected by the choice of a single player.

---

[6]We ran the sessions corresponding to Treatment 3 in June 2011 and those corresponding to Treatments 1 and 2 in November 2011.

[7]In T3, the exact individuals affected are unknown prior to ex post randomization, but their payoffs are known since, by design, they belong to the four category × type groups. See Appendix 3 for a formal demonstration.

3.1.1 Variation across treatments

We begin by showing that play varies systematically with treatments. The right panel of Table 2 presents the raw aggregate distribution of choices for each scenario. We present the share of participants choosing a partner of the same type. We observe large differences across types and across scenarios.

To analyze these differences, we begin by regressing the choices w_{its} of subject i of type t in scenario s on treatment dummies and session dummies. All standard errors are clustered at the individual level to account for possible interdependence across decisions for a given subject. We summarize the results of these regressions in Table 3 for different types of choices. We consider four types of choices, each presented in a separate panel. In the first three columns of each panel are presented the predicted value of w_{ist} for each of the three treatments. Results are presented separately for subjects assigned to high payoff, low payoff in the A category, and low payoff in the M category. As indicated earlier, low payoffs for subjects experimentally assigned to the A category are higher than for those assigned to the M category.

The last three columns present the test statistic and p-value of F-tests of equality of means between treatments. All F-tests are based on estimated regressions and thus correct for clustering at the individual level. We present three separate tests: (1) whether there is no difference between the treatments, i.e., whether T3=T2=T1; (2) whether there is a significance difference between T2 and T1 – this estimates the pure effect of framing the decision as a match selection vs. an allocation choice; and (3) whether there is a difference between T3 and T2 – this estimates the salience effect associated with anonymous mates between formally selected ex ante or ex post.

We first report in the upper left panel of Table 3 the proportion of times that subjects pick the choice that maximizes their individual payoff. We see that this happens most of the time, with little difference across treatments

– with only one exception, differences are not statistically significant. We also note that the proportion of choices consistent with selfish preferences is highest for participants with a low average payoff and lowest for participants with a high average payoff.

The upper right panel of Table 3 shows the proportion of times that a subject picks the choice that maximizes the payoff of the other three players affected by their decision. As before, we report proportions under each of the three treatments as well as the results of F-tests of equality of means across treatments. We find that, on average, the proportion of choices that maximizes others' payoffs decreases systematically as we go from treatment T1 to treatment T3. This is true for all player types, but is strongest and highly significant for high payoff participants. For the lowest payoff types, those in category M, the difference is not statistically significant at standard levels. For low and high payoff participants, the fall in other-regarding choices is significant both between T1 and T2 and between T2 and T3. In other words, both dimensions of treatment – switching from allocation to match selection, and reducing salience – reduce other-regarding behavior, albeit less among those with the lowest payoffs – who act more selfishly throughout.

In many cases, the choice that maximizes the payoff of others also maximizes one's own payoff. This explains why it is possible for, say, high payoff subjects in treatment T3 to maximize their own payoff in 80.7% of the cases while at the same time maximizing the other players' payoff in 52.3% of the cases. To investigate this issue further, in the lower half of Table 3 we split the choices that maximize others' payoff depending on whether doing so also maximizes one's own payoff or not. A 'good Samaritan' spirit corresponds to the case where participants maximize others' payoffs even though doing so reduces their own. This case is displayed in the lower right panel of Table 3. In the lower left panel we show the proportion of choices that maximize others' payoffs when doing so also maximizes the subject's own material payoff. Such choices are consistent with efficiency consideration but are less remarkable from an equity point of view.

16

In the lower left-hand panel we find that participants are more sensitive to aggregate efficiency in treatment T1 than in partner-selection treatments T2 and T3. This is true for all three payoff categories, but differences in behavior are statistically significant only for high payoff participants.

Turning to the lower right-hand panel, we see that the 'good Samaritan' spirit is generally highest in treatment T1 and lowest in T3. Difference in behavior are large in magnitude, especially among lowand high payoff subjects, and they are statistically significant for all payoff categories between treatments T1 and T2. In other words, moving from an allocation to a match selection frame significantly reduces subjects' willingness to reduce their own payoff in order to benefit others. This is a remarkable result, and we believe this is the first time that such an effect is documented.

To investigate these findings in more depth, we test whether treatments affect the sensitivity of participants' choice to differences in payoffs between the two options they face. Let $\Delta\pi_i \equiv \pi_i^h - \pi_i^l$ be the gain in i's payoff from choosing a high partner (or equivalent allocation in T1). Similarly let $\Delta\pi_{-i} \equiv \sum_{j\neq i, j\in N_i}(\pi_j^{h_i} - \pi_j^{l_i})$ be the gain in the payoff of the other three players affected by i's choice. To calculate the marginal effects of $\Delta\pi_i$ and $\Delta\pi_{-i}$ on the probability of choosing a high partner (or equivalent allocation in T1) we estimate a regression model of the form:

$$y_i = \Delta\pi_i \otimes T \otimes X + \Delta\pi_{-i} \otimes T \otimes X + \varepsilon_i \tag{1}$$

where T is a vector treatment dummies (i.e., T1, T2 or T3), X is a vector of dummies for player types (i.e., high type, lowA type, or low M type). $\Delta\pi_i \otimes T \otimes X$ is shorthand for all the possible interaction terms between them and similarly for $\Delta\pi_{-i} \otimes T \otimes X$.[8] Session fixed effects are included throughout.

We report in Table 4 the estimated marginal effects with their t-value for each of the $T\otimes X$ combinations.[9] What the Table reveals is that choice sensitivity to the own payoff difference between the two options is comparable

---

[8]In other words, we include terms in π_i, T, X, TX, $\pi_i T$, $\pi_i X$ and $\pi_i TX$ where T and X are themselves vectors of dummies. Similarly for π_{-i}, T and X.

[9]Regression (**??**) is estimated using logit with standard errors clustered at the individual level. Virtually identical results obtain using a linear probability model.

across treatments: a one unit increase in own payoff increases the probability of choosing the more beneficial option by 5 to 6 percentage points across all subject types and treatments. Since the standard deviation of $\Delta\pi_i$ is 4.67, this is a large effect.

In contrast, choice sensitivity to $\Delta\pi_{-i}$ varies dramatically across treatments and subject types. For high types – who on average earn higher payoffs – sensitivity to $\Delta\pi_{-i}$ is absent in treatment T3 but present in the other two treatments. The effect is large in magnitude: a one standard deviation (i.e., 7.67) increase in $\Delta\pi_{-i}$ raises the probability of choosing high by 5.6% in treatment T2 and 8.9% in treatment T1. In treatment T3 the effect is numerically 0. This suggests that among these players, redistributive considerations are eliminated in T3, a finding that is consistent with the dilution hypothesis.

Results are different for low types. Here we find that, under anonymous partner selection T3, participants in the low A category are at the margin *less* likely to choose a high partner if other players benefit more from that choice, controlling for their own payoff gain. The effect is large in magnitude: a one standard deviation increase in $\Delta\pi_{-i}$ reduces the probability of choosing high by 8.3% for low A subjects. The effect of one's choice on others' payoffs is not significant for low types in Treatments 1 and 2.

These findings are to be read in the context of the literature on inequality aversion (e.g., Fehr and Schmidt 1999, Okada and Riedl 2005). Experimental evidence has suggested that individuals display a desire to reduce the difference between their payoff and that of others both from above and from below. In other words, if a subject has a high payoff relative to other participants, this subject is often observed taking redistributive actions that reduce the difference between her payoff and that of other participants. This is consistent with the behavior of high types in our experiment, who have higher average payoffs and, in treatments T2 and T1, are more likely to choose an action that increases the payoff of other participants who, on average, earn a lower payoff. In other words, high types often choose an action that reduces the difference between their payoff and the lower payoff of others, but do so

more often when the choice is framed as an allocation decision than a match selection.

Also according to inequality aversion, a subject who has a low payoff relative to others often takes actions that increase her payoff at the expense of others. This is what low A types do in T3: controlling for their own payoff, these subjects are more likely to take an action that reduces the payoff of others. What is interesting is that, in our experiment, the two behaviors do not coexist: altruistic behavior (inequality aversion from above) is only present in treatments that emphasize the effect one's choice has on several others; envy or spite (inequality aversion from below) is only present in the treatment that blurs the effect of one's choice on others. This suggests that the dilution effect reduces altruistic/redistributive/efficiency considerations, but not envy which is, rather, exacerbated by anonymity. In other words, the two sides of inequality aversion respond differentially to an anonymous partner selection environment: altruism and redistributive norms are blunted by it, while envy is heightened. Next we show that the two effects together combine to reduce the efficiency of participants' choices.

3.1.2 Choices and efficiency

We now investigate the relationship between treatment and the efficiency of individual choices. We ignore the 60 observations in which choices generate the same total payoff for the four players. We focus on whether the choice the subject makes is efficient or not. Across all subjects we see that, in treatment T1, participants choose the efficient allocation in 70.2% of the observations. This proportion falls to 66.7% in T2 and 60.9% in T3. These differences are statistically significant at the 1% level.

We reproduce this finding in the first column of Table 5. Treatment T3 is the default category so that reported coefficients capture the efficiency gain of individual choices in T2 and T1 relative to T3. We see that efficiency is higher in T2 and, especially, T1 than in T3. The difference between T1 and T2, however, is not statistically significant.

19

In the other columns of Table 5 we disaggregate the results and regress the efficient choice dummy on treatment for each of the three types of participants. The results indicate that the T1 and T2 are associated with more efficient choices for both high types and low A types. For low M types, the treatment effects are not significant although, in terms of average efficiency across all treatments, low M types are not statistically different from other payoff categories. It appears that an anonymous partner selection setting such as T3 leads higher payoff participants to behave in a more selfish manner while in treatments T2 and, especially T1, they may feel moral pressure to behave altruistically towards lower payoff participants. In contrast, low M types do not seem to have these concerns. This could be because they feel more entitled to pursue their self-interest, having been assigned to the lowest payoff category at the onset of the experiment.

3.2 Preference archetypes

In this section we check the robustness of our findings using a more structural approach. Our objective is to assign participants to preference archetypes characterizing the form of their other-regarding preferences, and we test whether archetype assignments vary across treatments. Since assignment to treatment is random, variation in revealed preferences across treatments would signal that choices follow different preferences depending on the way decisions are framed. As discussed in the research design section, this could arise for various reasons, one of which is that different frames trigger different heuristic norms of behavior. If we know what archetypes best capture the behavior of a large fraction of the population in a given environment, we may be in a better position to predict what types of behavior to expect in that environment.

