Social preferences and coordination: An experiment

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Social Preferences and Coordination: An Experiment*

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Abstract

In this paper, we use a laboratory experiment to analyze the effect of social preferences in a coordination game with Pareto-ranked equilibria. Inequality is increased by increasing the coordination payoffs of some subjects while the coordination payoffs of others remain unchanged. Theoretically, in this setting, inequality aversion may lead to a negative relationship between inequality and coordination success, while total payoff motivations lead to a positive relationship. Using a within-subject experimental design, we find that more inequality unambiguously yields a higher level of coordination success. Furthermore, this result holds *even for subjects whose payoffs remain unchanged*. Our results suggest that total payoff motivations drive the positive relationship between inequality and coordination success found in this experiment. Moreover, our data highlight that the order of treatment matters. Groups facing over time a reduction in inequalities reach the efficient outcome more often, over the entire experiment, compared to groups facing over time an increase in inequalities. This study thus contributes to understanding whether social preferences and variations in inequality affect the outcome of coordination problems.

Keywords: Coordination game, inequality, inequality aversion, total payoff motivation.
JEL codes: C9, D6, D7.

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1 Introduction

Social preferences are now acknowledged as being prevalent in many economic decisions (see for instance Charness and Rabin, 2002, or Gangadharan et al. 2017). More specifically, different types of social preferences, such as social efficiency[1] motivation or aversion to inequality, may play an important role in the acceptability of economic decisions and thus have notable effects in settings such as the design of salary structures in organizations (Lazear, 1989), instruments and mechanisms designed to overcome social dilemmas (e.g. see Gangadharan et al., 2017) or development policy issues (Bardhan, 1996). At the same time, coordination between economic agents in markets, contracts, firms, governments and organizations is a necessary condition to reach efficiency in most economic activities.

Social preferences do matter in various contexts, as highlighted in the literature. For instance, total payoff motivation plays a major role in dictator games (Charness and Rabin 2002), gift exchange and ultimatum games (Charness and Haruvy 2002), or bargaining problems (Isoni et al. 2014, Galeotti et al. 2018). Another important example is related to the effect of payoff asymmetries. In an extensively studied class of games characterized by the existence of a coordination problem and a conflict of interest, such as battle of the sexes games, increasing inequality (payoff asymmetry) decreases coordination success (see Crawford et al. 2008). By contrast, the connection between social preferences and coordination success has received relatively limited attention in situations where there is no conflict of interest, such as games with Pareto ranked equilibria.

Yet, this connection raises several interesting questions. Do social preferences affect the emergence of (Pareto) efficient outcomes through their influence on the agents' ability to coordinate? Does the prospect of unevenly distributed larger coordination gains decrease or increase the rate at which agents coordinate efficiently? What kind of motivations drive the agents' behavior in such situation? In this paper, we present the results of a laboratory experiment to address these questions.

We study whether coordination success is related to different types of social preferences, namely total payoff motivation and inequality aversion, using a laboratory experimental coordination game with Pareto ranked equilibria. Inequality is increased by increasing the coordination payoffs of some subjects while the coordination payoffs of others remain unchanged. Theoretically, in this setting, inequality aversion (see Fehr and Schmidt, 1999) may lead to a negative relationship between inequality and coordination success, while total payoff motivations (see Charness and Rabin, 2002) lead to a positive relationship. Using a within-subject experimental design, we find that more inequality in coordination payoffs unambiguously increases coordination success. Moreover, this result holds even for subjects who were assigned the least favorable role and whose payoffs were not affected by the increase in inequality. These results suggest that total payoff motivations drive the positive relationship between inequality and coordination success. Moreover, our data suggest

[1] In the sense of Pareto efficiency.

that the order of treatment matters. Groups facing first the treatment with high inequality in coordination payoffs, then the treatment with low inequality in coordination payoffs, reach the Pareto dominant equilibrium more often in both treatments compared to groups playing first the treatment with low inequality in coordination payoffs, then the treatment with high inequality in coordination payoffs. We suggest that this order effect may be consistent with the assumption that subjects assess the level of strategic risk based on their past experience.

Our paper contributes to two strands of the literature. The first contribution is related to the experimental works analyzing the effects of social preferences on economic decisions. A notable part of this literature focuses on dictator games and similar settings (see Charness and Rabin, 2002).[2] Few other works consider more elaborate settings such as gift exchange or ultimatum games (Charness and Haruvy, 2002), bargaining problems (see Isoni et al. 2014 , or Galeotti et al. 2018), and public good contribution games (see Gangadharan et al. 2017 or Balafoutas et al. 2013). In the present paper, we provide experimental evidence on the effect of social preferences in coordination situations. Our results suggest that total payoff motivations drive the behavior of the subjects.[3] Moreover, the use of a three-player game setting allows us to avoid a limitation that is present when using two-player settings, where it would not be possible to distinguish whether a specific type of motivation (selfish preferences, aversion to either advantageous or disadvantageous inequality, and total payoff motivation) would be a driver in the subjects' decisions.[4]

The second contribution is related to the literature on coordination games with Pareto ranked equilibria, which has mostly focused on games with symmetric payoffs.[5] A strand of the literature focuses on the relationship between payoffs asymmetry and coordination in Battle of the sexes experimental games, that is, coordination games with no Pareto dominant equilibrium. Crawford et al. (2008) show that introducing a small degree of asymmetry in a Battle of the sexes game has a negative effect on coordination. However, the pattern of discoordination reverses when payoff asymmetry becomes sufficiently large.[6] Another strand of the literature analyzes coordination problems in games with Pareto ranked equilibria (e.g. see Brandts and Cooper, 2006 and Goeree and Holt, 2005), but related contributions focus on games with symmetric payoffs.[7] As in the present

[2]For other works related to this type of game see Engelmann and Strobel (2004), Bolton and Ockenfels (2006) or Fehr et al. (2006).

[3]Faillo et al. (2017) consider two-player games and provide evidence suggesting that collectively-optimal type of reasoning is facilitated when the collectively optimal equilibrium yields a more equal payoff distribution than the other equilibria.

[4]We will elaborate on this problem in Section 2.1.

[5]The research agenda dealing with the analysis of factors that may affect agents' abilities to coordinate is of course quite broad, as illustrated by the analysis of the effect of subjects' background provided in Jackson and Xing (2014).

[6]Parravano and Poulsen (2015) analyze the role of stake size on coordination frequency on the label salient choice (they hypothesize that label "A" is more salient than label "B") in symmetric and asymmetric coordination games with no Pareto dominant equilibrium.

[7]Devetag and Ortmann (2007) provide a survey of the literature on coordination failures in order-statistics and Stag Hunt games. Lòpez-pèrez et al. (2015) focus on the relative performance of a proposed equity-related selection criterion in several 2×2 coordination games. Finally, our game setting allows for the existence of one Pareto-dominant equilibrium, as in Hi-Lo games used in Bacharach (2006), Bardsley et al. (2010) and Isoni et al. (2019), while our game setting allows for asymmetric payoffs and Nash equilibria.

paper, Chmura et al. (2005) and Jacquemet and Zylbersztejn (2014) analyze coordination games with a Pareto dominant equilibrium and focus on variations in the subjects' payoffs (at this equilibrium).[8] Chmura et al. (2005) argue that the existence of beliefs about other subjects' inequality aversion is consistent with the observed subjects' behaviors. Their results are however difficult to interpret since they use a between-subject setting with a relatively small number of subjects per treatment[9] and then they cannot distinguish between the effect of subjects' heterogeneity (in terms of preferences, behavior, etc.) and the effect of the various treatments. By contrast, we use a within-subject setting in order to control for subjects and group characteristics that may influence subjects' play, and we show that subjects' behaviors are consistent with total payoff motivation and not with inequality aversion.[10] Jacquemet and Zylbersztejn (2014) use two-player games in which one subject is advantaged and faces strategic risk. Their results suggest that inequality aversion cannot overcome strategic risk.

As in the present paper, Goerg et al. (2010) provide experimental results based on a theoretical model in which differentiated incentives are optimal.[11] They show that the optimal differentiated incentives outperform non optimal equal incentives in terms of coordination success.[12] They find that the subjects' decisions are highly sensitive to their own payoff but largely insensitive to the payoffs of the other subjects. Their experiment is not designed to analyze the role of total payoff motivation, since they keep the sum of the individual rewards constant across the treatments. In the present paper, the sum of the payoffs changes from one treatment to the other while the payoff of one of the subjects is kept constant across the treatments. This design enables us to test the hypothesis that this subject gives weight to the payoffs of the other subjects in the same group, and thus that coordination may be facilitated when the payoffs of some subjects are larger, even if the payoffs are more unequal.

We present the results from an experiment where groups of three subjects play a coordination game based on the optimal solution to a club good production problem analyzed in Bernstein and Winter (2012).[13] The game admits multiple Nash equilibria (thus raising coordination issues) that are Pareto ranked, and the efficient outcome is unique and is always an equilibrium outcome. The game has another interesting property for our purpose: the efficient outcome is such that the players' payoffs are always asymmetric. We take advantage of this property and implement two treatments, one treatment in which the differences between the players' payoffs are almost equal at the efficient

[8]They focus on games with two players and two strategies.

[9]They implemented a quite large number of different treatments (seven) and the number of participant was almost identical as ours (280 and 270 respectively). Since their games involve two subjects each, they ended up with 20 subjects (i.e. observations) per treatment.

[10]This is not to say that subjects are not averse to inequality. However, our results suggest that the effect of inequality aversion is weaker than the effect of total payoff maximization.

[11]Their experiment is based on Winter (2004) while ours is based on Bernstein and Winter (2012).

[12]In the present paper, we compare the two optimal reward schemes of two different games.

[13]There are multiple examples of multilateral coordination problems in practice, which may induce additional issues compared to bilateral situations due to the increase in the complexity of the interactions. It is thus both interesting and important to provide experimental evidence about the likelihood of coordination success in multilateral situations.

outcome, and a second treatment in which one of the subjects' payoffs remain unchanged while the other two subjects in the group earn a substantively higher payoff at the efficient outcome. Each of the 90 groups of three subjects repeatedly plays the two treatments (in different orders).[14]

Our first main result is that groups reached the Pareto efficient outcome more often in the treatment with high inequality in coordination payoffs than in the treatment with low inequality in coordination payoffs. Our second main result is that, at the individual level, subjects choose to play the strategy that corresponds to the efficient outcome more often in the treatment with high inequality in coordination payoffs, *even if their situation remains unchanged between the two treatments* (while the two other subjects in their group get higher payoffs at the efficient outcome). To provide these results, we take advantage of the panel structure of our data that allows us to control for effects that are due to groups/individuals and time. These two results suggest that subjects have social preferences consistent with total payoff motivation rather than with inequality aversion. A third important result is that groups facing over time a reduction in inequalities reach the efficient outcome more often, over the entire experiment, compared to groups facing over time an increase in inequalities. Specifically, groups that first play the treatment with high inequality in coordination payoffs coordinate on the efficient outcome more frequently. This suggests that, in relative terms, situations where groups of agents face over time a reduction in inequalities facilitate coordination compared to situations where they face an increase in inequalities over time.

The rest of the paper is organized as follows. Section 2 introduces the games that are used in the experiment. In Section 3 we describe the experimental design and procedures. In Section 4 we present descriptive statistics and our main results, while Section 5 discusses potential alternative explanations to some of the conclusions. Section 6 concludes.

2 Theory and qualitative hypotheses

In this section, we describe the games used in our experiment and we provide various qualitative predictions.

2.1 The experimental games

We choose payoff structures that are consistent with a class of problems analyzed in Bernstein and Winter (2012), who study the decision of group members to participate in a collective activity generating positive externalities to participants. Indeed, this class of problems is prevalent in economics, as it relates to situations where a club good is provided, and the induced game structure is often characterized by coordination issues due to the existence of strategic complementarity between the group members' individual choice of actions. Indeed, the game admits multiple Nash

[14] We do not allow subjects to communicate since the effect of communication is not the focus of our analysis. Regarding this aspect, we refer to Charness (2000), Clark et al. (2001) and Manzini et al. (2009) for some related works.

equilibria (thus raising coordination issues) that are Pareto ranked, and the efficient outcome is always an equilibrium. Moreover, the setting of this analysis allows one to introduce heterogeneous benefits from coordination: these benefits may be member-specific. This is an important feature in order to consider issues raised by inequality in payoffs.