We selected the payoff matrices (or scenarios) used in the experiment so as to facilitate the assignment of participants to a shortlist of commonly used archetypes. Some of the archetypes we considered received no support in

the data and are ignored here.[10] The three archetypes on which we focus on here are the following (as defined in Engelmann and Strobel, 2014, Charness and Rabin 2002, Cooper and Kagel 2013):

1. *Selfish:* Chooses the allocation that maximizes own *absolute* payoff π_i.

2. *Invidious*: Chooses the allocation that maximizes own *relative* payoff $\pi_i - \frac{1}{3} \sum_{j \neq i, j \in N_i} \pi_j$.

3. *Maximin*: Chooses the allocation that maximizes the minimum payoff among the four affected individuals $\max \min \pi_j$, $j \in N_i$. These preferences capture a simple form of inequ

We seek to assign each participant to the archetype that best describes their behavior over the six rounds. Different participants may follow different archetypes. We start by identifying participants who follow a single archetype perfectly over the 6 rounds of the session, and we observe what proportion of participants we can assign in this manner. We also report the proportion of subjects whose behavior fits more than one archetype, and which proportion fits none of them.[11] Results are shown in Table 6.

The archetype most consistent with observed choices is the 'selfish' archetype, with some systematic differences across treatments and types. Overall, fewer participants are assigned to the selfish archetype in treatment T1 than in other treatments. The difference is strongest for high types. This confirms earlier results indicating that these participants behave in less selfishly when the experiment is framed as an allocation process. Low types behave more

---

[10]We originally allowed for three archetypes in addition to the three listed here: (1) efficiency-only (i.e., subjects choose the allocation that maximizes the total joint payoff); (2) homophily (i.e., subjects choose a partner of the same high or low type); and (3) equity-only (i.e., subjects choose the allocation that minimize absolute inequality defined as $\sum_{j \in N_i} |\pi_j - \overline{\pi}_i|$). Because these archetypes have very low predictive power in our sample, the ML search fails to converge when they are included.

[11]Formally, for each archetype k, we calculate $\Delta u_i^k \equiv u_k(\pi_i^h) - u_k(\pi_i^l)$ where preference function $u_k(.)$ is that corresponding to archetype k. For each subject we then count the proportion of rounds for which the subject behaves in accordance to archetype k, that is, for which $y_i = 1$ if $\Delta u_i^k > 0$ and $y_i = 0$ if $\Delta u_i^k < 0$. We then assign the archetype with the highest count to the subject. If there are two or more with an equal number of counts, we say that the subject fits multiple archetypes. We ignore cases in which $\Delta u_i^k = 0$ because they are uninformative.

selfishly than high types, again confirming earlier results. A sizeable proportion of low payoff subjects exhibit invidious preferences, more often so in T3, in line with earlier results. Maximin preferences are followed by a non-negligible proportion of low types. For the latter, however, selfish and maximin preferences typically coincide for low type subjects who, by construction, have the lowest payoff.

At the bottom of the table we report the proportion of participants who were assigned to multiple archetypes, and those whose behavior does not fit any. We see that the behavior of low types often satisfies more than one type. At the bottom of the table, we report the proportion of subjects failing to fit any archetype. This typically arises because their behavior does not follow one archetype consistently. A non-negligible proportion of subjects fall into this category, especially in treatment T1.

One possible explanation for this is that people make mistakes: they may have preferences that follow one of our archetypes but, due to inattention or lack of interest, they occasionally make choices that do not correspond to their underlying preferences. To further investigate this possibility, we estimate a mixed maximum likelihood model. The details of the estimation methodology are presented in Appendix 3. Estimates of posterior probabilities are summarized in Table 7. We find that most participants fit the selfish archetype best, but high types are less likely to act selfish in the allocation treatment T1. In contrast, participants with the lowest average payoff (low M types) overwhelmingly play selfish, even in T1. A number of players are classified as having invidious preferences, but only in T3. Finally, we find that a number of subjects in the low payoff category A are classified as maximin players, especially in treatment T1. These results largely confirm our earlier findings.

4 Discussion and conclusions

We have reported on an experiment designed to test whether people exhibit different other-regarding preferences depending on whether choices are framed as allocation or match selection, and depending on the size of the group from which they select a match. We find that when choices are framed as match selection, agents are less likely to sacrifice their own material well-being to increase the well-being of others. But some are more willing to sacrifice a higher payoff to reduce the difference between their payoff and that of others. Similarly, subjects behave more selfishly if they are in a larger group setting.

These findings are broadly consistent with the literature on inequality aversion (e.g., Fehr and Schmidt 1999, Okada and Riedl 2005), but with a twist. Experimental subjects with a higher than average payoff exhibit some altruistic or redistributive concerns, but more so when choices are framed as an allocation problem. In contrast, when asked to select a partner in a large anonymous setting, high payoff players seldom behave altruistically and simply maximize their own material payoff. In contrast, subjects with the lowest average payoff display no altruistic behavior or concern for efficiency in a small group setting, but exhibit spiteful preferences in a large anonymous setting. In other words, we get the two 'sides' of inequality aversion (altruism and spite), but not in the same setting.

These findings have a number of implications. First, they suggest that it may be possible to improve the allocative efficiency of first-come-first-serve assignment processes in which some individuals are put in a position to unilaterally select a match. In particular, consider assignment process in which buyers enter a queue and only make a selection once they reach the top of the queue. In the typical situation, the choices the buyer makes are constrained by choices made by buyers whose turn came first – i.e., they can only choose among what is left. But they are unconstrained by the needs/willingness-to-pay/consumer surplus of buyers further down in the queue. The evidence provided here suggests that making buyers aware of

the consequences of their choices on others may alter their behavior in a less rival, more altruistic direction – especially if their choices are couched as an allocation decision involving multiple individuals, instead of a pure match selection. For instance, someone at the head of the queue for a kidney transplant may be asked to assign the kidneys that have become available between themselves and the next individuals in the queue – instead of simply selecting one kidney for themselves.

Secondly, our findings potentially cast some new light on the relationship between altruistic preferences and the development of market institutions. Fafchamps (2011) argues that economic development requires a change in allocation processes away from allocation within the household or extended family to allocation within markets or within hierarchical organizations. This transformation requires a change in social norms from risk sharing in long-term gift exchange to contract compliance in an anonymous market setting. In a gift exchange allocation process, efficiency requires that individuals make choices that are altruistic or efficiency-seeking. In market exchange, efficiency can be achieved through competition alone; altruistic or efficiency-seeking behavior is not required. To the extent that the behavior of our experimental subjects can be interpreted as reflecting context-specific norms, they fit this pattern to a large extent. We do, however, also find that less fortunate participants occasionally select a partner so as to prevent them from achieving a higher payoff. It is unclear whether competition is sufficient to counter the inefficiency produced by such choices. More research is needed.

The findings also raise a more fundamental question: Why, even with minimal contextualization, do human subjects respond the way they do to differences in the type of choice they make? Is it possible that the human brain processes moral choices in a way that systematically reduces altruistic behavior and reinforces spite in an anonymous partner-selection setting relative to a small group setting? This question is of policy relevance too and related to the idea of nudging: Is it possible to nudge people into making decisions that are more altruistic and efficient in situations that are essentially

competitive?

The literature on trolley experiments (e.g., Greene 2012) suggests one possible avenue of inquiry suggests that people care about their social image and feel less guilty about the consequences of their actions when these consequences seemingly depend on mechanical devices, random events, and choices made by others.[12] Andreoni and Berheim (2009) show that concerns about being *perceived* fair sometimes dominate fairness concerns themselves. The idea that people care about how their actions will be perceived is also documented in Cox and Deck (2006) and Castillo and Leo (2010). Related to this, Toussaert (2017) shows that noisier perceptions (due to difficulties of signalling pro-socialiaty), discourage pro-social behavior. Delegating decision-making has also been shown to induce more punishment and less pro-social behavior (Bartling and Fischbacher, 2012). If correct, this interpretation suggests that markets – and partner selection problems in large populations – blunt other-regarding preferences by diluting the perceived effect that actions have on the welfare of others, thereby eliciting less guilt for failing to follow norms of acceptable behavior that apply in small groups. We offer in Appendix 2 a simple model of such preferences.[13] Further work is needed on the origin of other-regarding preferences, and especially the extent to which they are shaped by the decision domains over which altruistic norms apply.[14]

---

[12]Mikhael (2011) goes so far as to suggest that this is because the human brain processes moral choices of cause and effect by applying syntaxic rules. This means that 'pushing the man to his death' generates more guilt than 'pushing the button that activates the lever that opens the door that pushes the man to his death' even though the ultimate consequence is the same.

[13]The difference between treatments T2 and 3 similarly is in the *process* by which payoff are generated, not in the actual payoffs themselves. Dilution as defined here can thus be seen as a situation in which people's preferences depend not just on payoffs but also on the way these payoffs are obtained. If this interpretation is correct, this paper can be seen as following in the footsteps of Charness and Rabin (2003) who demonstrated that people have preferences over process, not just final outcomes.

[14]This line of thought also relates to recent work on how social preferences are shaped by the risky nature of the environment. Brock et al. (2013) show that risk makes dictators less altruistic; Cappelen et al. (2013) show that the nature of the risk (luck versus choices) plays an important role in shaping social preferences.

5 References

Agarwal, Nikhil and Paulo Somaini (2016). "Demand Analysis using Strategic Reports: An application to a school choice mechanism", Stanford Graduate School of Business, November 2 (mimeograph)

Andersen, Steffen, Glenn W. Harrison, Morten I. Lau, and E. Elisabet Rutström, (2008). "Eliciting Risk and Time Preferences," *Econometrica*, 76(3): 583-618, 05.

Andreoni J. and D. Bernheim, (2009) "Social Image and the 50-50 Norm: A Theoretical and Experimental Analysis of Audience Effects." *Econometrica* 77(5), 1607-1636

Baccara, Mariagiovanna, Sang Mok Leey, and Leeat Yariv (2016). "Optimal Dynamic Matching", Cal Tech, October 23 (mimeograph)

Bartling, B. and U. Fischbacher (2012), "Shifting the Blame: On Delegation and Responsibility", *Review of Economic Studies* 79(1), 67-87.

Bartling B., R.A. Weber and L. Yao (2015), "Do Markets Erode Social Responsibility?", *Quarterly Journal of Economics* 130 (1), 219-266.