The game structure of the experiment is as follows. We consider a group of three agents where each agent is randomly assigned a role, namely A, B, or C. Each agent's decision is binary: choose 0 or choose 1. In the context of the analysis provided in Bernstein and Winter (2012) choosing 0 would mean that the agent does not participate to the joint project, while choosing 1 would mean that the agent participates. In the remainder of the paper, we will sometimes use the term "participate" instead of "choose 1".

All agents decide simultaneously. We consider two cases, one where there is a high degree of inequality in payoffs at the Pareto efficient equilibrium, which corresponds to Table 1, and one where there is a low degree of inequality in payoffs at the Pareto efficient equilibrium, which corresponds to Table 2.

Table 1: Payoff matrix (High inequality)

	Combinations	Payoffs		
	(A, B, C)	A	B	C
1	(0, 0, 0)	60	60	60
2	(1, 0, 0)	60	60	60
3	(0, 1, 0)	60	29	60
4	(0, 0, 1)	60	60	10
5	(1, 1, 0)	90	60	60
6	(1, 0, 1)	81	60	32
7	(0, 1, 1)	60	56	38
8	(1, 1, 1)	111	87	60

Notes: (A, B, C) means that the first index is for agent A, the second for agent B and the last one for agent C. Line 5, for instance, means that the (1, 1, 0) combination is reached, thus A gets 90, B gets 60 and C gets 60.

The two tables are obtained by relying on the setting introduced in Bernstein and Winter (2012), explanations are provided in Appendix A.

This game structure is such that (i) the efficient outcome is always part of the equilibrium set (ii) the benefits from coordination increase when moving from the low inequality case to the high inequality case (iii) the coordination payoffs of two agents increase (namely, the agents who are assigned roles A and B), while the coordination payoff of one agent is unaffected (namely, the agent who is assigned role C).

Before going further, let us make several remarks as regards the choice of the experimental games. Our choices are driven by the fact that we want to study how social preferences (inequality

Table 2: Payoff matrix (Low inequality)

	Combinations	Payoffs		
	(A, B, C)	A	B	C
1	(0, 0, 0)	60	60	60
2	(1, 0, 0)	60	60	60
3	(0, 1, 0)	60	29	60
4	(0, 0, 1)	60	60	10
5	(1, 1, 0)	61	60	60
6	(1, 0, 1)	61	60	32
7	(0, 1, 1)	60	30	38
8	(1, 1, 1)	62	61	60

Notes: (A, B, C) means that the first index is for agent A, the second for agent B and the last one for agent C. Line 5, for instance, means that the (1, 1, 0) combination is reached, thus A gets 61, B gets 60 and C gets 60.

aversion and total payoff motivation) affect coordination success.[15] Our main choices are as follows:

First, we choose to consider a three-player setting as in Engelmann and Strobel (2004) in order to avoid an issue related to two-player games. It would not be possible to distinguish whether a specific type of motivation (selfish preferences, aversion to either advantageous or disadvantageous inequality, and total payoff motivation) would be a driver of the subjects' decisions. This issue can be illustrated by relying on the game setting used in Engelmann and Strobel (2004), where players must choose between various payoff distributions. Assume that, instead of a three-player game, we choose to use a two-player setting, and that each agent i cares both about her own payoff x_i ($OWN = x_i$) and about total payoff $x_i + x_j$ ($EFF = x_i + x_j$). Moreover, assume that the agents are both averse to advantageous and disadvantageous inequalities, that is, each agent i takes two additional components into account, $FSD = -\max\{x_j - x_i, 0\}$ and $FSA = -\max\{x_i - x_j, 0\}$. An issue for the identification of the effect of these different components on the choices made by the players is that, in a two-player setting, we have $FSA - FSD = EFF - 2 * OWN$ for any payoff x_i and x_j. If the players' utility functions depend on a linear combination of the four components (an assumption made by Engelmann and Strobel, 2004 for instance), then the effects of all components will be confounded (because of collinearity). As such, we do not use a two-player setting in our design, because we are concerned that this issue can lead to inconclusive results in our experiment even if, in our three-player games, the subjects do not simply choose between payoff distributions. Notice that our three-player setting is such that, if all group subjects do coordinate on the efficient equilibrium, one subject faces advantageous inequality only, one subject faces disadvantageous inequality only, and one subject faces both types of inequality. Using a two-player setting would not allow to distinguish between these different cases.

[15] From now on, coordination success refers to the situation where players' choices correspond to the efficient outcome $(1, 1, 1)$.

Second, we choose to consider experimental games based on a theoretical model (Bernstein and Winter, 2012). An important advantage of doing so is that we quickly ensure that the set of Nash equilibria is identical in both cases. Specifically, decision vectors $(0, 0, 0)$, $(1, 0, 0)$, $(1, 1, 0)$ and $(1, 1, 1)$ constitute the set of Nash equilibria. One can notice that, as mentioned previously, this set can be Pareto ranked. Decision vectors $(0, 0, 0)$ and $(1, 0, 0)$ yield lower payoffs for all group members compared to $(1, 1, 0)$, and this equilibrium is Pareto dominated by $(1, 1, 1)$. Vector $(1, 1, 1)$ is the unique Pareto efficient outcome of the game. This game structure allows to avoid any conflict of interest between agents. A recent study by Isoni et al. (2018) tends to suggest that the disruptive effect of payoff asymmetries on coordination mainly results from the existence of such a conflict of interest. As such, our game structure allows to focus on the specific effect of different types of social preferences in a context where there is no conflict of interest.

Third, we think that both selfish preferences and social preferences (total payoff, disadvantageous and advantageous inequality aversion) may play a role in the way the subjects make their choice in our experimental games. However, we cannot manipulate separately total payoff, inequality and the individual payoffs in the coordination game considered here. However, we can keep one of these three components constant, and manipulate the other two. Since we are primarily interested in social preferences, we choose to keep the payoff vector of one agent unchanged (namely, the agent who is assigned role C) between the two treatments. Section 2.2 will highlight that our setting is such that one type of subjects (Role-C subjects) allows for clear-cut and different predictions about the effect of inequality aversion versus total payoff motivations.

Finally, we choose to convey no information to participants about the situation considered in Bernstein and Winter (2012) who interpret their model as a situation where agents choose to participate or not in a project with heterogeneous externalities (see Appendix A). We present the game in an abstract form and ask the subjects whether they prefer to choose between 0 and 1. As in any experiment, we face here the trade-off between a decontextualized and a contextualized setting, which both have their advantages and drawbacks. We choose to follow our main reference in the literature on social preferences (Engelmann and Strobel, 2004) as well as many references on coordination games (see for instance Faillo et al., 2017, Lòpez-pèrez et al., 2015 or Parravano and Poulsen, 2015): as such, we consider a decontextualized setting. Furthermore, this is used to avoid framing effects that may bias subjects' decisions.[16]

2.2 Qualitative predictions

There are at least two strategic and behavioral aspects that are not accounted for in Bernstein and Winter (2012) that may play an important role in our laboratory experiment.

First, inspecting the payoff matrices suggests that strategic risk may play an important role in the way subjects play the game. We will be more specific about this below. Second, subjects may have social preferences. Two main broad categories of social preferences models are "inequality

[16]See Kahneman and Tversky (1979) or Druckman (2001) for more details on this topic.

aversion" and "social welfare". Specifically, we here consider cases where agents may be averse to inequality (*a la* Fehr and Schmidt 1999) or have total payoff motivation (a special case of the model introduced in Charness and Rabin 2002).[17]

We now develop three sets of predictions based on assumptions of strategic risk and social preferences. We first provide predictions assuming that subjects take strategic risk into account and have standard preferences. We then make predictions assuming that subjects are averse to inequality. We finally provide predictions assuming that subjects are motivated by the possibility to increase total payoff.

In order to derive predictions in the case of strategic risk, we make the following weak assumptions. We say that subject with role $i = A, B, C$ is "more likely to choose 1 in the high inequality treatment than in the low inequality treatment" if the difference between her expected utility when she chooses 1 and when she does not is larger in the high inequality treatment than in the low inequality treatment. In the case of inequality aversion and total payoff motivation, we make the following weak assumption. We assume that the probability that a subject chooses 1 is an increasing function of the difference between the utility she gets when she chooses 1 and when she does not for all possible configurations of the other group members' choices. Formally, we say that subject with role $i = A, B, C$ is "more likely to choose 1 in the high inequality treatment than in the low inequality treatment" if

$$Pr(\text{agent } i \text{ chooses } 1|\text{treatment=High ineq.}) > Pr(\text{agent } i \text{ chooses } 1|\text{treatment=Low ineq.}).$$

Taking the example of $i = A$, the probability function is thus defined as follows:

$$Pr(\text{agent } A \text{ chooses } 1|\text{treatment=T}) =$$
$$F_A\left(u_A^T(1,1,1) - u_A^T(0,1,1), u_A^T(1,1,0) - u_A^T(0,1,0), u_A^T(1,0,1) - u_A^T(0,0,1), u_A^T(1,0,0) - u_A^T(0,0,0)\right),$$

where $T = \text{High ineq.}, \text{Low ineq.}$, and F_A is a strictly increasing function in all its arguments. The probability function corresponding to $i = B, C$ is defined in a similar way, keeping in mind that F_i is an increasing function of all its arguments.

Before discussing predictions, a first feature of the game setting will be useful. It is easily checked that choosing 1 is a dominant strategy for role A subjects, choosing 0 is a (weakly) dominant strategy for role C subjects, while there is no dominant strategy for role B subjects.

A1. ***Strategic risk:*** Assume that the subjects have standard preferences and that they take strategic risk into account. Specifically, we here assume that each agent is uncertain about the choices of the other members of the group: as such, he assumes that any given member will choose 0 with probability $p \in]0, 1[$. Each agent can then compute the expected payoff resulting from choosing 1 and compare it to the expected payoff when choosing 0. An agent's incentive to

[17] We do not consider intention-based behaviors as in Rabin (1993).

choose 1 over 0 is then given by the difference in expected payoffs. So, in this first case, an agent's utility function is given by the expected payoffs. We have the following predictions:

Proposition 1 *If there is some strategic risk and each subject assumes that any other given subject of her group will choose 0 with probability* $p \in]0,1[$ *then we have the following conclusions:*

- *Role-A and role-B subjects are more likely to choose* 1 *in the high inequality treatment than in the low inequality treatment;*
- *Role-C subject's likelihood to choose* 1 *is not influenced by whether the high or the low inequality treatment is considered.*

All proofs are relegated in Appendix B.

Hence, if the subjects are sensitive to some strategic risk, the predictions highlight that one might expect subjects to exhibit different behaviors according to their respective roles in the group. Specifically, one might expect differences between role-A and role-B subjects on one side, and role-C subjects on the other side. Role-A subjects are clearly more likely to choose 1 in the high inequality treatment as choosing 1 remains a dominant strategy while individual stakes are getting higher at the same time. Regarding role-B subjects, while choosing 0 yields the same expected payoffs in both treatments, expected payoffs when choosing 1 increase when moving from the low inequality to the high inequality treatment. Indeed, payoffs strictly increase in two cases, which correspond to the participation of role-A group member. As a result, the difference in expected payoffs increases when moving from the low inequality to the high inequality treatment. Finally, role-C subjects' expected payoffs corresponding to each choice of strategy remain unaffected when moving from the low inequality to the high inequality treatment.

If one interprets these results in relative terms, then they suggest that coordination success is more likely in the high inequality treatment: role-A and role-B subjects are more likely to choose 1 while role-C subjects' behavior should remain pretty much the same.

A2. ***Inequality aversion:*** Now, assume that players have some aversion to differences between subjects' payoffs (Fehr and Schmidt, 1999). So, for any $i = A, B, C$, agent i's utility function is

$$U_i = x_i - \frac{\alpha_i}{2}\sum_{j\neq i}\max\{x_j - x_i, 0\} - \frac{\beta_i}{2}\sum_{j\neq i}\max\{x_i - x_j, 0\}$$

where x_i denotes agent i's own material payoff, while $\alpha_i \geq 0$ denotes this agent's disadvantageous inequality aversion parameter, and $\beta_i \geq 0$ denotes this agent's advantageous inequality aversion parameter. It is usually assumed that $\alpha_i \geq \beta_i$ and $1 \geq \beta_i$ are satisfied. We obtain:

Proposition 2 *If the subjects have inequality aversion preferences, we have the following conclusions:*

- *A role-A subject is more likely to choose* 1 *in the high inequality treatment than in the low inequality treatment;*
- *A role-B subject may be more or less likely to choose* 1 *in either treatment;*
- *A role-C subject is less likely to choose* 1 *in the high inequality treatment than in the low inequality treatment.*

All proofs are relegated in Appendix B.

The case of inequality aversion yields contrasting predictions: one should expect role-A subjects and role-C subjects to have opposite behavioral patterns, while role-B subjects' adjustment may be ambiguous. Intuitively, regarding role-A subjects, even though differences between group members' payoffs tend to increase when moving from the low inequality to the high inequality treatment (which makes role-A subjects less likely to choose 1), material payoffs of role-A subjects tend to increase as well (which makes role-A subjects more likely to choose 1). This second effect is actually dominant (because $\beta_i \leq 1$), and role-A subjects are more likely to choose 1 in the high inequality treatment. By contrast, role-C subjects' material payoffs remain unaffected when moving from one treatment to the other, while differences between group members' payoffs tend to increase when moving from the low inequality to the high inequality treatment (which makes role-C subjects less likely to choose 1). Overall, role-C subjects are less likely to choose 1 in the high inequality treatment. Finally, the case of role-B subjects is ambiguous, as the effect resulting from changes in material payoffs is not as clear-cut as for role-A subjects. Indeed, role-B subject's payoffs depend on their advantageous inequality aversion parameter α_i (as for role-A subjects) and also on their advantageous inequality aversion parameter β_i (differently from role-A subjects).

These predictions on individual strategies imply in turn that the overall impact on coordination success should be ambiguous when moving from one treatment to the other, as role-A and role-C subjects have always opposite patterns.[18]

A3. ***Total payoff motivation:*** Last, assume that the subjects put some weight on total payoff. That is, role-i agent's utility function looks like:[19]

$$U_i = \gamma_i x_i + (1-\gamma_i) \sum_{l=A,B,C} x_l = x_i + (1-\gamma_i) \sum_{l \neq i} x_l$$

where $1-\gamma_i \in [0,1[$ denotes the weight put on the payoffs of the other members of the group. We have the following result:

[18] If one assumes that the chances of coordination success are higher if all group members have lower incentives to deviate, these are related to the difference between a subject's payoff when deviating and the one corresponding to $(1,1,1)$, for all roles.

[19] This utility function corresponds to the one introduced in Charness and Rabin (2002) on page 852 without the Rawlsian component, that is $\delta = 0$.

Proposition 3 *If the subjects have some total payoff motivation, all subjects (role-A, role-B and role-C) are more likely to choose* 1 *in the high inequality treatment than in the low inequality treatment.*

All proofs are relegated in Appendix B.

From an individual point of view, the conclusion is non-ambiguous: intuitively, as overall stakes are getting higher when moving from the low inequality to the high inequality treatment, individuals who are motivated to some extent by total payoff motivation should be more likely to choose 1 in the high inequality treatment. This conclusion yields in turn a clear prediction: coordination success is more likely in the high inequality treatment than in the low inequality treatment.

These three sets of predictions will allow us to discriminate between these three kinds of preferences (strategic risk, inequality aversion and total payoff motivation).

3 Experimental design and procedures

The experiment was conducted using the Experimental Economics Laboratory (*laboratoire Montpellierain d'économie experimentale*, LEEM), at the University of Montpellier (France). We ran 16 sessions with 15 or 18 participants each (a total of 270 subjects). We used the Online Recruitment Software for Economic Experiments (ORSEE) (Greiner, 2015) to recruit subjects and the Z-Tree software to program and conduct the experiment (Fischbacher, 2007). Average earnings were around 14 € net of show up fees.[20] Each session lasted about one hour.

Upon arrival in the experimental room, each subject was asked to sit in front of an individual desk computer. Instructions (see Appendix F) were circulated and read aloud by the experimenter before each game. Participant subjects were requested to make their decision without any form of communication. Participants were informed that they would be paid according to the outcome generated by one randomly chosen treatment out of two. They would be paid for sure the earnings corresponding to the outcome of the first period plus the earnings corresponding to the outcome of one randomly selected period between the nine remaining periods. We expect that subjects thus played very carefully in the first period of each treatment. For our baseline results we use data on all the periods and we provide results using data on the first periods only as a robustness check.

Participant subjects were informed that, before the experiment, their computer were randomly matched into groups of three. In each group, subjects were randomly assigned a role, that can be either role A, role B or role C. Each role corresponds to a specific column in each payoff matrix. Subject were told that the payoffs are in experimental currency (ECU) and that their gains will be converted into euros using the exchange rate of 1 € $\simeq$ 11 ECUs.[21]

[20] Show up fees were 6 € for participants coming from outside the University of Montpellier and 2 € for the students from the University of Montpellier.

[21] Using ECU allows to provide simple forms of payoffs (avoiding decimal numbers).

An experimental session consisted of two treatments, three additional modules, and a short socio-demographic characteristics survey. Table 3 summarizes the experimental design. Treatments and modules are exhibited in Block 1 and Block 2, respectively. Block 1 refers to the two treatments played in a specific (random) order: around half of the groups played the two treatment according to order 1 (High inequality then Low inequality), and the other groups played the two treatments according to order 2 (Low inequality then High inequality). As a consequence, each group played the two treatments according to one of the two orders. Block 2 refers to the three additional modules.

Table 3: Orders in the experiments

	Block 1		Block 2			Survey
Order 1 (High then Low ineq.)	*High ineq.*	*Low ineq.*	*MD*	*Ult*	*HL*	yes
Order 2 (Low then High ineq.)	*Low ineq.*	*High ineq.*	*MD*	*Ult*	*HL*	yes

Let us first describe the content of Block 1. For each of the two treatments, participant subjects were invited to play 10 rounds. Each round was split into two stages:

1. **Decision:** Subjects first get the common knowledge payoff matrix from Table 1 or 2, then they decided whether to play 0 (we call this choice *non participation*) or 1 (we will call this choice *participation*). We used neutral terminology in the instructions in order to avoid framing effects that may bias subjects' decisions.[22]

2. **Payoffs:** Once the subjects' decisions were completed, a group outcome was reached and displayed to each group member. Subjects then receive payoffs that are equivalent to the one indicated by the reached combination outcome.

Now let us describe the content of block 2, that is the three additional modules. Subjects first played a modified dictator game. Then subjects played an ultimatum game.[23] Finally, they played a multiple price-list lottery game. The modified dictator game allows us to estimate individuals' degrees of aversion toward advantageous inequality (Blanco et al., 2011) as well as a proxy for subjects' altruism. The ultimatum game allows us to estimate subjects' degrees of aversion toward disadvantageous inequality (Blanco et al., 2011), and the multiple price-list lottery game allows us to estimate a measure of their risk aversion (Holt and Laury, 2002). Further details concerning these modules and the estimates of subjects' preferences can be found in Appendix D.

Last, subjects were asked to fill a short socio-economic survey including information on their age and gender. Summary statistics of our sample can be found in Appendix D (Table 11).

Before going further, let us discuss two important choices we made in this experiment. First, we use a within setting for analytical purposes (each group plays the two treatments). A within setting

[22]See Kahneman and Tversky (1979) or Druckman (2001) for more details on this topic.
[23]See Güth et al. (1982), Camerer and Thaler (1995), and Thaler (1988).

allows for within group and within individual comparison as it allows us to control for group and individual invariant characteristics and makes a more powerful statistical analysis possible. This type of design increases the number of independent observations (for a fixed number of subjects) and by the same vein the precision of the statistical tests (e.g. see Charness et al., 2012). However, we have to deal with the possibility that order effects are present,[24] that is, subjects might be sensitive to the given order of the treatments. Confounding variables can then interfere with the effect of the treatment and bias the results of the experiment. We follow Budescu and Weiss (1987) to control for order effects. They suggest counterbalancing the treatments among the sessions. Block 1 in Table 3 is built to counterbalance the orders.

Second, we use a partner setting. Indeed, groups were formed and roles were assigned at the beginning of the experiment and they remained unchanged during all the experiment. Specifically, the matching of participants in groups applies for the two series of ten rounds. This setting may generate reputations effects within groups and these effects evolve from one period to the following (e.g. see Andreoni et al., 2008). We use two different strategies to take these effects into account. First, due to the way the individual total payoff was computed, we expect that the subjects focused on the first periods of each treatment like in a single-shot game. In our analysis, we provide results when using all the periods and when using the sub-sample of the first periods only. Notice that these random payments also allow us to eliminate wealth accumulation effects (Samuelson, 1963; Rabin, 2000).[25] Second, when we consider all the periods in our regressions, we cluster the standard errors at the group level in order to correct autocorrelation that can be due to reputation effects or other phenomena that generate correlation between different periods.

4 Results

4.1 Data and descriptive statistics

Our sample is based on observations of decisions made by 90 groups (composed of three subjects), among which 46 played with order 1 and 44 played with order 2. Our data consists of 5400 individual decisions and 1800 group outcomes.

Table 4 provides descriptive statistics on the frequency of the various outcomes. Groups reached the efficient outcome (i.e. they played (1, 1, 1)) 20.5% of the time. They played the Nash equilibrium in which none of the players choose to participate (i.e. they played (0,0,0)) 10% of the time, the Nash equilibrium in which only player A participates (i.e. they played (1,0,0)) 41% of the times, and the Nash equilibrium in which subjects A and B participate but not C (i.e. they played (1,1,0)) 21% of the time.

Table 4 also provides descriptive statistics on individual participation. Subjects chose to participate 52% of the time. Subjects A's participation rate is 86%. Recall that choosing 1 is a dominant

[24] See Schuman et al. (1981) for further details about order-effects in experiments.
[25] See also Heinemann (2008) for more details on how to measure wealth effects.

strategy for these subjects. Subjects B chose to participate almost half less often (44%). Subjects C chose to participate 26% of the time. Recall that choosing 0 is a weakly dominant strategy for these subjects.

Table 4: Descriptive statistics: outcome

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Group level					
Nash Eq. (1, 1, 1)	0.205	0.404	0	1	1800
Nash Eq. (1, 1, 0)	0.212	0.409	0	1	1800
Nash Eq. (1, 0, 0)	0.407	0.491	0	1	1800
Nash Eq. (0, 0, 0)	0.101	0.302	0	1	1800
Individual level					
Indiv. participation	0.523	0.500	0	1	5400
Subj. A participation	0.863	0.344	0	1	1800
Subj. B participation	0.443	0.497	0	1	1800
Subj. C participation	0.262	0.440	0	1	1800

Notes: The sample consists of 270 subjects, 90 groups playing 2 treatments with 10 repetitions each. In the table *participation* means that the individual chooses 1.

Individual characteristics from the survey (age, gender) and estimated by using the three modules (risk aversion, inequality aversion and altruism) are presented in Appendix D (Table 11).

4.2 The effect of inequality on coordination

In order to study how inequality affects individual participation decisions and coordination sucess, we compare the two treatments using two different strategies. First, we perform a between-subject analysis using the first ten periods only. Second, we take advantage of the within-subject design and use a difference-in-difference estimation strategy that controls for individual characteristics, as well as sequence and order effects.

4.2.1 Between groups/subjects analysis

We first focus on the first ten periods to perform a between-subject analysis: we compare the data from the high inequality treatment for groups (and individuals) playing the high inequality treatment first (Order 1) with the data from the low inequality treatment for groups (and individuals) playing the low inequality treatment first (Order 2).

Table 5 summarizes the results. Column (1) provides the rate of occurrence of the various Nash equilibria in the two treatments. In columns (2) and (3), we provide the average value of the outcome (at the group or at the individual level) for the low inequality treatment and the high inequality treatment, respectively. In column (4), we provide the results of non parametric tests (Wilcoxon rank-sum tests) of equality of the distributions of coordination success or of individual participation in each treatment over the relevant ten repetitions of the game (we report the z-score and the p-value between brackets).