Becker, G. (1973), "A Theory of Marriage: Part I", *Journal of Political Economy,* 81(4): 813-46.

Blanchflower, D.G. and A.J. Oswald (2004), "Well-Being over Time in Britain and the USA", *Journal of Public Economics*, 88(7-8), July 2004

Bolton, G.E. and A. Ockenfels (2000), "ERC: A Theory of Equity, Reciprocity, and Competition", *American Economic Review*, 90(1): 166-193, March.

Bowles, S. (1998). "Endogenous Preferences: The Cultural Consequences of Markets and Other Economic Institutions", *Journal of Economic Literature*, 36(1): 75-111.

Brock, J.M., A. Lange, and E.Y. Ozbay (2013), "Dictating the Risk: Experimental Evidence on Giving in Risky Environments." *American Economic Review*, 103(1), 415-37.

Castillo, M. and G. Leo (2010), Moral Hazard and Reciprocity, *Southern Economic Journal* 77(2), 271-81,

Charness, G. and M. Rabin (2002). "Understanding Social Preferences With Simple Tests", *Quarterly Journal of Economics,* 117(3): 817-869,

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

Camerer, C. (2003), *Behavioral Game Theory: Experiments in Strategic Interaction.* Princeton University Press, Princeton NJ.

Cappelen, A.W., J. Konow, E.Ø. Sørensen, and B. Tungodden (2013), "Just Luck: An Experimental Study of Risk-Taking and Fairness." *American Economic Review* 103(4), 1398-1413.

Comola, Margherita and M. Fafchamps (2015), "An Experimental Study of Decentralized Link Formation with Competition", Stanford University and Paris School of Economics (mimeograph)

Cooper, David J. and John H. Kagel (2013). "Other-Regarding Preferences: A Selective Survey of Experimental Results", *Handbook of Experimental Economics*, Vol 2, J. H Kagel and A. E Roth (eds)

Currarini, S. and F. Mengel (2012), Identity, Homophily and In-Group Bias, *FEEM Working Paper* No. 37.2012

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

Engelmann, D. and M. Strobel (2004). "Inequality Aversion, Efficiency, and Maximin Preferences in Simple Distribution Experiments." *American Economic Review* 94(4): 857-869.

Enke, B. and F. Zimmermann (2013), Correlation Neglect in Belief Formation, *IZA Discussion Papers* 7372

Fafchamps, M. and F. Shilpi (2008). "Subjective Welfare, Isolation, and Relative Consumption", *Journal of Development Economics*, 86(1): 43-60, April

Fafchamps, M. (2011). "Development, Social Norms, and Assignment to Task", *Proceedings of the National Academy of Sciences (PNAS)*, 108(Supplement 4): 21308-15, December

Fafchamps, M. and D. Mo (2016), "Peer Effect Heterogeneity in Com-

puter Assisted Learning: Evidence from a Randomized Experiment", Stanford University (mimeograph)

Falk, A. and N. Szech (2013), "Morals and Markets", *Science* 10, 340(6133), 707-711

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

Fehr, E. and U. Fischbacher (2004). "Third-Party Punishment and Social Norms" *Evolution and Human Behavior*, 25, 63-87.

Grossman, Z. (2014). "Strategic ignorance and the robustness of social preferences", *Management Science* 60 (11), 2659-2665

Guth, W., R. Schmittberger and B. Schwarze (1982). "An experimental analysis of ultimatum bargaining", *Journal of Economic Behavior and Organization*, 3(4), 367-388.

Harbring, C. (2010), "On the Effect of Incentive Schemes on Trust and Trustworthiness", *Journal of Institutional and Theoretical Economics* 166(4), 690-714(25)

Henrich, J., Boyd, R., Bowles, S., Camerer, C., Fehr, E., Gintis, H., McElreath, R., Alvard, M., Barr, A., Ensminger, J., Henrich, N. Smith, Hill, K. Gil-White,F., Gurven, M. , Marlowe,F. W., Patton, J.Q., Tracer, D. (2005), "Economic man in cross-cultural perspective: Behavioral experiments in 15 small-scale societies", *Behavioral and Brain Sciences*, 28(6): 795-815.

Hoffman, E., K. McCabe and V. Smith (1996), "Social distance and other-regarding behavior in dictator games", *American Economic Review,* 86: 653-60.

Kirchler, M., J. Huber, M. Stefan, and M. Sutter (2016), "Market Design and Moral Behavior", *Management Science* 62(9), 2615-2625

Greene, Joshua (2010). "Of Trolleys and Cheaters: Automatic and Controlled Influence on Moral Judgment", video available at http://www.santafe.edu/research/videos/play/?id=a7 06e5-4533-8ebe-9ba3d3a5c131,

Greene, Joshua (2012). *For The Greater Good: How the Moral Brain Works and How it Can Work Better*, Penguin Press, New York, (forthcom-

ing)

Grether (1980), Bayes Rule as a Descriptive Model: The Representativeness Heuristic, *Quarterly Journal of Economics* 95 (3), 537-557

Ledyard, John O. (1995). "Public Goods: A Survey of Experimental Research", in J. Kagel and A. E. Roth, *Handbook of Experimental Economics*, 111-194. Princeton: Princeton University Press.

Luce, R.D., (1959). *Individual choice behavior*, John Wiley., Oxford, England

Luce, R. Duncan (2010). "Behavioral Assumptions for a Class of Utility Theories: A Program of Experiments", *Journal of Risk and Uncertainty*, 41: 19-27.

McPherson, M., L. Smith-Lovin and J. M. Cook (2001). "Birds of a Feather: Homophily in Social Networks", *Annual Review of Sociology*, 27: 415–44.

Mikhail, John (2011). *Elements of Moral Cognition: Rawls' Linguistic Analogy and the Cognitive Science of Moral and Legal Judgment*, Cambridge University Press, Cambridge

Null, Claire (2012). "Warm glow, information, and inefficient charitable giving", *Journal of Public Economics*, (forthcoming)

Okada, Akira and Riedl, Arno, (2005). "Inefficiency and social exclusion in a coalition formation game: experimental evidence," *Games and Economic Behavior*, 50(2): 278-311, February.

Rabin, M.and G. Weizsacker (2009), "Narrow Bracketing and Dominated Choices." *American Economic Review* 99(4), 1508-43.

Roth, A., Prasnikar, V., Okuno-Fujiwara, M., & Zamir, S. (1991). Bargaining and Market Behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An Experimental Study. *The American Economic Review*81(5), 1068-1095.

Schwieren, C. and D. Weichselbaumer (2010), "Does competition enhance performance or cheating? A laboratory experiment", *Journal of Economic Psychology* 31(3), 241-253

Smith, Adam. (1759) 1976. *The theory of moral sentiments*, Liberty classics, edited by D. D. Raphael and A. L. Mactie. Indianapolis:Liberty

Press.

Smith, Adam. (1776) 1909. *Wealth of nations*, Harvard Classics. volume 10, edited by C. J. Bullock. New York: P. F Collier

Smith,V.L. (1962). "An experimental study of competitive market behavior", *Journal of Political Economy*, 70(2): 111-37.

Smith, V. (1998), "The Two Faces of Adam Smith", *Southern Economic Journal*, 65(1): 1-19

Thaler, Richard H. and Cass R. Sunstein (2008). *Nudge: Improving Decisions about Health, Wealth, and Happiness*, Yale University Press, New Haven

Toussaert, S. (2017), Intention-Based Reciprocity and Signaling of Intentions, *Journal of Economic Behavior and Organiztion* 137, 132-44.

Young, P. and M.A. Burke (2001), "Competition and Custom in Economic Contracts: A Case Study of Illinois Agriculture", *American Economic Review*, 91(3): 559-573

Appendix 2. Dilution and Accommodation
1. Dilution

In this Appendix we first illustrate how altruism or redistributive concerns can become diluted in an anonymous partner-selection setting.[15] We then compare this pure dilution effect to a related effect whereby subjects fool themselves that others will accommodate their selfish choice.

The trolley experiments (e.g., Greene 2012, Mikhael 2011) suggest that people feel less guilt when the consequences of their action involve mechanical devices, random events, or choices made by others. This suggests that, say, pushing an anonymous person to their death generates more guilt than pushing a button that randomly selects one of M anonymous persons to be pushed to their death. A similar contrast characterizes the difference between treatments T2 and T3: selecting in T2 a partner that leaves other experimental subjects a lower payoff may generate more guilt than indicating in T3 a preference for a partner type that, de facto, removes one partnership from the choice set of others and has similar consequences on randomly selected individuals.

To illustrate how this idea can be formalized, we construct preferences in which individuals value the consequence of their action on others differently depending on whether they affect, with certainty, one person or one of M randomly selected individuals. Let $W_2(h)$ denote the utility gain from choosing a 'high' partner h instead of a 'low' partner l. Consider treatment T2 and let this choice be efficient, so that for a subject with sufficiently altruistic preferences, we have:

$$W_2(h) \equiv \pi_i^h - \pi_i^l + \beta \sum_{j \neq i, j \in N_i} (\pi_j^{h_i} - \pi_j^{l_i}) > 0$$

where $\beta \leq 1$ is a parameter capturing the strength of redistributive concerns, N_i is the set of four subjects that includes i, and $\pi_j^{h_i} - \pi_j^{l_i}$ is the effect that player i has on the payoff of player j in N_i when choosing h.

---

[15]More sophisticated models can be written – e.g., models in which subjects have preferences on whether they interfere or not with other subjects' choices – but they would take us too far from the object of this paper, which is primarily empirical.

In treatment T3, the effect on the payoff of others is essentially the same as in treatment T2 but i does not know which exact players will be affected. The expected efficiency gain from choosing a partner h can now be written:

$$W_3(h) \equiv \pi_i^h - \pi_i^l + \beta \sum_{j \neq i, j=1}^{24} E[\pi_j^{h_i} - \pi_j^{l_i}]$$

Realized payoffs are as in treatment T2, but the identity of the three individuals affected by i's decision has not yet been determined. It is this difference that opens the door to a possible dilution effect as follows.