Table 5: Between groups/subjects analysis

	(1) **Mean**	(2) **Low ineq.**	(3) **High ineq.**	(4) **Wilcoxon rank sum stat.**	(5) **p-value**	(6) **Obs.**
Group level						
Nash Eq. (1, 1, 1)	0.201	0.114	0.285	-6.400***	0.000	900
Nash Eq. (1, 1, 0)	0.212	0.175	0.200	-0.659	0.337	900
Nash Eq. (1, 0, 0)	0.387	0.452	0.324	3.951***	0.000	900
Nash Eq. (0, 0, 0)	0.112	0.166	0.072	4.379***	0.000	900
Individual level						
Indiv. participation	0.513	0.430	0.593	-8.441***	0.000	2700
Subj. A participation	0.832	0.791	0.872	-3.242***	0.001	900
Subj. B participation	0.424	0.311	0.533	-6.709***	0.000	900
Subj. C participation	0.283	0.189	0.374	-6.162***	0.000	900

The results are as follows. Groups achieved coordination on the Pareto efficient equilibrium 25% of the time in the high inequality treatment and 16% of the time in the low inequality treatment and the difference is statistically significant. The Nash equilibrium in which subjects A and B participate and subject C does not participate was not significantly more frequent in the high inequality treatment than in the low inequality treatment (23% in the high inequality treatment versus 19% in the low inequality treatment). The Nash equilibrium in which subject A is the only one to participate was significantly less frequent in the high inequality treatment than in the low inequality treatment, and the Nash equilibrium in which none of the subjects participate was less frequent in the high inequality treatment than in the low inequality treatment. The largest difference between the two treatments is for the equilibrium in which all the subjects participate. This result suggests that coordination on the Pareto efficient equilibrium is facilitated in the high inequality treatment.

The results on individual participation decisions are clear cut. The high inequality treatment induces an additional 10 percentage points in the participation rate compared to the low inequality treatment, which corresponds to a 20% increase compared to the participation rate in the low inequality treatment (47%). For each role, participation is significantly larger in the high inequality treatment than in the low inequality treatment. Role-A subjects' participation rate is 5 percentage points larger in the high inequality treatment than in the low inequality treatment (it corresponds to a 4% increase). The difference is modest but statistically significant. For role-B subjects, the difference between the two treatments is both large and significant. The participation rate is 15 percentage points larger in the high inequality treatment than in the low inequality treatment (and it corresponds to a 40% increase). More strikingly, the difference between the two treatments is also large (and significant) for role-C subjects. Their participation rate is 10 percentage points larger in the high inequality treatment than in the low inequality treatment (and it corresponds to a 48% increase). This result is not consistent with the qualitative predictions in Proposition 1 and 2 (standard preferences with strategic risk and inequality aversion) while it is consistent with the qualitative prediction in Proposition 3 (total payoff motivation).

Figure 1 (group level) and Figure 2 (individual level) provide time series for group coordination on the Pareto efficient equilibrium and individual participation decision (for each role). We plot a time series for each treatment.

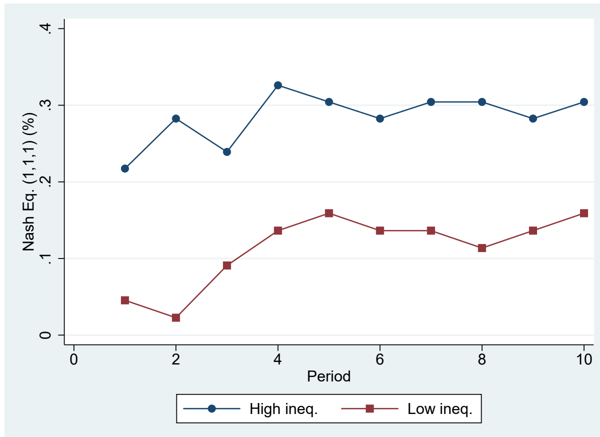

Figure 1: Coordination success per period for each treatment

Figure 1 highlights that the rate of coordination on the Pareto efficient equilibrium is higher in the high inequality treatment than in the low inequality treatment in each period. The difference is remarkably stable between periods: the average difference is 17 percentage points and the standard deviation of the difference is small (0.04). The difference between the treatments in the first period - for which the subjects knew they will get payments for sure - is also 17 percentage points.

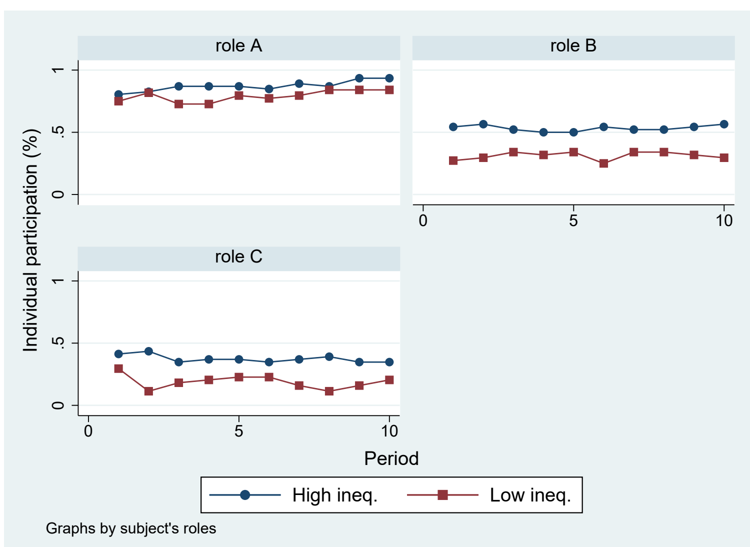

Figure 2: Individual participation by role for each treatment

Figure 2 highlights that the participation rate is higher in the high inequality treatment than in the low inequality treatment in each period for each role. As for the rate of group coordination, the difference between the two treatments is remarkably stable between periods.

These results are conclusive thanks to our qualitative predictions (section 2.2). Among the three models (strategic risk, inequality aversion, total payoff motivation), only the total payoff motivation model is consistent with all the results found in this section.

4.2.2 Difference-in-difference estimation

In this section, we take advantage of the structure of our data in order to control for individual characteristics as well as sequence and order effects. We use panel regressions estimates of a linear probability model that links the treatments and group coordination success (i.e. when a group reaches the $(1, 1, 1)$ Nash equilibrium) or individual participation decisions.

We first perform an analysis at the group-period level. Figure 3 provides time series for group coordination on the Pareto efficient equilibrium. We plot a time series for each treatment.

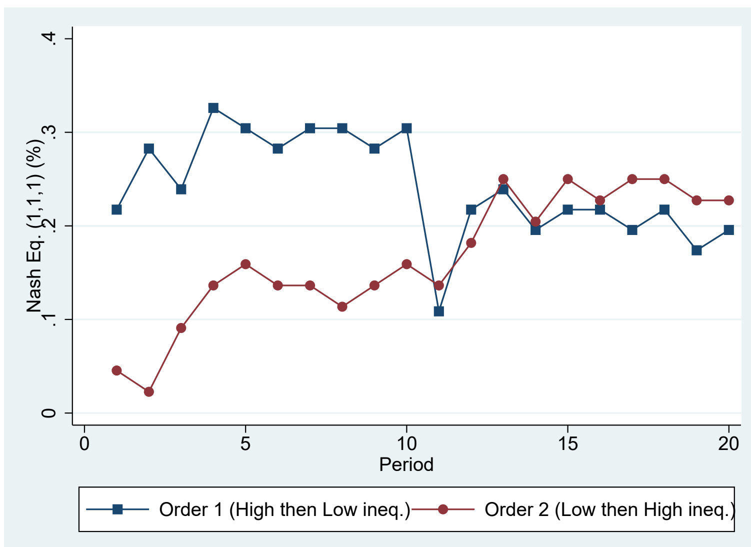

Figure 3: Order comparison (panel data)

The right hand side variable is High ineq., a dummy which is 1 if the group plays the high inequality treatment and 0 if the group plays the low inequality treatment in the current period. The outcome variable (Group Coordination) is a dummy variable which is 1 if the group achieves coordination on the Pareto efficient equilibrium in the current period and 0 otherwise. In order to control for period and group characteristics and to be able to interpret the analysis as a difference-in-difference, we include both period and group fixed effects. Notice that order effects are controlled for by the group fixed effects, since each group played the two treatments according to one of the two orders (as explained in Section 3). Also notice that since each group is formed once and the subjects are matched for the 20 periods, there may be autocorrelation in the error term. We thus cluster the standard errors at the group level.

Table 6 provides the results. Column (1) shows that the probability that a group achieves coordination is significantly higher in the high inequality treatment. The increase is as high as 9.7 percentage points compared to the low inequality treatment. In column (2) we only include the first periods (period 1 and period 11) and the result is very similar.

These results are clear-cut. However, in order to discriminate between the three models that we consider (strategic risk, inequality aversion, total payoff motivation), we go one step further and study individual participation decisions.

Table 6: Inequality and group coordination (fixed effects)

	Dependent variable: Group Coordination	
	(1)	(2)
	All periods	First periods only
High ineq.	0.0969***	0.0998**
	(0.035)	(0.039)
Model	LPM	LPM
Group FE	YES	YES
Period FE	YES	YES
Time trend	NO	NO
Obs.	1,800	180
Nb of groups	90	90
R^2	0.589	0.699

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. Group Coordination is a dummy variable equal to one if the group plays $(1, 1, 1)$ and zero otherwise. High ineq. is a dummy variable equal to one if treatment is "High ineq." and zero if treatment is "Low ineq.". LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level.

We ask whether subjects with role A, B and C are more likely to participate when inequality is low or high. In other words, we analyze the effect of the high inequality treatment - compared to the low inequality treatment - for each role A, B and C.

We answer this question using panel regressions of a linear probability model that links the treatments and individual participation decisions. The analysis is performed at the individual-period level. The right-hand side variable is defined as in the group level analysis. The outcome variable is a dummy variable which is 1 if the individual decides to participate (i.e. chooses 1) in the current period and 0 otherwise (i.e. if she chooses 0). In order to control for period and individual characteristics and to be able to interpret the analysis as a difference-in-difference, we include both period and individual fixed effects. As for the group level estimates, we cluster the standard errors at the group level.

The results are provided in Table 7. In columns (1) and (2) we use the full sample that includes the subjects for each role. In columns (3) to (5), we consider the sub-samples that include subjects with role A, role B or role C only. In column (1), we find that the probability that a subject participates is 9.9 percentage points higher in the high inequality treatment than in the low inequality treatment and that this effect is significant. In column (2) we use interaction variables in order to separate the effect of the treatments for each role A, B and C. We find that the likelihood that subjects with role A, B and C participate is higher in the high inequality treatment.[26] The likelihood that subjects with role A, role B, and role C participate is respectively 5, 15, and 10 percentage points higher in the high inequality treatment than in the low inequality treatment (and all these effects are significant). In columns (3) to (5), that is when we consider each role separately, we find very similar results to those from column (2).

Table 7: Inequality and individual decision (fixed effects)

	Dependent variable: Individual participation				
	(1) Full sample	(2) Full sample	(3) Role A	(4) Role B	(5) Role C
High ineq.	0.099***				
	(0.028)				
High ineq. x subject A		0.052**	0.052**		
		(0.024)	(0.023)		
High ineq. x subject B		0.147***		0.148***	
		(0.046)		(0.046)	
High ineq. x subject C		0.099***			0.098***
		(0.036)			(0.035)
Model	LPM	LPM	LPM	LPM	LPM
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes
Obs.	5,400	5,400	1,800	1,800	1,800
Nb of subjects	270	270	90	90	90
R^2	0.59	0.59	0.37	0.47	0.49

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. Individual participation is a dummy variable equal to 1 if the subject chooses 1 and zero otherwise. High ineq. is the outcome of the dummy for treatment when it is equal to 1. Individual participation equals 1 when subjects choose 1 and it is zero otherwise. × indicates interaction variable. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level.

The results at the group and individual level are (qualitatively) very similar to those obtained in the between groups/subjects analysis in Section 4.2.1. They show that the positive effect of inequality on coordination success is sustained by all the subjects, independently of their role. The fact that subjects with role C are more likely to participate under the high inequality treatment is not consistent with the strategic risk model or the inequality aversion model considered in Section 2.2. This is however consistent with total payoff motivation models that predict the qualitative results of the present section.

[26]We reject the equality of the three coefficients at the 10% significance level.