Let us first consider i's randomly assigned partner and, without loss of generality, assume that this person belongs to category M. The effect of i's choice on this person's payoff is $\pi_j^{h_i} - \pi_j^{l_i}$; other possible high M partners are unaffected by i's decision so that for them the effect is 0. The total effect of i's choice on the expected payoff of high M subjects is thus:

$$\sum_{j=1}^{6} E[\pi_j^{h_i} - \pi_j^{l_i}] = \sum_{j=1}^{6} \frac{1}{6}(\pi_j^{h_i} - \pi_j^{l_i}) = \pi_j^{h_i} - \pi_j^{l_i} \tag{2}$$

Similar calculations can be done for subjects in the other two category $\times$ type groups. We get:

$$W_2(h) = W_3(h)$$

which predicts that, in the absence of dilution, individuals should make identical decisions under treatments 2 and 3.

Let us now introduce a dilution parameter $\alpha \geq 1$ and rewrite (??) as:

$$\sum_{j=1}^{6} E^\alpha[\pi_j^{h_i} - \pi_j^{l_i}] = \sum_{j=1}^{6} \left(\frac{1}{6}\right)^\alpha (\pi_j^{h_i} - \pi_j^{l_i}) \leq \pi_j^{h_i} - \pi_j^{l_i} \tag{3}$$

with strict inequality if $\alpha > 1$. The effect on the other two category $\times$ type groups can be handled in the same way. Equation (??) is equivalent to positing that individuals underweight the probability that they affect other players, and is formally similar to probability weighting in prospect theory. With α large enough, $\sum_{j=1}^{6} \left(\frac{1}{6}\right)^\alpha (\pi_j^{h_i} - \pi_j^{l_i})$ tends to 0 and we have:

$$W_3(h) = \pi_i^h - \pi_i^l$$

which corresponds to selfish preferences: with enough dilution, people no longer take into account any efficiency cost they impose on others, and pursue their own material welfare only.

2. Accommodation vs dilution

We now turn to the accommodation hypothesis. At first glance it appears that treatments T2 and T3 are fundamentally different in the sense that, in T3, how my choice affect others depends on the actions of others while in T2 it does not. This turns out to be largely an illusion, but showing this rigorously is rather intricate. Although nothing of importance hinges on this issue for our results, we discuss it here in some detail, lest it become a distraction for the reader.

We first show that, if players have the same preferences, T2 and T3 are identical in the sense that my choice affects three other players. The only difference is that, in T3, these players are yet to be determined. We then allow for the possibility that players have different preferences, and we introduce the concept of accommodation. Finally we show that, in expectation, there is, if anything, more accommodation in T2 than in T3. To the best of our knowledge, these concepts have never been formalized before.

Developing some intuition

Since all subjects in T3 play the same scenario, players in the same category (A or M) and type (high or low) face the same payoff structure. It follows that, if they all have the same preferences, they should make the same choice. If they make the same choices, the impact on others' payoffs in T3 is the same as in T2: if I take an attractive partner for myself, this attractive partner is not available to someone else and, at the margin, this forces two other players into a less profitable pairing. The only difference is that, in T3, the identity of these players is determined by chance *after* I have made my choice while in T2 the identity of these players is determined by chance *before* I have made my choice. Since the identity of other players is never revealed, the two should be equivalent – except for the dilution effect (see above).

If we allow players to have different preferences, I could delude myself into thinking that, while I am acting to maximize my own payoff, others are not – in which case my choice may be *accommodated* by the unselfish choice of others. In other words, I could hope to free ride on another player's altruism.[16] Consequently, in T3 subjects may convince themselves that they are only affecting one participant's payoff as long as there exists other players who exactly complement their choice. Because players face the same payoff structure, such beliefs are largely wishful thinking, however: why would I be the only one who is selfish. But accommodation opens the possibility that players make choices based on their (possibly self-serving) beliefs about how others play.

It therefore appears that how people play in T3 depends on expectations about others' actions while in T2 it does not. This is largely an optical illusion, though. When a player acts selfishly in T2, he may similarly convince himself that others in his group would accommodate his choice – so that he has no determinant effect on the payoffs of other players because these players would accommodate his choice. To illustrate, imagine two grooms and two brides, as in T2. Groom 1 selects the handsomest bride for himself, de facto forcing the other groom to pair with the less desirable bride. Yet groom 1 can convince himself that this is precisely the bride that groom 2 would have selected, no matter how unlikely this possibility may be. In other words, in both treatments 2 and 3 subjects can delude themselves that others will make choices that render their own decision unconstraining.

Having presented the argument in an intuitive manner, we now offer a slightly more rigorous treatment of accommodation, while discussing its relationship with the dilution hypothesis. To recall, we define dilution as the property by which people put less weight on affecting two individuals each with probability 1/2 than on affecting one individual with probability 1. We define accommodation as expecting that other players will accommodate my preferences so that I can pretend to myself that I did not deprive anyone

---

[16]Or, alternatively, I could hope that my altruism is accommodated by someone else's selfishness.

and I can therefore ignore the consequences of my choices on others.

Accommodation

To formalize the above intuitive argument, we need to distinguish between what i's preferences are with respect to others' payoff, and the realization that other individuals may choose something else than what i would choose in their place, that is, the possibility that others players have different preferences. To demonstrate this, without loss of generality we examine what is i's gain from playing high instead of low:

$$
\begin{aligned}
W_3^i(h) &\equiv \pi_i^{h_i} - \pi_i^{l_i} + \beta \sum_{j \neq i, j=1}^{N} E[\pi_j^{h_i} - \pi_j^{l_i}] \\
&= M_i^i + \beta \sum_{j \neq i, j=1}^{N} E[M_j^i]
\end{aligned}
$$

where $\pi_j^{h_i}$ is the payoff to j if i chooses to play high (i.e., to select a high type partner), $\pi_j^{l_i}$ is j's payoff if i plays low, N is the number of players in the game and $M_j^i \equiv \pi_j^{h_i} - \pi_j^{l_i}$.

The whole issue is how $E[M_j^i]$ is calculated. To illustrate how this matters, we now assume that the rules of T3 apply but we vary N. We focus on i, and assume for simplicity that his choice is selected first.

We start by considering the case where $N = 4$ (as in T2). There are three other players apart from i. Let m be the partner selected by i and let k be the player of the same category (A or M) as i but of the opposite type. In T3, the choices of i and k are first collated. If i chooses high and k chooses low, then both their choices are accommodated. Hence player i only directly affects the payoff of one person, i.e., the high player in the other category. In contrast, if i and k choose high, their selections are in conflict and only one of them can be implemented. Imagine that i's choice is selected. In this case, his choice determines the payoffs of all four players. This examples shows that i's choice only affects three other players if k does not accommodate his choice.

Let us now assume that k accommodates i's choice (i.e., chooses low)

with probability p. We have:

$$W_3^i(h) = M_i^i + \beta M_m^i + p\beta 0 + (1-p)\beta \sum_{j \neq i,m,j=1}^{N} E[M_j^i]$$

We immediately see that T3 creates an accommodation effect: when player k accommodates i's choice, i can imagine that his choice has no effect on k and the partner that player k chooses. In other words, whatever effect i's choice has on players k and his partner can be 'blamed' on k's choice, not on i's. As this example illustrate, this accommodation effect is present in T2 as well, depending on i's thought process. The key idea is that i can blame someone else for some of the choices made, and this reduces the sense of responsibility i feels for the effect of his choice on others.

This reasoning can be generalized to more players. For instance, consider the case in which $N = 8$ and continue to assume that i plays high and that other k players choose low with probability p. There are three possible choice vectors for the three k players: $\{high, high, high\}$ which happens with probability $(1-p)^3$, $\{high, high, low\}$ which happens with probability $p(1-p)^2$, $\{high, low, low\}$ which happens with probability $p^2(1-p)$, and $\{low, low, low\}$ which happens with probability p^3. We see that if the other three players exactly accommodate i's choice (i.e., the $\{high, low, low\}$ case), i can imagine that his choice has no effect at the margin on k players (and hence on anyone except m) since k players got their choice. In other cases, i's choice has an effect at the margin but only with a certain probability.

A simple example

This probability can be calculated for a simple example. Imagine $2N$ players asked to choose between two possible prizes, chocolate or marmite, each available in quantity N. Each player expresses a choice for one of them. Let M be the number of players who choose marmite. If there is excess demand for marmite, i.e., if $M > N$, the prizes are assigned at random (as is done in T3) such that the probability of getting marmite is N/M for someone who chooses marmite. Similarly if $M < N$, chocolates are assigned at random. If $M = N$ everyone gets their choice.

Now consider i's choice and imagine that i derives disutility from preventing another player from getting his choice. For simplicity let this disutility be D. Similarly let $-D$ be the utility player i derives from helping another player get his preferred choice. Without loss of generality, imagine that i prefers marmite and let i's payoff in this case be π. Let other players' choices be M_i. If $M_i = N - 1$ then i can get marmite without preventing anyone else from getting their choice. In this case, i's payoff is π. In contrast, if $M_i > N - 1$, by choosing marmite, with probability 1 player i deprives *one* other player from getting their preferred choice. Hence i's payoff is $\pi - D$. Similarly, if $M_i < N$, that is, if most other players do not like marmite, by choosing marmite i makes *one* other player happy with probability 1. Hence i's payoff is $\pi + D$.

Now let p be the probability that other players like marmite. We have:

$$\Pr(M_i = N-1) = \binom{2N-1}{N-1} p^{N-1}(1-p)^N = \frac{(2N-1)!}{N!(N-1)!}(1-p)[p(1-p)]^{N-1}$$

since there are $2N - 1$ players other than i. If $N = 1$ (i.e., there are two players) the above boils down to:

$$\Pr(M_i = N - 1) = \Pr(M_i = 0) = 1 - p$$

With two players only, there is a non-negligible probability that i's choice is accommodated by the other player. In contrast, if N is large, then $\frac{M_i}{2N-1}$ tends to p. In this case, if $p > 0.5$ player i knows that, with a very high probability, he is taking marmite away from people who like it while if $p < 0.5$ he knows he is leaving chocolate for people who like it. In other words, if i has reasons to believe that $p \neq 0.5$ then the *larger* the group is, the *more* likely i is to affect someone else's choice. It follows that if i believes that everybody likes marmite, or that everyone has the same taste as he does, then i's expected utility is $\pi - D$ while if he believes others dislike marmite, his expected utility is $\pi + D$.

If the game's payoff is not a good but money, then player i would normally expect other players to prefer the choice that yields the highest monetary payoff for them. In our example, if marmite is a higher monetary

payoff, $p > 0.5$ and player i expects to take away from others' payoff. It follows from the above reasoning that, in a large group where one choice is better for most other players, i expects to affect other players' payoff such that:

$$W_3(h|N = \infty) \equiv \pi_i^h - \pi_i^l + \beta \sum_{j \neq i, j=1}^{N} E[\pi_j^{h_i} - \pi_j^{l_i}]$$

$$\approx \quad \pi_i^h - \pi_i^l + \beta \sum_{j \neq i, j=1}^{N} \frac{4}{N}(\pi_j^{h_i} - \pi_j^{l_i}) = W_2(h)$$

In contrast, in a small group, say a group of 2, i could imagine that player k will accommodate his choice. This occurs with probability $1 - p$. Hence i's utility then is:

$$W_3(h|N = 2) \equiv \pi_i^h - \pi_i^l + \beta(1 - p)0 + \beta p \sum_{j \neq i, j=1}^{N} E[\pi_j^{h_i} - \pi_j^{l_i}]$$

which is a *less* other-regarding preference, i.e., it is equivalent to a smaller $\beta' = \beta p$. This means that the accommodation effect works in the opposite direction from the dilution effect.

It is possible to show that *for any p*, the probability that other players accommodate i's selfish choice *falls* with N. Run the stata do file below for an illustration:

```
clear all
set obs 101
gen p=(_n-1)/101
forvalues i=1(1)6 {
    gen b'i'=binomialp(2*'i'-1,'i'-1,p)
}
sum b*
scatter b1 b2 b3 b4 b5 b6 p
```

This shows that, if anything, accommodation is *more* likely in T2 than in T3, so accommodation *cannot* be an explanation for dilution.

Conclusion

1. If others' payoff are monetary, then the utility of helping or hindering others' choices can be approximated by the effect on their payoff times a welfare weight β. This simplifies the notation to $W_3(h|N)$.

2. If there is a large group, then the choice of i is nearly never indifferent (i.e., it is nearly never accommodated) *unless* $p = 0.5$ (or more precisely some unique value $\frac{N-1}{2N-1}$ close to 0.5).

3. For groups of intermediate size (e.g., $N = 6$ as in T3) the probability of accommodation is highest for $p = \frac{N-1}{2N-1}$.

4. For groups of smallest size ($N = 1$ as in T2) the probability of accommodation is $1 - p$ and thus is 1 for $p = 0$ and 0 for $p = 1$.

5. The probability of accommodation falls with group size: the larger the group, the lower the probability of accommodation is for any p.

6. If players form rational expectations about others' play, they will expect them to seek to improve their own payoff. Hence $p >> 0.5$ if marmite/high partner is a better choice for the majority of players in i's category and type (who by experimental design share i's payoff structure). In this case, the probability of accommodation is low in all games.

The bottom line is that accommodation *cannot* explain dilution since it operates in the opposite direction.

Appendix 3: Estimation of mixture model

The starting point of the estimation is the observation that:

$$\Pr(y_i = 1 | \pi_i, \pi_{j \neq i}, T_i) = \sum_{k=1}^{K} \Pr(y_i = 1 | \pi_i, \pi_{j \neq i}, T_i, k) \Pr(u = u_k | T_i) \quad (4)$$

where π_i and $\pi_{j \neq i}$ denote the three payoffs potentially entering the preference utility u_k of archetype $k = \{1, \ldots K\}$. Since π_i and $\pi_{j \neq i}$ are randomly assigned in the experiment, we can ignore correlations between payoffs and preferences. But we allow preferences to differ across treatments, hence the conditioning on T_i. A similar probability can be derived for $y_i = 0$. Since, for a given treatment T_i, $\Pr(u = u_k | T_i)$ is a constant, we denote it as γ_{kT}.

Next we assume that the probability of choosing action $y_i = 1$ increases in Δu_i^k, the utility gain from choosing $y_i = 1$ that is associated with payoffs π_i and $\pi_{j \neq i}$ when preferences are given by $u_k(.)$. To formalize this idea, we borrow from Luce (1959) and write:

$$\Pr(y_i = 1 | \pi_i, \pi_{j \neq i}, T_i, k) = \frac{e^{\sigma_T \Delta u_i^k}}{1 + e^{\sigma_T \Delta u_i^k}} \quad (5)$$

Parameter σ_T captures how sensitive decisions are to Δu_i^k in treatment T. If $\sigma_T = 0$, the choice between $y_i = 0$ and $y_i = 1$ is random and does not depend on payoffs. If σ_T is arbitrarily large, expression (??) tends to 1 if $\Delta u_i^k > 0$ and to 0 if $\Delta u_i^k < 0$ – which corresponds to the case where choices are perfectly predictable. Intermediate values of σ_T capture situations in which participants systematically diverge from random play in the direction predicted by archetype k. The model assumes that participants are more likely to follow the decision predicted by their archetype when the utility gain Δu_i^k between the two choices is large. Mixed models have successfully been fitted to experimental data (e.g., Andersen et al. 2008, Null 2012).

The likelihood function has the form:

$$L(\gamma_k, \sigma | y_i, \{\Delta u_i^k\}, T_i) = \sum_{k=1}^{K} \gamma_{kT} \left(\frac{e^{\sigma_T \Delta u_i^k}}{1 + e^{\sigma_T \Delta u_i^k}} y_i + \frac{1}{1 + e^{\sigma_T \Delta u_i^k}} (1 - y_i) \right)$$
$$(6)$$

and is estimated separately for each treatment, ensuring that $0 < \gamma_{kT} < 1$ and imposing that $\sum_{k=1}^{K} \gamma_{kT} = 1$.[17] Once σ_T and $\{\gamma_{1T}, \ldots \gamma_{KT}\}$ have been

---

[17] Estimation is achieved by numerical optimization in Stata. To ensure that all Δu_i^k

estimated, we compute, for each subject, the posterior probability that their choices follows a particular archetype.[18] Accumulating across rounds for each individual i, the posterior probability that i follows archetype k is:

$$\Pr(k|\{y_i\}) = \frac{\Pr(k)\Pr(\{y_i\}|k)}{\Pr(\{y_i\})} \tag{7}$$

where $\{y_i\} = \{y_i^1, y_i^2, \ldots y_i^6\}$ is the set of decisions made by i over the six rounds. Since $\widehat{\gamma}_{kT}$ and $\widehat{\sigma}_T$ vary across treatment, $\Pr(k|\{y\})$ also varies across treatment for the same set of choices made. Once we have $\Pr(k|\{y\})$ for each subject, we look at how accurate the predictions are for different individuals, i.e., how accurately they are estimated to follow a given archetype.

The first panel of Table 7 reports the average of estimated posterior probabilities (??) calculated using parameters $\widehat{\gamma}_{kT}$ and $\widehat{\sigma}_T$ estimated using (??). The second panel of the Table assigns each individual to an archetype if their $\Pr(k|\{y_i\})$ exceeds 0.5 for one k – which can happen at most for one k, but could happen for none.

---

have the same weight in the estimation, we normalize them to all have a unit standard deviation. Convergence difficulties can arise when $\gamma_{kT} \approx 0$ for some k, or when the Δu_i^k are too correlated across archetypes (akin to multicollinearity).

[18]Let $\Pr(k|y_i = 1)$ denote the probability that individual i is of archetype k if he/she sets $y = 1$. The starting point of our calculation is the following relationship that holds for each choice i makes:

$$
\begin{aligned}
\Pr(k|y = 1) &= \frac{\Pr(k)\Pr(y = 1|k)}{\Pr(y = 1)} \\
&= \frac{\Pr(k)\Pr(y = 1|k)}{\sum_k \Pr(k)\Pr(y = 1|k)}
\end{aligned}
$$

For simplicity of exposition, we have omitted the dependence on π and T. Unconditional probability $\Pr(k)$ is estimated by $\widehat{\gamma}_{kT}$ while $\Pr(y = 1|k)$ is obtained from expression (??) using estimated $\widehat{\sigma}_T$.

Table 2 – Scenarios, parameters and choice distribution

Scenario	Positive sorting				Negative sorting				Positive sorting efficient	Sharing rule	Share choosing Positive sorting		
	Bottom 50%		Top 50%		Bottom 50%		Top 50%		Yes / No	Share Low in NS[1]	Bottom 50%	Bottom 50%	Top 50%
	A	M	A	M	A	M	A	M			A	M	A/M
1	10	2	18	18	3	3	15	15	Yes	1/6	0.90	0.09	0.87
2	12	4	16	16	3	3	15	15	Yes	1/6	0.92	0.73	0.80
3	8	8	16	16	9	9	9	9	Yes	1/2	0.00	0.18	0.90
4	10	2	12	12	2.5	2.5	12.5	12.5	Yes	1/6	0.82	0.00	0.41
5	2	2	16	16	5	5	10	10	Yes	1/3	0.00	0.00	0.92
6	6	6	12	12	7.5	7.5	7.5	7.5	Yes	1/2	0.17	0.18	0.96
7	2	2	16	16	9	9	9	9	Equal	1/2	0.09	0.00	0.95
8	7.5	7.5	7.5	7.5	3	3	15	15	No	1/6	0.89	0.91	0.05
9	4	4	11	11	6	6	12	12	No	**1/3**	0.09	0.05	0.17
10	8	2	10	10	9	9	9	9	No	1/2	0.30	0.00	0.73
11	2.5	2.5	12.5	12.5	9	9	9	9	No	1/2	0.03	0.06	0.86
12	8	2	10	10	3	3	15	15	No	1/6	0.90	0.04	0.13
13	2.5	2.5	12.5	12.5	3	3	15	15	No	**1/6**	0.27	0.27	0.09
14	3	3	15	15	8	8	16	16	No	**1/3**	0.09	0.09	0.28
15	8	2	13	13	12	12	12	12	No	1/2	0.92	0.00	0.67
16	9	9	9	9	4	4	20	20	No	1/6	0.08	0.09	0.24
17	3	3	15	15	12	12	12	12	No	1/2	0.00	0.00	0.82

[1] The share is underlined if it is a sharing rule that would guarantee an efficient allocation with selfish preferences.