In order to provide evidence on the social preferences of the subjects, we first follow the same type of analysis performed in Engelmann and Strobel (2004). Specifically, we provide some estimated values for the social preference parameters (coefficients α, β and γ, see Section 2) by relying on regressions using the conditional logit model (see Chapter 15.5 in Moffatt, 2015). There is one important remark to have in mind. We do not rely on data from our three-player game when using the procedure, since the individuals do choose between different allocations but yet they play a game. As such, we decide to implement the procedure described in Moffatt (2015) using the additional dictator and ultimatum games. Notice that since these are two-player games, the problem mentioned in Section 2.1 arises here. Our specification follows Moffatt (2015) and thus we exclude OWN from the specification (we include EFF, FSA and FSD). This allows to estimate the social preference parameters of a utility function using individual decisions over different payoff allocations. The results suggest that the subjects are averse to disadvantageous inequality, and that they are motivated by total payoffs. However, we find no evidence that the subjects are averse to advantageous inequality (see Appendix D.1 for a detailed presentation of the results).

In order to provide some direct evidence on the link between individual preferences and participation decision, we use estimated measures of individual preferences that can be obtained from the three modules that were played after the two treatments (see the description of Block 2 in Section 3) and the survey.

We can then include interaction variables in our regressions in order to test whether the effect of the treatment is larger or smaller depending on whether the individual is averse to disadvantageous or advantageous inequality, altruistic or not, more or less risk averse. However, we do not find evidence of such heterogeneous treatment effects. One interpretation of this conclusion is that social preferences parameters elicited in one context are not predictive of behavior in another context. This questions the idea that such preferences are in any sense stable (see Galizzi and Navarro-Martinez, 2019). If one thinks about why that is, a possibility is that dictator and ultimatum games are in some significant sense different from coordination games.[27] We relegate the description of this analysis to Appendix D (Table 12).

In order to test whether individual preferences played a role in the subjects' decisions, we also estimated the same models as in Table 7 without individual fixed effects in order to include individual preference measures (see Table 13 in Appendix D). We do not find evidence of an effect of the individual measures except of risk aversion on the individual decision to participate. Risk aversion has a negative effect on the likelihood to participate. This is consistent with the assumption that subjects take strategic risk into account.

It may be useful to discuss the conclusion that the estimates obtained here do not have predictive power for choices made in the coordination game.[28] There may be a puzzle if one thinks about the finding that payoff asymmetries have a large disruptive effect on coordination (as first documented

[27]We thank a referee for suggesting this discussion.

[28]We thank a referee for suggesting this discussion.

by Crawford et al. (2008), and replicated by a number of authors). One way of thinking about payoff asymmetries is in the sense of payoff inequalities. But in this respect, our results would be at odds with that rather robust evidence. Another way is to think about payoff asymmetry as conflict of interest. Crawford et al. (2008), as well as many others, study games with a Battle-of-the-Sexes structure in which players have conflicting preferences over the two equilibria. A recent study by Isoni et al. (2018) uses a new game to discriminate between these two possibilities and finds that the disruption comes from (almost entirely) conflict of interest. Once this is removed, coordination success is very high even under extreme payoff inequalities. Because in the games studied in the present study there is no conflict of interest, our results seem consistent with this conclusion, and could be read as further evidence that, when it comes to coordination, inequality aversion may matter much less than it does in games in which the task is more explicitly related to the allocation of some resources between players.

4.3 Coordination over time

In each treatment, the game was repeated 10 times. It is thus natural to ask how coordination success and individual decisions evolve through time. Figure 1 suggests that coordination success increases over time and Figure 2 suggests that the rate of subjects with role A who participate tends to increase over time. However, maybe surprisingly, subjects with role B (or C) seem to participate at a rather stable rate.

To test whether group coordination and individual participation decisions are trending over time, we include time trends in the regressions instead of period fixed effects. The results are provided in Table 8. Columns (1) and (2) provide the estimates of the treatment effect and of time trends when the outcome is group coordination success. The first column uses the same specification as in Table 6 column (1) and includes a common time trend instead of period fixed effects. In column (2) we add a common trend term squared to capture non linear trends. The coefficient of the time trend variable is positive in both cases and weakly significant or non-significant. Columns (3) to (5) provide the estimates of the treatment effect and of time trends when the outcome is individual participation decision. Column (3) uses the same specification as in Table 7 column (1) and includes a common time trend instead of period fixed effects. In column (4) we add a common trend term squared to capture non linear trends. Column (5) provides the estimates of the treatment effects for each role and of role specific time trends. This column uses the same specification as in Table 7 column (2) and includes role specific time trends instead of period fixed effects. The estimates suggest that subjects tend to participate more over time (see column (4)) and they provide evidence of a positive and significant non linear trend. The estimates from column (5) suggest that role A subjects tend to participate more over time (their participation rate increases by 0.5 percentage point per period).

It is also interesting to notice that the participation decisions of the subjects are associated with what they have observed in the previous round. Indeed, individual participation decision in the

Table 8: Time trends instead of period fixed effects

Dependent variable:	Group Coord.		Individual participation		
	(1)	(2)	(3)	(4)	(5)
High ineq.	0.097***	0.097***	0.099***	0.099***	
	(0.034)	(0.034)	(0.028)	(0.028)	
High ineq. x Subject A					0.052**
					(0.023)
High ineq. x Subject B					0.147***
					(0.046)
High ineq. x Subject C					0.098***
					(0.035)
Time trend	0.002	0.010*	0.001	0.009**	
	(0.003)	(0.006)	(0.002)	(0.004)	
Time trend squared		-0.003		-0.004**	
		(0.002)		(0.002)	
Time trend x Subject A					0.005***
					(0.002)
Time trend x Subject B					0.002
					(0.004)
Time trend x Subject C					-0.004
					(0.003)
Model	LPM	LPM	LPM	LPM	LPM
Group FE	Yes	Yes	-	-	-
Indiv. FE	-	-	Yes	Yes	Yes
Period FE	No	No	No	No	No
Obs.	1,800	1,800	5,400	5,400	5,400
R^2	0.58	0.58	0.59	0.59	0.59

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. Group Coord. is a dummy variable equal to one if the group plays $(1, 1, 1)$ and zero otherwise. Individual participation is a dummy variable equal to one if the subject chooses 1 and zero otherwise. High ineq. is the outcome of the dummy for treatment when it is equal to 1. Individual participation equals 1 when subjects select "participation". $\times$ indicates interaction variable. Time trend is a common trend variable. Time trend squared is a common time trend variable squared (divided by ten). LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level.

current period is positively and significantly (at the 1% level) correlated with the previous period coordination success of the group to which the individual belongs (the coefficient of correlation is 0.34) and this correlation is the highest for subjects with role C (0.57) and for subject with role B (0.37) while it is much lower for the subjects with role A (0.17).

Our data provide information on the evolution of individual decisions and of the group coordination success that can be used to simulate what would happen if the game was repeated more than ten rounds.[29] In order to do so, we assume that the process underlying a given treatment can be approximated by a Markov chain. For each treatment, a vector of strategy is thus identified as a state of the corresponding Markov chain, and this allows us to compute the corresponding $8 * 8$ transition matrix: in order to do so, we use the data generated from the experiment to compute the empirical probability of transition between any pair of states. We also compute the initial probability of each state using the rates observed in the first period. Then, using the properties of Markovian processes, we can compute the probability that the process reaches a given state at a given time period and then, looking at a stationary distribution, it is possible to simulate how quickly the process converges to an equilibrium outcome. We perform this exercise for each treatment separately. In order to make the simulations consistent with Figure 1, we only consider data used in the between-subject analysis (i.e. the first ten periods, see Section 4.2.1). We run the process over 25 periods and provide the results in Figure 4.

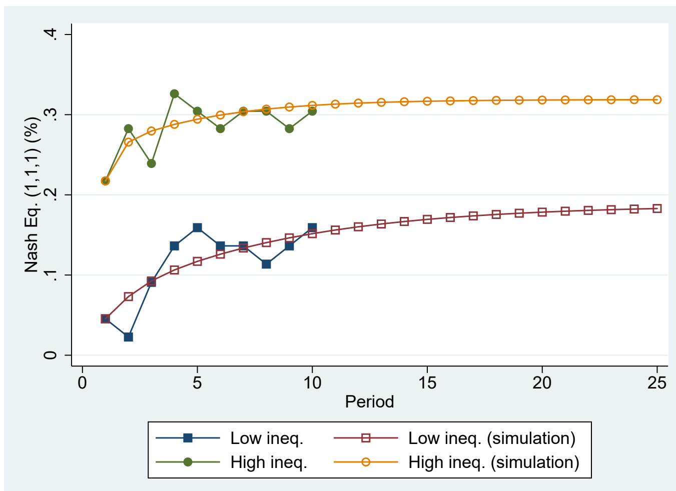

Figure 4: Simulations based on a Markov chain assumption (25 periods)

The process becomes fairly stable after twenty to twenty-five periods,[30] and it is interesting to notice that the differential between the rate of group coordination remains notable across the two treatments (around 13 percentage points). These simulations thus suggest some robustness of our results. The reader is reminded that this simulation work relies on a strong underlying assumption about the group behavior (that it may be approximated by a Markovian process).

4.4 Increasing versus decreasing inequality

While we control for order effects in our main results, it is interesting to investigate whether there was a difference in the rate of coordination success depending on the order of the treatments.

[29]We are grateful to a referee for this suggestion.

[30]Figure 6 in Appendix C provides simulation results over 50 periods.

To derive a first comparison of the two orders, we plot one graph for each treatment in Figure 5. The left hand side plot represents the average rate of coordination by period in the low inequality treatment for each order. The right hand side plot represents the average rate of coordination by period in the high inequality treatment for each order. For almost all periods, the average rate of coordination is larger for decreasing inequality (order 1: High then Low ineq.) than for the increasing inequality order (order 2: Low then High ineq.).

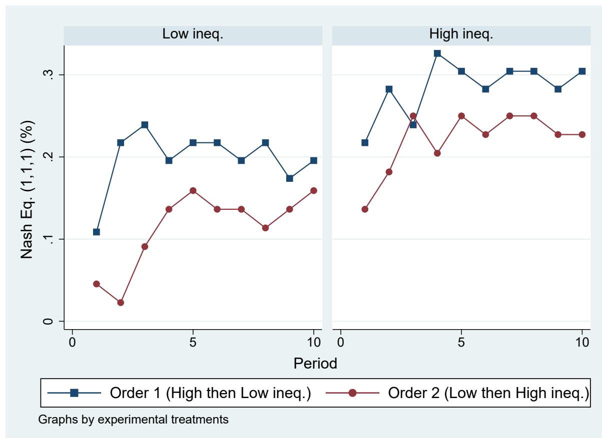

Figure 5: Order comparison by treatment

In order to provide more detailed information on the effect of the order of the treatments, we derive comparisons at the group and individual level between the effect of the high inequality treatment when it is played first (order 1: High then Low) and when it is played second (order 2: Low then High), and comparisons between the effect of the low inequality treatment when it is played first (order 2: Low then High) and when it is played second (order 1: High then Low). Table 9 provides the results. Column (1) and (2) provide the average rate of coordination success or individual participation for the decreasing inequality order (order 1: High then Low) and for the increasing inequality order (order 2: Low then High), respectively. Column (3) provides the result of a Wilcoxon rank sum test of equality of the distributions and column (4) provides the p-value.

Table 9 shows that groups who played the treatments in the decreasing inequality order (order 1: High then Low) reach the Pareto efficient equilibrium more often than the groups that played the treatment in the increasing inequality order (order 2: Low then High), and this holds for both the high inequality treatment phase (28.5% of the time in order 1 versus 22% of the time in order 2: Low then High) and the low inequality treatment phase (20% of coordination success in order 1 versus 11% in order 2: Low then High) and the differences are statistically significant. These two results confirm the observation obtained using Figure 5 that the participation rate is larger under the decreasing inequality order than under the increasing inequality order.

We find similar results as regards individual participation. Subjects who played the treatments in the decreasing inequality order (order 1) participate more often than the subjects that played the

Table 9: Between groups/subjects analysis of order effects

	(1) **High then Low (order 1)**	(2) **Low then High (order 2)**	(3) **Wilcoxon rank sum stat.**	(4) **p-value**	(5) Obs.
Group Coordination					
Low ineq. treatment	0.198	0.114	3.471***	0.000	900
High ineq. treatment	0.285	0.220	2.217**	0.027	900
Individual participation					
Low ineq. treatment	0.514	0.430	4.379***	0.000	2700
High ineq. treatment	0.593	0.551	2.204**	0.028	2700

Notes: Group Coordination is a dummy variable equal to 1 if the group plays $(1, 1, 1)$ and zero otherwise. Individual participation is a dummy variable equal to 1 if the subject chooses 1 and zero otherwise. z is the z-score of a Wilcoxon rank-sum test of equality of the distributions. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

treatment in the increasing inequality order (order 2), and this holds for both the high inequality treatment phase (59% of the time in order 1 versus 55% of the time in order 2) and the low inequality treatment phase (51% of coordination success in order 1 versus 43% in order 2) and the differences are statistically significant.