Table 3. Proportion of choices that maximize the payoff of self or others

	% of choices that maximize own payoff				% of choices that maximize others' payoff			
	T3	T2	T1	χ^2 test	T3	T2	T1	χ^2 test
High payoff subjects	80.8%	84.8%	80.8%	1.93	52.7%	60.0%	68.9%	**13.3**
Nber of observations	276	282	234	*0.377*	264	270	222	*0.001*
Low payoff A subjects	88.7%	90.9%	85.0%	2.16	43.6%	50.0%	57.5%	**4.87**
Nber of observations	133	132	120	*0.340*	133	132	120	*0.088*
Low payoff M subjects	90.2%	94.9%	88.9%	3.46	40.2%	42.8%	46.0%	0.91
Nber of observations	132	138	126	*0.177*	132	138	126	*0.634*
	% of choices that maximize others' payoff: A. when this does not reduce own payoff				% of choices that maximize others' payoff: B. when this reduces own payoff			
	T3	T2	T1	χ^2 test	T3	T2	T1	χ^2 test
High types	78.3%	89.5%	94.4%	**17.09**	17.9%	21.4%	36.1%	**10.31**
Nber of observations	152	153	125	*0.000*	112	117	97	*0.006*
Low A types	83.1%	90.9%	93.2%	3.63	5.9%	9.1%	23.0%	**9.63**
Nber of observations	65	66	59	*0.163*	68	66	61	*0.008*
Low M types	87.0%	94.8%	92.3%	2.24	7.7%	5.0%	13.5%	3.66
Nber of observations	54	58	52	*0.326*	78	80	74	*0.160*

T3, T2 and T1 stand for treatment 3, treatment 2, and treatment 1, respectively. The two lower panels break down the upper right panel into cases where the altruistic choice does or does not reduce the subject's own material payoff. The χ2 test columns report the test statistic for equality of means across treatments and, below it in italics, the corresponding p-value of the test. Test statistics significant at the 10% or better appear in bold.

Table 4. Marginal effect of a payoff difference on the probability of choosing high

	T3		T2		T1	
	dy/dx	t-stat.	dy/dx	t-stat.	dy/dx	t-stat.
A. Marginal effect of own payoff difference:						
High type	6.4%	**5.97**	6.6%	**8.41**	6.1%	**7.90**
Low A type	6.6%	**16.64**	6.6%	**11.23**	5.1%	**8.81**
Low M type	5.1%	**4.76**	4.8%	**3.30**	5.4%	**3.26**
B. Marginal effect of others' combined payoff difference:						
High type	0.0%	0.01	1.0%	**2.35**	1.5%	**3.31**
Low A type	-1.5%	**-3.76**	-0.4%	-0.91	0.4%	0.37
Low M type	-0.9%	**-4.84**	-0.7%	-1.36	-0.1%	-0.14

Marginal effects from regression (1).

Table 5. Regression of efficiency on treatment dummies

	All subjects		High payoff		Middle payoff		Low payoff	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
Treatment T1	9.0%	3.64	10.9%	3.22	10.2%	1.84	4.2%	0.93
Treatment T2	6.2%	2.79	5.8%	1.71	10.5%	2.24	2.9%	0.84
Intercept = T3	60.8%	40.03	60.9%	25.99	59.4%	20.97	62.2%	23.72
Nber. Observations	1573		792		385		396	

Dependent variable = percentage of maximum achievable aggregate payoff.

Estimator is a LPM. Standard errors clustered at the participant level.

Table 6. Assignment to archetypes, assuming no mistakes

Archetype:	All subjects			High types			Low A type			Low M type		
	T3	T2	T1	T3	T2	T1	T3	T2	T1	T3	T2	T1
Selfish	57%	64%	45%	54%	53%	38%	50%	73%	55%	68%	78%	48%
Efficiency only	0%	2%	4%	0%	4%	5%	0%	0%	5%	0%	0%	0%
Equity only	10%	7%	9%	2%	0%	0%	23%	14%	15%	14%	13%	19%
Invidious	17%	12%	11%	4%	0%	0%	50%	41%	35%	9%	9%	10%
Maximin	22%	23%	21%	2%	0%	8%	18%	14%	20%	68%	78%	48%
Homophily	2%	1%	1%	2%	2%	3%	0%	0%	0%	5%	0%	0%
Multiple archetypes:												
Fits more than one	27%	29%	21%	2%	0%	0%	36%	41%	35%	68%	78%	48%
Fits none	31%	30%	43%	37%	40%	46%	32%	27%	35%	18%	13%	43%
Number of observations	540	552	480	276	282	234	132	132	120	132	138	126

Table 7. MLE Assignment of subjects to archetypes

Pr(Archetype)	Treatment T3				Treatment T2				Treatment T1			
	All	**High**	**Low A**	**Low M**	**All**	**High**	**Low A**	**Low M**	**All**	**High**	**Low A**	**Low M**
Selfish	73.9%	80.9%	57.2%	76.0%	79.6%	82.4%	78.6%	74.7%	59.3%	55.8%	51.5%	73.4%
Efficiency only	1.1%	0.0%	4.6%	0.0%	4.3%	6.2%	0.8%	3.8%	5.9%	6.7%	7.8%	2.4%
Equity only	1.3%	0.4%	4.3%	0.3%	1.8%	0.0%	6.8%	0.7%	0.0%	0.0%	0.0%	0.0%
Invidious	15.8%	11.8%	27.1%	12.9%	2.3%	0.3%	1.0%	7.6%	9.1%	12.5%	3.6%	8.1%
Maximin	5.0%	3.5%	6.8%	6.3%	10.9%	8.9%	12.8%	13.1%	14.1%	1.2%	37.1%	16.1%
Homophily	2.9%	3.4%	0.0%	4.5%	1.1%	2.2%	0.0%	0.0%	11.6%	23.8%	0.0%	0.0%
Predicted archetype												
Selfish	81.1%	89.1%	63.6%	81.8%	83.7%	83.0%	86.4%	82.6%	73.8%	64.1%	70.0%	95.2%
Efficiency only	1.1%	0.0%	4.5%	0.0%	4.3%	6.4%	0.0%	4.3%	5.0%	7.7%	5.0%	0.0%
Equity only	1.1%	0.0%	4.5%	0.0%	1.1%	0.0%	4.5%	0.0%	0.0%	0.0%	0.0%	0.0%
Invidious	11.1%	4.3%	22.7%	13.6%	2.2%	0.0%	0.0%	8.7%	3.8%	5.1%	0.0%	4.8%
Maximin	3.3%	4.3%	4.5%	0.0%	7.6%	8.5%	9.1%	4.3%	6.3%	0.0%	25.0%	0.0%
Homophily	2.2%	2.2%	0.0%	4.5%	1.1%	2.1%	0.0%	0.0%	11.3%	23.1%	0.0%	0.0%
Nber observations	540	276	132	132	552	282	132	138	480	234	120	126

Posterior probability of archetype based on mixed MLE model (see text for details). Parameters are estimated separately for each treatment.

Predicted archetype =1 if Pr(archetype)>0.5 and 0 otherwise.

[TREATMENT 1]

Centre for Experimental Social Sciences

EXPERIMENT
November2011

INSTRUCTIONS

Please wait for the experimenter to indicate the start of the experiment. These instructions will be read aloud by the experimenter shortly.

Dear participants,

Welcome and thank you for participating to this experiment. Before we describe the experiment, we wish to inform you of a number of rules and practical details.

Important rules

- Your participation is considered voluntary and you are free to leave the room at any point if you wish to do so. In that case, we will only pay you the show-up fee of £4.
- **Silence:** Please do remain quiet from now on until the end of the experiment. You will have the opportunity to ask questions in a few minutes.

What will happen at the end of the experiment

Once the experiment is finished, please remain seated. We will need around 10 minutes to prepare your payment. We will move to another room and you will be called up successively by the number on your table (please take it with you as you leave the lab); you will then receive an envelope with your earnings and you will be asked to sign a receipt.

Description of the experiment

The experiment is structured in two stages. The first stage is called the production stage, you will be asked to do a calculation task for 3 minutes. The second stage is called the earnings division stage, where you will be asked how to divide earnings between participants. Both stages are important in determining your final earnings.

STAGE 1: PRODUCTION STAGE
Production here is achieved through two tasks: Adding and Multiplying numbers. Half of the participants will be asked to add numbers ("addition" task) and the other half will be asked to multiply numbers ("multiplication" task). The goal is to do as many calculations correctly as possible in 3 minutes. At the end of the 3 minutes, we will identify who are the TOP 50% best performers in each task and who are in the BOTTOM 50% performers in each task (that is, there will be ¼ of participants in the "TOP 50% multiplication", a ¼ in the "TOP 50% addition", a ¼ in the "BOTTOM 50% multiplication" and a ¼ in the "BOTTOM 50% addition").

STAGE 2: EARNINGS DIVISION STAGE
In a second stage, you will be placed in groups of 4, each composed of
- A TOP 50% "multiplication" participant
- A BOTTOM 50% "multiplication" participant
- A TOP 50% "addition" participant
- A BOTTOM 50% "addition" participant

You will be asked to choose between two allocations of earnings that differ by
(1) the total amount of earnings to be divided
(2) how it is allocated between the four people in the group

We will present you with 6 scenarios. Each scenario corresponds to different joint earnings and different ways of dividing these earnings. All the participants in the session will receive the same scenarios (and these will be numbered 1, 2, 3, 4, 5 and 6). You will have more than enough time (10 minutes in total) to study each of these scenarios carefully.

Please report your preferred choice in each scenario. You will simply need to tick one of the boxes at the bottom of the answer sheet.

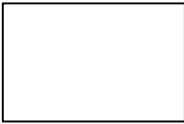After we have collected your answers for all 6 scenarios,
- One of these 6 scenarios will be selected at random
- For each group of four people, we will choose one person at random and implement her choice of allocation for the four people in her group.

Note that the first line of the answer sheet will inform you privately about whether your performance was in the TOP 50% or BOTTOM 50% of your own group.
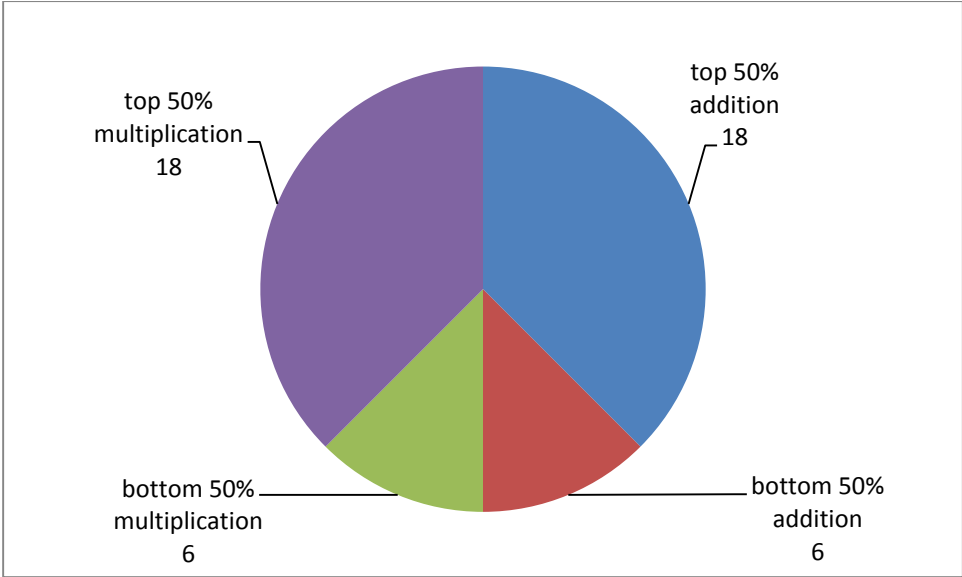We show you on the next page an example of answer sheet.

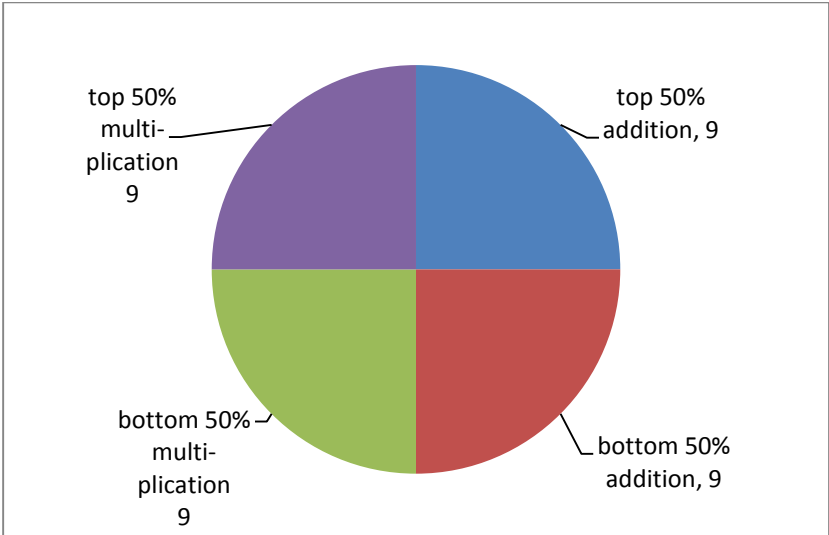The earnings are indicated in Experimental Currency Units. The exchange rate is £1=1.50 ECU.

You did the MULTIPLICATION task.
Your performance in the first round was in the TOP 50%

POSSIBLE EARNINGS DIVISION 1: Total earnings 48

top 50%
multiplication
18

top 50%
addition
18

bottom 50%
multiplication
6

bottom 50%
addition
6

POSSIBLE EARNINGS DIVISION 2: Total earnings: 36

top 50%
multi-
plication
9

top 50%
addition, 9

bottom 50%
multi-
plication
9

bottom 50%
addition, 9

INDICATE (WITH A TICK) WHICH EARNINGS DIVISION YOU PREFER:

☐ Earnings division 1
☐ Earnings division 2

At the very end, we will circulate a questionnaire that has no implications for your earnings. We would be grateful if you can fill it in carefully.

SUMMARY

STAGE 1: Production Stage (3 minutes)

5 minutes break

STAGE 2: Earnings Division stage (10 minutes)
Scenario 1
Scenario 2
Scenario 3
Scenario 4
Scenario 5
Scenario 6

STAGE 3: Implementation
In each group one person is selected to implement the earnings division
One scenario is randomly selected to be implemented
Choices are implemented

QUESTIONNAIRE

Preparation of payment

Payment

If you have any questions, please raise your hand now and wait for the experimenter to come to you.
Please leave these instructions on your table when you leave the room.

Centre for Experimental Social Sciences

EXPERIMENT
November2011

INSTRUCTIONS

Please wait for the experimenter to indicate the start of the experiment. These instructions will be read aloud by the experimenter shortly.

Dear participants,

Welcome and thank you for participating to this experiment. Before we describe the experiment, we wish to inform you of a number of rules and practical details.

Important rules

- Your participation is considered voluntary and you are free to leave the room at any point if you wish to do so. In that case, we will only pay you the show-up fee of £4.
- **Silence:** Please do remain quiet from now on until the end of the experiment. You will have the opportunity to ask questions in a few minutes.

What will happen at the end of the experiment

Once the experiment is finished, please remain seated. We will need around 10 minutes to prepare your payment. We will move to another room and you will be called up successively by the number on your table (please take it with you as you leave the lab); you will then receive an envelope with your earnings and you will be asked to sign a receipt.

Description of the experiment

The experiment is structured in two stages. The first stage is called the production stage, you will be asked to do a calculation task for 3 minutes. The second stage is called the partnership formation stage, where you will be asked with whom you would like to form a partnership. Both stages are important in determining your final earnings.

STAGE 1: PRODUCTION STAGE
Production here is achieved through two tasks: Adding and Multiplying numbers. Half of the participants will be asked to add numbers ("addition" task) and the other half will be asked to multiply numbers ("multiplication" task). The goal is to do as many calculations correctly as possible in 3 minutes. At the end of the 3 minutes, we will identify who are the TOP 50% best performers in each task and who are in the BOTTOM 50% performers in each task (that is, there will be ¼ of participants in the "TOP 50% multiplication", a ¼ in the "TOP 50% addition", a ¼ in the "BOTTOM 50% multiplication" and a ¼ in the "BOTTOM 50% addition").

STAGE 2: PARTNERSHIP FORMATION STAGE
In a second stage, you will be placed in groups of 4, each composed of
- A TOP 50% "multiplication" participant
- A BOTTOM 50% "multiplication" participant
- A TOP 50% "addition" participant
- A BOTTOM 50% "addition" participant

You will be asked to form a partnership with a person who performed a different task than yours (addition or multiplication). The joint earnings will depend on the performance of both partners in the first stage. More precisely, the joint earnings will depend on whether:
• Both are in the TOP 50%,
• One is in the TOP 50% and the other is in the BOTTOM 50%,
• Both are in the BOTTOM 50%.

We will give you a choice between forming a partnership with a person from the TOP 50% or with a person from the BOTTOM 50% in the other task. Each partnership will be associated with a given division of earnings in the group, which will be clearly indicated.

We will present you with 6 scenarios. Each scenario corresponds to different joint earnings and different ways of dividing these earnings in the group. All the participants in the session will receive the same scenarios (and these will be numbered 1, 2, 3, 4, 5 and 6). You will have more than enough time (10 minutes in total) to study each of these scenarios carefully.

Please report your preferred choice in each scenario. You will simply need to tick one of the boxes on the answer sheet.

After we have collected your answer sheet for all 6 scenarios,
• One of these 6 scenarios will be selected at random
• For each group of four people, we will choose one person at random and implement her choice. For example, if the person selected at random prefers to form a partnership with the person from the TOP 50% in the other task, then automatically the two people from the BOTTOM 50% will be matched to each other.

Note that the first line of the answer sheet will inform you *privately* about whether your performance was in the TOP 50% of BOTTOM 50% of your own group.
We show you on the next page an example of an answer sheet. You will have two answer sheets per page corresponding to 2 different scenarios.

You did the MULTIPLICATION task.
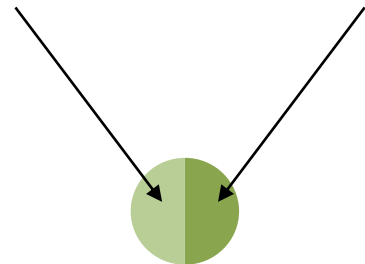Your performance in the first round was in the TOP 50%

DIVISION OF EARNINGS IF YOU FORM A PARTNERSHIP WITH TOP 50% ADDITION

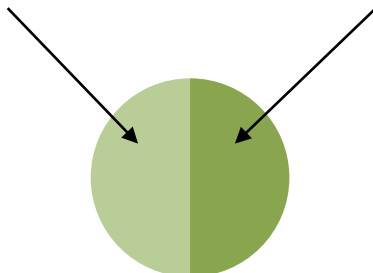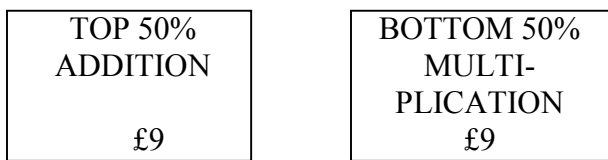| TOP 50% ADDITION & TOP 50% MULTIPLICATION (YOU) JOINT INCOME: £32	BOTTOM 50% ADDITION & BOTTOM 50% MULTIPLICATION JOINT INCOME: £4
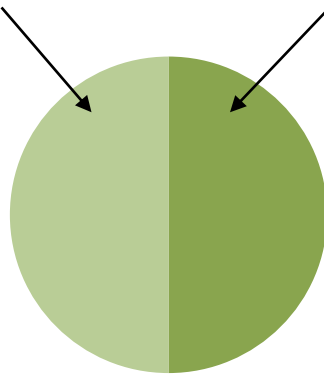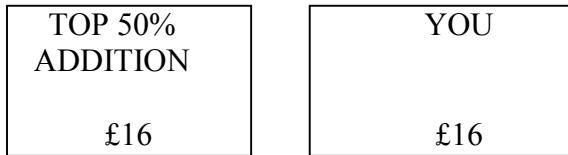
TOP 50% ADDITION £16	YOU £16	BOTTOM 50% ADDITION £2	BOTTOM 50% MULTI-PLICATION £2

DIVISION OF EARNINGS IF YOU FORM A PARTNERSHIP WITH BOTTOM 50% ADDITION

TOP 50% ADDITION & BOTTOM 50% MULTIPLICATION JOINT INCOME: £18	BOTTOM 50% ADDITION & TOP 50% MULTIPLICATION JOINT INCOME: £18

TOP 50% ADDITION £9	BOTTOM 50% MULTI-PLICATION £9	BOTTOM 50% ADDITION £9	YOU £9
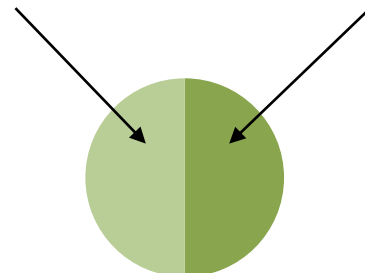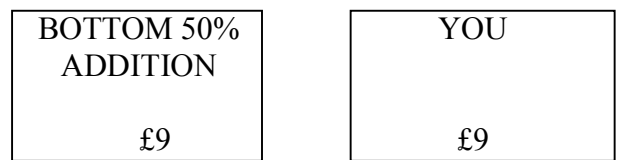
Indicate (with a tick) with whom you would prefer to form a partnership:
☐ TOP 50% "ADDITION"
☐ BOTTOM 50% "ADDITION"

At the very end, we will circulate a questionnaire that has no implications for your earnings. We would be grateful if you can fill it in carefully.