These results suggest that the order of treatments matters. Subjects who belong to groups facing over time a reduction in inequalities participate more often and then reach the efficient outcome more often, over the entire experiment, compared to subjects who belong to groups facing over time an increase in inequalities.[31]

We conjecture that this order effect might be due to the fact that subjects assess the level of strategic risk based on their past experience. Let us elaborate on this point. In light of the results from the previous sections, we know that the probability of coordination success is larger in the high inequality treatment than in the low inequality treatment (see Section 4.2). The results obtained thanks to individual preference measures suggest that strategic risk plays a role in the way subjects make their choices in the experiment (see Table 13 in Appendix D). Using these results, we believe that the order effect may be explained as follows. Assume that the subjects assess the magnitude of strategic risk based on their past experience of coordination success. In the decreasing inequality order, the participation rate is large in the first - high inequality treatment- phase (59%). In the second -low inequality treatment - phase, the participation rate is 8 percentage point lower. It is however still relatively high. This observation is consistent with the conjecture that the subjects estimate - based on the observation of a high participation rate in the first phase - that the strategic risk will remain relatively low and then they continue to participate quite frequently. By contrast, in the increasing inequality order, the participation rate is relatively low in the first - low inequality treatment- phase (43%). In the second -high inequality treatment- phase, the participation rate

[31] Another possible interpretation is that the rate of coordination success converges to a specific value (slightly above 0.20) independently of the order of the treatments. It is however difficult to find an explanation for the latter interpretation.

is 12 percentage points larger. It is however not as large as the participation rate in the high inequality treatment for the decreasing inequality order. This observation is consistent with the conjecture that the subjects estimate - based on the observation of a low participation rate in the first phase - that the strategic risk will remain relatively high and then they still choose not to participate quite frequently.

5 Alternative interpretations

5.1 The treatment effect

In our analysis we rely on predictions based on some of the main categories of individual and social preferences.[32] Specifically, we use different predictions based on several assumptions about subjects' behavior, namely: selfish preferences (with an emphasis on the possibility of strategic risk), inequality aversion, and total payoff motivations. Even though these are reasonable assumptions, we would like to point out other possibilities that might open ways for future research.[33] First, due to the game setting, a role C subject could also choose to participate because of altruism if role A and role B subjects are expected to participate. However, since the payoff of role C subjects remains unchanged in the two treatments, it seems difficult to make a clear difference between altruism and total payoff motivation for such subjects. Second, one may notice that subjects are exogenously awarded a specific role in each group. As such, all payoffs are somehow predetermined by the experimenter, unlike ultimatum games or dictator games, in which participants are responsible for the inequality that is generated in the laboratory. One possibility could be that, if role A subject is lucky to be in that position, role C subject may not feel much resentment and choose to participate.[34] An interesting follow-up to this work would thus consist in designing a real-effort game that would determine role allocations, and to analyze whether the results would vary significantly. A final possibility is that, given that the game has feedback, role C agent may not like the idea that other subjects (or the experimenter) perceive them as depriving others from free money. In other words, even if the game is anonymous, role C subjects may have some self-image concerns that are made more salient by the feedback. An interesting follow-up to this work would thus consist in testing the role of the feedback in comparing treatments with and without feedback. Other motivations, such as status seeking, may provide explanations for the behavior of some subjects but not all. Indeed, one may think that status seeking can explain why subjects with role A participate more in the high inequality treatment. However, status seeking cannot explain why subjects with role C –for which none of the payoffs is changed between the two treatments– participate more in the high inequality treatment.

[32] We do not consider intention-based behaviors as in Rabin (1993).

[33] We are grateful to a referee for these suggestions.

[34] This relates to the concept of procedural fairness (Bolton et al., 2005).

5.2 The order effect

We provide a possible explanation for the order effect documented in Section 4. Another possible explanation for this effect might be related to the complex nature of the game,[35] even though it exhibits the minimum amount of extra complexity to tackle our research question. Some authors have shown that subjects' behavior more closely match the predictions of rational-behavior theories as the cognitive costs decrease and as the stakes of the decision increase (Smith and Walker, 1993; Camerer and Hogarth, 1999; List and Lucking-Reiley, 2000). If participants are confused about what to do when a table with 24 numbers is presented to them, the presence of larger numbers may make it easier for participants to see differences between alternative courses of action and make up their mind about what to do. This would be consistent with the higher level of coordination success in high inequality relative to low inequality treatment when it is presented first, and with the fact that, after the high inequality treatment, low inequality treatment results in greater coordination success than when it is faced as first task. The idea would then be that, having seen the solution with larger differences, participants are more likely to use it when differences are smaller.

6 Conclusion

Coordination is often required to reach an efficient outcome, and the effect of social preferences in coordination problems is a question that has received little attention.

In this paper, we report the results from an experiment where the subjects face a coordination problem and we compare a situation in which the coordination payoffs are close to equal with a situation in which some of the subjects' coordination payoffs are increased.

We show that groups reach the efficient outcome more frequently in the second case, and that subjects play the strategy that corresponds to this outcome more frequently even when their individual payoffs are unchanged. This suggests that subjects are motivated by total payoff maximization rather than by inequality aversion considerations.

Our results suggest that larger levels of welfare for some but not all increases coordination success. Specifically, a larger surplus tends to facilitate the emergence of an efficient outcome, even if inequality levels are higher. Yet, this does not mean that one should necessarily make the size of the "pie" larger over time if inequalities simultaneously increase. The final results suggest that situations where groups of agents face over time a reduction in inequalities facilitate coordination, compared to situations where groups face over time an increase in inequalities.[36]

This study thus contributes to understanding whether social preferences and variations in inequality affect the outcome of coordination problems. Obviously, more work is needed to develop our understanding of this important problem.

[35] We are grateful to a referee for suggesting this explanation and motivating the related discussion.

[36] This conclusion is stated in strictly relative terms, as we abstract from the case where both the size of the "pie" and inequalities would remain large over time.

Appendices

A Computation of the payoff matrices

We now explain how the two tables are obtained by relying on the setting introduced in Bernstein and Winter (2012). The game setting considered in Bernstein and Winter (2012) corresponds to a participation problem, where each agent decides to participate in a joint project or not. Participation results in positive externalities for participating members, and the bilateral externalities between the agents can be characterized by the following matrix:

$$w = \begin{pmatrix} w_A(A) & w_A(B) & w_A(C) \\ w_B(A) & w_B(B) & w_B(C) \\ w_C(A) & w_C(B) & w_C(C) \end{pmatrix} \tag{1}$$

where $w_i(j)$ denotes the added benefit for agent i when participating jointly with agent j. Since an agent does not gain additional benefit from own participation $w_i(i) = 0$ is satisfied. Agent i's benefit from participating with a set of players M is $\sum_{j \in M} w_i(j)$. If an agent decides not to participate then he gets a payoff of c, which corresponds to the outside option.

We choose values for the externalities such that when all the players choose to participate, the differences between the players' payoffs are very small in the low inequality case and relatively large in the high inequality case. In the high inequality case, the matrix specifying the externalities is

$$w_h = \begin{pmatrix} 0 & 30 & 21 \\ 31 & 0 & 27 \\ 22 & 28 & 0 \end{pmatrix} \tag{2}$$

while in the low inequality case, the matrix is

$$w_l = \begin{pmatrix} 0 & 1 & 1 \\ 31 & 0 & 1 \\ 22 & 28 & 0 \end{pmatrix} \tag{3}$$

The value of the outside option is $c = 60$ for both cases.

In order to ensure that participation of all members is an equilibrium outcome of the participation game, Bernstein and Winter (2012) characterize an appropriate incentive structure $v = (v_A, v_B, v_C)$ such that agent i gets payoff v_i if he participates and 0 if he does not participate. The resulting participation game is such that, if M denotes the set of agents who decide to participate, then agent $i \in M$ obtains $v_i + \sum_{j \in M} w_i(j)$, and each agent who does not participate gets the outside option c.

We choose parameter values to ensure that the incentive structure is identical in our two cases.[37]

[37] As such the efficient outcome is always an equilibrium of the induced coordination game, and the change in

Specifically, this incentive structure is given by $(v_A, v_B, v_C) = (c,\ \ c-w_B(A),\ \ c-w_C(A)-w_C(B)) = (60, 29, 10)$. Now it remains to compute the payoffs derived from the different vectors of agents' decisions in the resulting participation game.

We provide the computations for the "High ineq." treatment. First, the vector of decisions $(0, 0, 0)$ corresponds to a payoff vector $(c, c, c) = (60, 60, 60)$.

Secondly, consider the case where only one agent participates. If agent A decides to participate while agents B and C do not, the corresponding payoff vector is $(c + 0, c, c) = (60, 60, 60)$. If agent B decides to participate while the other agents do not, then one obtains $(c, c - w_B(A), c) = (60, 29, 60)$. If agent C decides to participate while the other agents do not, then one obtains $(c, c, c - w_C(A) - w_C(B)) = (60, 60, 10)$.

Now consider that only two agents decide to participate. If agents A and B are the only participating members, then one obtains $(c+w_A(B), c-w_B(A)+w_B(A), c) = (90, 60, 60)$. Similarly, we obtain that decision vector $(1, 0, 1)$ corresponds to payoff vector $(c + w_A(C), c, c - w_C(A) - w_C(B) + w_C(A)) = (81, 60, 32)$, while decision vector $(0, 1, 1)$ corresponds to payoff vector $(c, c - w_B(A) + w_B(C), c - w_C(A) - w_C(B) + w_C(B)) = (60, 56, 38)$.

Finally, if all agents decide to participate, the decision vector is given by $(1, 1, 1)$ and the resulting payoff vector is $(c + w_A(B) + w_A(C), c - w_B(A) + w_B(A) + w_B(C), c - w_C(A) - w_C(B) + w_C(A) + w_C(B)) = (111, 87, 60)$.

Collecting all payoff vectors, we obtain Table 1 corresponding to the high inequality case (Table 2 that corresponds to the "Low ineq." treatment is computed in a similar way).

B Proofs of the Propositions

B.1 Proof of Proposition 1

In the low inequality treatment we can compute the difference in role-A agent's expected payoffs when choosing 1 and when choosing 0:

$$U_A(1) - U_A(0) = 2p\,(1-p) + 2\,(1-p)^2$$

Similarly, in the high inequality treatment, we obtain:

$$U_A(1) - U_A(0) = 51p\,(1-p) + 51\,(1-p)^2$$

We thus conclude that this difference is larger when moving from the low to the high inequality treatment. Together with the assumption on the relationship between an agent's probability to choose 1 and the above differences, this concludes the proof for role-A agent.

payoffs resulting from incentives does not drive differences from one treatment to the other, since this change is the same for both cases.

In the low inequality treatment we then compute the difference in role-B agent's expected payoffs when choosing 1 and when choosing 0:

$$U_B(1) - U_B(0) = 1 - 32p$$

Similarly, in the high inequality treatment, we obtain:

$$U_A(1) - U_A(0) = 27 - 58p$$

We thus conclude (as $0 \leq p \leq 1$) that this difference is larger when moving from the low to the high inequality treatment. Together with the assumption on the relationship between an agent's probability to choose 1 and the above differences, this concludes the proof for role-B agent.

Finally, in the low inequality treatment we compute the difference in role-C agent's expected payoffs when choosing 1 and when choosing 0:

$$U_C(1) - U_C(0) = -\left[50p\,(1-p) + 50p^2\right]$$

Similarly, in the high inequality treatment, we obtain:

$$U_C(1) - U_C(0) = -\left[50p\,(1-p) + 50p^2\right]$$

We thus conclude that this difference remains unaffected when moving from the low to the high inequality treatment. Together with the assumption on the relationship between an agent's probability to choose 1 and the above differences, this concludes the proof for role-C agent.

B.2 Proof of Proposition 2

In the low inequality treatment, we can compute the following differences:

$$U_A(1,0,0) - U_A(0,0,0) = 0;\;\; U_A(1,1,0) - U_A(0,1,0) = 1 + \frac{29}{2}\beta_A$$

$$U_A(1,0,1) - U_A(0,0,1) = 1 + 10\beta_A;\;\; U_A(1,1,1) - U_A(0,1,1) = 2 + \frac{49}{2}\beta_A$$

Similarly, in the high inequality treatment, we obtain:

$$U_A(1,0,0) - U_A(0,0,0) = 0;\;\; U_A(1,1,0) - U_A(0,1,0) = 30 - \frac{29}{2}\beta_A$$

$$U_A(1,0,1) - U_A(0,0,1) = 21 - 10\beta_A;\;\; U_A(1,1,1) - U_A(0,1,1) = 51 - \frac{49}{2}\beta_A$$

We can finally compare a given difference in either the high inequality or the low inequality treatment. We easily derive that $U_A(1,0,0)-U_A(0,0,0)$ is the same in both treatments, so the difference is non negative. Second, $U_A(1,1,0)-U_A(0,1,0)$ is larger when moving from the low to the high inequality treatment (the difference is equal to $29\,(1-\beta_A)\geq 0$). Third, $U_A(1,0,1)-U_A(0,0,1)$ is larger when moving from the low to the high inequality treatment (the difference is equal to $20\,(1-\beta_A)\geq 0$). Finally, $U_A(1,1,1)-U_A(0,1,1)$ is larger when moving from the low to the high inequality treatment (the difference is equal to $49\,(1-\beta_A)\geq 0$). Together with the assumption on the relationship between an agent's probability to choose 1 and the above differences, this concludes the proof for role-A agent.

Using similar computations in the case of a role-C agent, we derive that $U_C(0,0,1)-U_C(0,0,0)$ is the same in both treatments, so the difference is non negative. Second, $U_C(1,0,1)-U_C(1,0,0)$ is smaller when moving from the low to the high inequality treatment (the difference is equal to $-\alpha_C\frac{19}{2}\leq 0$). Third, $U_C(0,1,1)-U_C(0,1,0)$ is smaller when moving from the low to the high inequality treatment (the difference is equal to $-9\alpha_C+4\beta_C\geq 0$). Finally, $U_C(1,1,1)-U_C(1,1,0)$ is smaller when moving from the low to the high inequality treatment (the difference is equal to $-\alpha_C\frac{47}{2}\leq 0$). Together with the assumption on the relationship between an agent's probability to choose 1 and the above differences, this concludes the proof for role-C agent.

Again, using similar computations in the case of a role-B agent, we derive that $U_B(0,1,0)-U_B(0,0,0)$ is larger when moving from the low to the high inequality treatment (the difference is equal to 8). Second, $U_B(1,1,0)-U_B(1,0,0)$ is smaller when moving from the low to the high inequality treatment (the difference is equal to $-\alpha_B\frac{29}{2}\leq 0$). Third, depending on parameter values $U_B(0,1,1)-U_B(0,0,1)$ may be larger or smaller when moving from the low to the high inequality treatment: the difference is equal to $26-8\alpha_B+41\beta_B$ which may be negative if α_B is large enough. Finally, depending on parameter values $U_B(1,1,1)-U_B(1,0,1)$ may be larger or smaller when moving from the low to the high inequality treatment: the difference is equal to $26-\frac{3}{2}\alpha_B-13\beta_B$ which may be negative if α_B is large enough. Together with the assumption on the relationship between an agent's probability to choose 1 and the above differences, the overall conclusion is that role-B agent may be more or less likely to participate in the high inequality treatment than in the low inequality treatment.

B.3 Proof of Proposition 3

Using similar computations than in the proof of Proposition 2, we conclude easily that each agent's payoff differences are larger when moving from the low to the high inequality treatment. This concludes the proof.

C Additional Figure

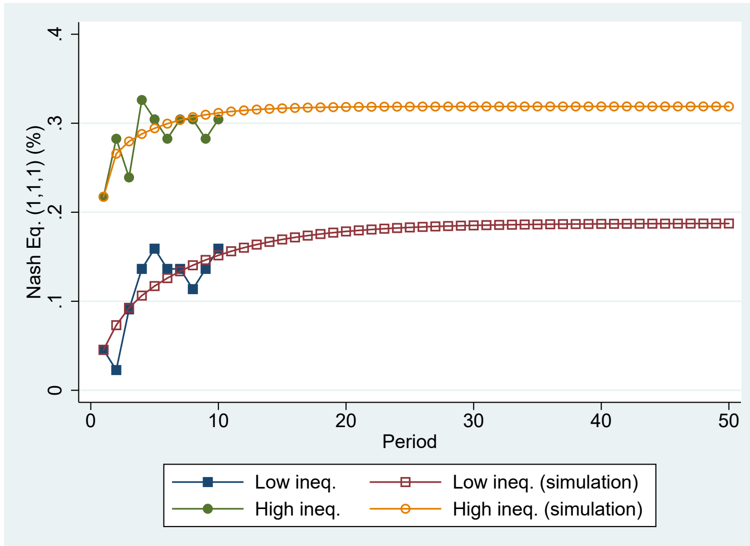

Figure 6: Simulations based on a Markov chain assumption (50 periods)

D Estimates of social preferences and direct tests of the effect of individual preferences

In this Appendix, we provide several direct tests of the effect of individual preferences. We first describe the procedure used to estimate the individual preference parameters. We then provide the regression results.

D.1 Estimates of social preferences

In order to estimate social preferences, we run a regression using the conditional logit model to estimate three variables: FSD which captures disadvantageous inequality aversion; FSA which captures advantageous inequality aversion; EFF which captures the importance put on the total payoff. We use the data obtained from our additional experimental modules: those from the modified dictator game, and those from the receiver decisions in the ultimatum game. In these experiments, the subjects choose between two different allocations. We use the data from the modified dictator game (see the description in Table 16) and the decisions of the receiver in the strategy method ultimatum game (see Game 4 point 2. in the Appendix). We do not use the decisions of the sender in the ultimatum game (see Game 4 point 1. in the Appendix), since these decisions depend on the anticipated choice of the receiver (which differs from the type of decisions presented in Moffatt, 2015). The results are provided in Table 10.

Table 10: Estimates of social preferences (Conditional logit)

	Dependent variable: Individual participation			
	Odds Ratio	Std. Err.	z	$P > \|z\|$
EFF	1.236***	0.011	23.89	0.000
FSA	0.989	0.009	-1.19	0.233
FSD	1.530***	0.035	18.82	0.000
Indiv. FE	Yes			
Obs.	11,880			
R^2	0.31			

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. Individual participation is a dummy variable equal to 1 if the subject chooses 1 and zero otherwise. FSD captures disadvantageous inequality aversion, FSA captures advantageous inequality aversion and EFF captures the importance put on the total payoff. Standard errors are clustered at the individual level.

The results from Table 10 suggest that the subjects are averse to disadvantageous inequality, and that they are motivated by the total payoffs. However, we find no evidence that the subjects are averse to advantageous inequality (the coefficient of FSA is negative and non significant). Notice however that, as in the reference you provided (as well as in Engelmann and Strobel 2004), one cannot estimate the "weight" the subjects award to their own payoffs when one uses the conditional logit model. More importantly, one has to be cautious with the interpretation of these results, since one cannot distinguish between the effect of the weight the subjects award to their own payoffs and to their social preferences.

D.2 Measures of individual preferences

D.2.1 Risk Aversion

In order to estimate individual risk aversion, we assume a constant relative risk aversion (CRRA) utility function, which enables us to compute the intervals corresponding to each choice proposed in Table 17. The CRRA utility function has the following form: $U(x) = x^{1-r_i}/(1-r_i)$, where x is the lottery prize and r_i, which denotes the constant relative risk aversion of the individual, is the parameter to be estimated. Expected utility is the probability weighted utility of each outcome in each row. An individual is indifferent between lottery A, with associated probability p of winning a and probability $1-p$ of winning b, and lottery B, with probability p of winning c and probability $1-p$ of winning d, if and only if the two expected utility levels are equal:

$$p.U(a) + (1-p).U(b) = p.U(c) + (1-p).U(d), \tag{4}$$

or,

$$p.\frac{a^{1-r_i}}{1-r_i} + (1-p).\frac{b^{1-r_i}}{1-r} = p.\frac{c^{1-r_i}}{1-r_i} + (1-p).\frac{d^{1-r_i}}{1-r_i} \tag{5}$$

which can be solved numerically in terms of r_i.

Our measure of individual risk aversion corresponds to the midpoint of the intervals.[38]

D.2.2 Inequality aversion

Since two-player games were used in the dictator and the ultimatum games, we assume Fehr and Schmidt (1999) type of utility functions in order to estimate individuals' inequality aversion parameters. This type of utility functions is defined as:

$$U_i(x_i, x_j) = x_i - \alpha_i \ max\{x_j - x_i; 0\} - \beta_i \ max\{x_i - x_j; 0\}, \tag{6}$$

where x_i and x_j, with $i \neq j$, are the monetary payoffs of i and j, respectively.

We compute α_i, which denotes i's individual parameter of aversion toward disadvantageous inequality, and β_i, which denotes i's individual parameter of aversion toward advantageous inequality aversion, by using respectively an ultimatum game and a modified dictator game. We follow Fehr and Schmidt (1999) and assume that subjects are harmed by increases in advantageous inequality, e.g. $\beta_i \geq 0$; they are also not willing to pay more than one unit for reduction of one unit in advantageous inequality, e.g. $\beta_i < 1$ is satisfied. Finally, we consider that subjects suffer more under disadvantageous inequality than under advantageous inequality, e.g. $\beta_i \leq \alpha_i$ is satisfied.

Advantageous inequality aversion: α_i

Regarding the strategy method we used in our ultimatum game (the game setting is described in Appendix E.2), we may identify the minimum acceptable offer for each individual. This offer can allow us to compute an estimation point of α_i. Let us consider that s'_i denotes the minimal offer that individual i is willing to accept. So individual i rejects offer $s'_i - 1$. He/she is then eager to accept a single offer $s_i \in [s'_i - 1, s'_i]$. Since individual i is indifferent when offered s_i, he gets a zero payoff when rejecting this offer. Thus, $U_i(s_i, d - s_i) = s_i - \alpha_i(d - s_i - s_i) = 0$, where d denotes the sender's endowment.[39] Therefore, α_i is given by:

$$\alpha_i = \frac{s_i}{2(\frac{d}{2} - s_i)}. \tag{7}$$

Our measure of s_i corresponds to a midpoint of the interval $[s'_i - 1, s'_i]$. For subjects with $s'_i = 0$, we set $\alpha_i = 0$. Also, for subjects that only accept offer $s'_i \geq \frac{d}{2}$ we follow Blanco et al. (2011) and set $\alpha_i = 4.5$. α_i thus lies in between 0 and 4.5, as we expect that a greater value of α_i (that is,

[38] We take the upper bound for the first interval and the lower bound for the last interval.

[39] d is arbitrarily set equal to 10 in our experiment.

individual i "hates" disadvantageous inequality) would not be much relevant for the purpose of this study.

Disadvantageous inequality aversion: β_i

Here, we use data from the modified dictator game played in strategy method (see Appendix E.1 and Table 16 in Appendix F) to compute the parameter β_i by looking for the distribution (x_i, x_i) which makes the dictator indifferent between keeping the entire endowment d (choose $(d, 0)$) or going for an equal split (x_i, x_i). Suppose that individual i switches toward the equal-share distribution at (x'_i, x'_i). Thus, we have $U_i(x'_i, x'_i) > U_i(d, 0) > U_i(x'_i - 1, x'_i - 1)$. Therefore, individual i is indifferent between $(d, 0)$ and (x''_i, x''_i) where $x''_i \in [x'_i - 1, x'_i]$ and $x'_i \in \{1, ..., d\}$. We now get $U_i(d, 0) = U_i(x''_i, x''_i)$. This is equivalent to $d - d\beta_i = x''_i$. This equation is solved in β_i such that,

$$\beta_i = 1 - \frac{x''_i}{d}. \tag{8}$$

We use the midpoint between $x'_i - 1$ and x'_i as a measure of x'' to compute β_i. For subjects who prefer $(0, 0)$ over $(d, 0)$, their β_i is greater than 1, and we set $\beta_i = 1$. Also, for those who choose $(d, 0)$ over (d, d) we set $\beta_i = 0$.