SUMMARY

STAGE 1: Production Stage (3 minutes)

5 minutes break

STAGE 2: Partnership Selection stage (10 minutes)
Scenario 1
Scenario 2
Scenario 3
Scenario 4
Scenario 5
Scenario 6

STAGE 3: Implementation
In each group one person is randomly selected to be the leader in partnership selection
One scenario is randomly selected to be implemented
Choices are implemented

QUESTIONNAIRE

Preparation of payment

Payment

If you have any questions, please raise your hand now and wait for the experimenter to come to you.
Please leave these instructions on your table when you leave the room.

[Treatment 3]

Centre for Experimental Social Sciences

EXPERIMENT
May 2011

INSTRUCTIONS

Please wait for the experimenter to indicate the start of the experiment. These instructions will be read aloud by the experimenter shortly.

Dear participants,

Welcome and thank you for participating to this experiment. Before we describe the experiment, we wish to inform you of a number of rules and practical details.

Important rules

- Your participation is considered voluntary and you are free to leave the room at any point if you wish to do so. In that case, we will only pay you the show-up fee of £4.
- **Silence:** Please do remain quiet from now on until the end of the experiment. You will have the opportunity to ask questions in a few minutes.

What will happen at the end of the experiment

Once the experiment is finished, please remain seated. We will need around 10 minutes to prepare your payment. We will move to another room and you will be called up successively by the number on your table (please take it with you as you leave the lab); you will then receive an envelope with your earnings and you will be asked to sign a receipt.

Description of the experiment

The experiment is structured in two stages. The first stage is called the production stage, you will be asked to do a calculation task for 3 minutes. The second stage is called the partnership formation stage, where you will be asked with whom you would like to form a partnership. Both stages are important in determining your final earnings.

STAGE 1: PRODUCTION STAGE
Production here is achieved through two tasks: Adding and Multiplying numbers. Half of the participants will be asked to add numbers ("addition" group) and the other half will be asked to multiply numbers ("multiplication" group). The goal is to do as many calculations correctly as possible in 3 minutes.

At the end of the 3 minutes, we will identify in each group who are in the TOP 50% best performers and who are in the BOTTOM 50%.

STAGE 2: PARTNERSHIP FORMATION STAGE
In a second stage, you will be asked to form a partnership with a person from the other group than your own (addition or multiplication). The joint earnings will depend on the performance of both partners in the first stage.

More precisely, the joint earnings will depend on whether:

- Both are in the TOP 50%
- One is in the TOP 50% and the other is in the BOTTOM 50%
- Both are in the BOTTOM 50%

We will give you a choice between forming a partnership with a person from the TOP 50% or with a person from the BOTTOM 50% from the other group. Each partnership will be associated with a given division of earnings, which will be clearly indicated.

We will present you with 6 scenarios. Each scenario corresponds to different joint earnings and different ways of dividing these earnings between you are your partner. All the participants in the session will receive the same scenarios (and these will be numbered 1, 2, 3, 4, 5 and 6). You will have more than enough time (10 minutes in total) to study each of these scenarios carefully.

Please report your preferred choice in each scenario. You will simply need to tick one of the boxes at the bottom of the answer sheet.

After we have collected your answers for all 6 scenarios,
- One of these 6 scenarios will be selected at random
- One of the groups (Addition / Multiplication) will be selected at random and the partnerships and earnings divisions will be implemented according to the preferred choices of the selected group.

Note that the first line of the answer sheet will inform you privately about whether your performance was in the TOP 50% of BOTTOM 50% of your own group.
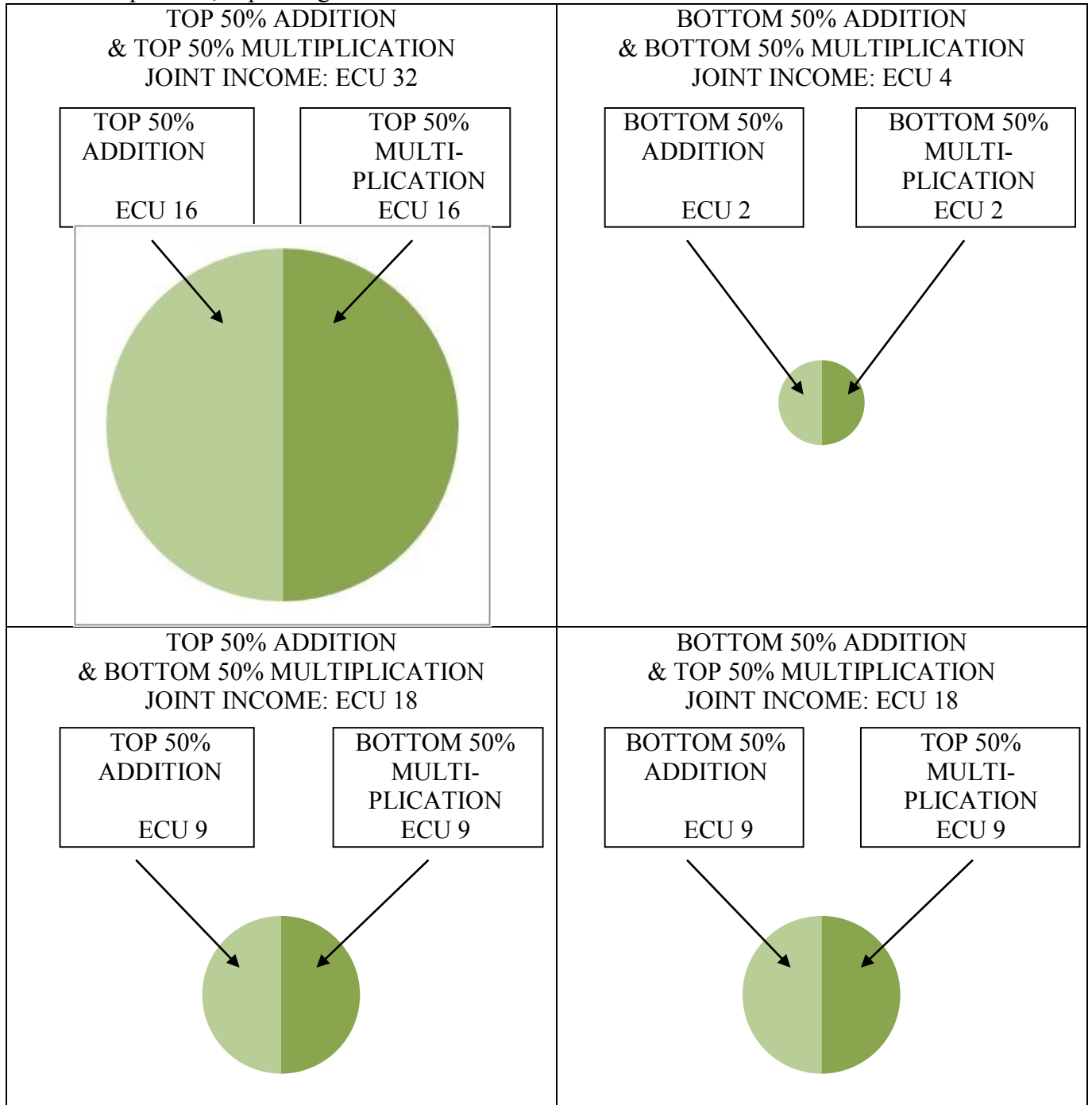
Finally, note that earnings will be indicated in experimental currency units (ECU). The exchange rate is £1 = ECU 1.5

We show you on the next page an example of answer sheet.

If your group is chosen to lead the partnership selection, your income will depend on whom you form a partnership with in the Addition group. We indicate below how the earnings will be distributed between the partners, depending on who is matched with whom.

TOP 50% ADDITION & TOP 50% MULTIPLICATION JOINT INCOME: ECU 32	BOTTOM 50% ADDITION & BOTTOM 50% MULTIPLICATION JOINT INCOME: ECU 4
TOP 50% ADDITION ECU 16 TOP 50% MULTI-PLICATION ECU 16	BOTTOM 50% ADDITION ECU 2 BOTTOM 50% MULTI-PLICATION ECU 2
TOP 50% ADDITION & BOTTOM 50% MULTIPLICATION JOINT INCOME: ECU 18	BOTTOM 50% ADDITION & TOP 50% MULTIPLICATION JOINT INCOME: ECU 18
TOP 50% ADDITION ECU 9 BOTTOM 50% MULTI-PLICATION ECU 9	BOTTOM 50% ADDITION ECU 9 TOP 50% MULTI-PLICATION ECU 9

INDICATE (WITH A TICK) WITH WHOM YOU WOULD PREFER TO FORM A PARTNERSHIP:

☐ A person from the TOP 50% of the "Multiplication Group"
☐ A person from the BOTTOM 50% of the "Multiplication Group"

We will implement these preferred choices insofar as possible. If there is a shortage of partners of a given type (either "Top 50%" or "Bottom 50%"), the most demanded partners will be allocated between those who demand them in a random manner. For example, suppose "Addition" is randomly selected for the partner selection. We will randomly choose a first person in the Addition group and implement her/his choice and match him/her to a random person from her preferred type (top 50%/bottom 50%). We repeat this for each person in the Addition group one by one. If we cannot accommodate the choice because the demanded type is not available anymore, we will match the person with the other type.

At the very end, we will circulate a questionnaire that has no implications for your earnings. We would be grateful if you can fill it in carefully.

SUMMARY

STAGE 1: Production Stage (3 minutes)

5 minutes break

STAGE 2: Partnership Selection stage (10 minutes)
Scenario 1
Scenario 2
Scenario 3
Scenario 4
Scenario 5
Scenario 6

STAGE 3: Implementation
One group is randomly selected to be the leader in partnership selection
One scenario is randomly selected to be implemented
Choices are implemented

QUESTIONNAIRE (10 minutes)

Preparation of payment (10 minutes)

Payment (10 minutes)

If you have any questions, please raise your hand now and wait for the experimenter to come to you.
Please leave these instructions on your table when you leave the room.