D.2.3 Altruism

We also define a proxy of altruism by using the modified dictator game. Since the mean spread is kept constant, we use question 6 in Table 16 to estimate the individuals' degree of altruism. More precisely, using question 6, we compute a dummy equal to 1 (altruist) if individual i selects the distribution $(\frac{d}{2}, \frac{d}{2})$ over $(d, 0)$. Otherwise, individual i is considered as non altruistic and we set the dummy equal to 0.

D.3 Descriptive statistics

Table 11 provides descriptive statistics about individual characteristics. Notice that instead of groups, we focus here on the subjects. *Risk aversion* is the individual relative risk aversion parameter computed following the procedure explained in Section D.2.1. *Disadv. ineq. aversion* and *Adv. ineq. aversion* are the inequality aversion parameters (disadvantageous and advantageous, respectively) computed following the procedure explained in Section D.2.2. *Altruist* is a dummy variable which is set to 1 if the individual is altruistic (see Section D.2.3).*Econ* is a dummy which is 1 if the student is majoring in economics or management (and 0 otherwise).

Our sample contains 270 subjects aged around 27, of which 49% of people are men and 51% are women. They are mainly risk-averse $\bar{r} = 0.49$. They also dislike disadvantageous inequality and advantageous inequality with mean coefficients corresponding to, $\bar{\alpha} = 1.65$ and $\bar{\beta} = 0.49$ respectively. Regarding our definition of altruism (see Section D.2.3), we observe that slightly more than a half of the population is altruistic (54%). 35% of the subjects in our sample are students majoring in economics or management.

Table 11: Descriptive statistics: individual characteristics

Variable	Mean	Std. Dev.	Min.	Max	Nb of subjects
Age	26.637	8.776	18	73	270
Gender (male=1)	0.492	0.5	0	1	266
Risk aversion	0.592	0.525	-.95	1.37	270
Disadv. ineq. aversion	1.647	1.822	0	4.5	270
Adv. ineq. aversion	0.493	0.304	0	1	270
Altruist	0.537	0.499	0	1	270
Econ	0.352	0.478	0	1	267
Not Univ.	0.052	0.222	0	1	270

Notes: The sample consists of 270 subjects, 90 groups playing 2 treatments with 10 repetitions each. *Risk aversion* is the individual relative risk aversion parameter. *Disadv. ineq. aversion* and *Adv. ineq. aversion* are the inequality aversion parameters (disadvantageous and advantageous, respectively). *Altruist* is a dummy variable which is set to 1 if the individual is altruistic.*Econ* is a dummy which is 1 if the subject is a student majoring in economics or management (and 0 otherwise). *Not Univ.* is a dummy which is 1 if the subject is from outside the University (and 0 otherwise).

D.4 Effect of individual preferences

In this section, we provide estimates of heterogeneous treatment effects at the subject level (Table 12), and correlations between individual characteristics and individual participation decisions (Table 13). The results are discussed in Section 4 in the body of the paper.

Table 12: Heterogeneous effect: individual characteristics

	Dependent variable: Individual participation							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
High ineq.	0.117***	0.094**	0.096**	0.095***	0.080**	0.163**	0.112***	0.100***
	(0.039)	(0.038)	(0.041)	(0.034)	(0.039)	(0.067)	(0.038)	(0.028)
High ineq. x Risk aversion	-0.030							
	(0.033)							
High ineq. x Disadv. ineq. aversion		0.003						
		(0.012)						
High ineq. x Adv. ineq. aversion			0.008					
			(0.066)					
High ineq. x Altruist				0.008				
				(0.043)				
High ineq. x Gender					0.039			
					(0.038)			
High ineq. x Age						-0.002		
						(0.003)		
High ineq. x Econ							-0.035	
							(0.040)	
High ineq. x Not Univ.								-0.018
								(0.082)
Model	LPM	LPM	LPM	LPM	LPM	LPM	LPM	LPM
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,400	5,400	5,400	5,400	5,320	5,400	5,340	5,400
R^2	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. Individual participation is a dummy variable equal to 1 if the subject chooses 1 and zero otherwise. High ineq. is a dummy variable equal to one if treatment is "High ineq." and zero if treatment is "Low ineq.". × indicates interaction variable. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level. *Risk aversion* is the individual relative risk aversion parameter. *Disadv. ineq. aversion* and *Adv. ineq. aversion* are the inequality aversion parameters (disadvantageous and advantageous, respectively). *Altruist* is a dummy variable which is set to 1 if the individual is altruistic.*Econ* is a dummy which is 1 if the subject is a student majoring in economics or management (and 0 otherwise). *Not Univ.* is a dummy which is 1 if the subject is from outside the University (and 0 otherwise).

E Additional experimental modules

In this part, we first present the modified dictator game that is quite specific especially in strategy method (Selten, 1967), which allows to get more information without lowering the size of the sample. Then, we describe the strategy method of the ultimatum game. We conclude by describing the commonly used Holt and Laury (2002) game.

E.1 Modified dictator game in strategy method

This modified dictator game is played in two sequences. In each sequence, subjects answer to a set of 11 questions. Each question corresponds to a binary choice between an egalitarian distribution

Table 13: Individual characteristics instead of indiv. fixed effects

	Dependent variable: Individual participation							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
High ineq.	0.099***	0.099***	0.099***	0.099***	0.099***	0.099***	0.099***	0.099***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)	(0.029)	(0.029)
Risk aversion		-0.092**	-0.092**	-0.092**	-0.092**	-0.102**	-0.101**	-0.092**
		(0.041)	(0.040)	(0.041)	(0.041)	(0.042)	(0.043)	(0.042)
Diadv. ineq. aversion			-0.010	-0.010	-0.010	-0.011	-0.011	-0.010
			(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Adv. ineq. aversion				0.006	-0.008	-0.038	-0.028	-0.045
				(0.076)	(0.122)	(0.125)	(0.125)	(0.123)
Altruist					0.011	0.029	0.031	0.038
					(0.067)	(0.069)	(0.069)	(0.068)
Age						-0.001	-0.000	-0.003
						(0.002)	(0.003)	(0.002)
Gender						-0.010	-0.005	0.009
						(0.053)	(0.054)	(0.054)
Econ							0.052	0.062
							(0.054)	(0.054)
Not Univ.								0.267**
								(0.104)
Model	LPM	LPM	LPM	LPM	LPM	LPM	LPM	LPM
Indiv. FE	No	No	No	No	No	No	No	No
Period. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,400	5,400	5,400	5,400	5,400	5,320	5,260	5,260
R^2	0.01	0.02	0.02	0.02	0.02	0.02	0.03	0.04

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. Individual participation is a dummy variable equal to 1 if the subject chooses 1 and zero otherwise. High ineq. is a dummy variable equal to one if treatment is "High ineq." and zero if treatment is "Low ineq.". × indicates interaction variable. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level. *Risk aversion* is the individual relative risk aversion parameter. *Disadv. ineq. aversion* and *Adv. ineq. aversion* are the inequality aversion parameters (disadvantageous and advantageous, respectively). *Altruist* is a dummy variable which is set to 1 if the individual is altruistic.*Econ* is a dummy which is 1 if the subject is a student majoring in economics or management (and 0 otherwise). *Not Univ.* is a dummy which is 1 if the subject is from outside the University (and 0 otherwise).

(s, s) and unequal distribution $(10, 0)$, with s an integer lying in $[0, 10]$. During the first sequence, all subjects are assigned the role of dictator and should choose only one distribution for each question. Once the sequence is completed, the second sequence starts. Subjects are randomly matched in groups of two members and receive information about their own role in their group. The subject roles in each group are different. Each subject could be either the dictator or the receiver. The group members payoffs depend on the choice of the dictator. Therefore, each receiver's outcome depends solely on his paired dictator.

E.2 Ultimatum game in strategy method

The ultimatum game module is conducted in three sequences. The first sequence relates to the senders' choices. In fact, each subject is first assigned the role of sender and receives a monetary endowment of 10 experimental units (ECUs). Then he/she chooses an amount s he/she wants to offer to his/her partner, thus keeping $10 - s$ units, with s an integer lying in $[0, 10]$. Once the first sequence is completed, subjects move on to the sequence on respondents' choices. In this sequence, each subject decides which distributions out of the 11 offers they are willing to accept or reject. Finally, the last sequence goes as follows. Subjects are randomly matched into pairs composed by a single proposer and a single respondent. Each proposer offer is matched with his/her paired respondent choice. The payment is then computed as follows: If the proposer offered s units in the first sequence and the respondent chose to accept this offer in the second sequence then, in the last sequence, the proposer receives $(10 - s)$ units and the respondent receives s units. Alternatively, if the respondent chose to reject this offer during the second sequence, they both receive 0 unit.

E.3 Multiple price list risk elicitation

In order to elicit individual's risk preferences, we use the well known Holt and Laury (2002) lottery game. In this game, subjects face a list of 10 questions involving paired gambles as presented in Table 17 in Appendix F. For each question, the two gambles are labeled option A and option B. For each question, each subject chooses which gamble he/she prefers to take: either option A or option B. The resulting payoffs in option A and option B are constant, only the probability associated with each payoff varies between questions. A risk-seeking subject would choose option B in the first question. On the other side, if a subject understands the instructions well, he/she should choose option B when dealing with question 10. So, if a subject understands the instructions well and is not a strong risk-seeker, then we expect he/she starts choosing option A then switches and chooses option B at some point. A subject's switching point is used to measure this subject's risk preference.

F Instructions

"*As we ran experiments on French population, all instructions provided here are translated from French.* "

You are about to participate in a decision-making experiment. We ask you to read the instructions carefully, they will allow you to understand the experiment. When all participants have read these instructions, an experimenter will perform a read-aloud. All your decisions will be handled anonymously. You will only use the computer in front of which you are sitting for entering your decisions.

From now on, we ask you to stop talking. If you have a question, please raise your hand, and an experimenter will come to you to answer it.

This experiment consists in a series of 5 games. You will receive instructions for a game at the end of the previous game. Your payments for each game are either in experimental currency (ECU) or in euros. If the gain is in ECUs the conversion rate will be specified at the end of the instructions of Game 5.

At the end of this experiment one of the first two games and one of the last three games will be drawn randomly, and the sum of your payments for each game that has been drawn will constitute your earning for the experiment. Your earning in euros will be paid in cash at the end of the experiment.

Game 1[40]

At the beginning of the game, the server will randomly create groups of 3 members. You cannot identify other members of your group, and they cannot identify you. In each group there are three roles: player A, player B and player C. Each member of the group will have a randomly assigned role. Groups, as well as roles within each group, will remain unchanged throughout the game. You will be informed about your role at the beginning of the game.

The game has 10 consecutive periods. For each period, there are two stages:

1. Each member of the group chooses between the option "0" and the option "1" .
2. When all participants have made their choice, a screen is displayed. This screen provides you with information on the choices of the other members of your group and on your earning for the period.

The payment of each player depends on his/her role (player A, player B or player C), on his/her choice, and on the choices made by other members of his/her group. Table 14 provides the payments of each player, according to his/her role and the combination of option 0 or option 1 chosen in the group. since there are three players in the group who each chooses either option 0 or option 1, we obtain a total of 8 possible combinations. Each row in this table corresponds to a given combination, which is provided in the second column. For example, line 4 states that players A and B have chosen option 0, while player C has chosen option 1. For this combination of choices, the gain of players A and B is 60 ECUs each, and the gain of player C is 10 ECUs.

Payment

The payment for this game is equal to the sum of the payment in the first period and of the payment corresponding to one period that is randomly drawn among the other 9 periods.

[40]This game setting corresponds to the high inequality payoff treatment.

Table 14

	Combinations (A, B, C)	Payoffs *A*	*B*	*C*
1	(0, 0, 0)	60	60	60
2	(1, 0, 0)	60	60	60
3	(0, 1, 0)	60	29	60
4	(0, 0, 1)	60	60	10
5	(1, 1, 0)	90	60	60
6	(1, 0, 1)	81	60	32
7	(0, 1, 1)	60	56	38
8	(1, 1, 1)	111	87	60

Game 2[41]

The groups and the roles remain the same as in Game 1: you still belong to the same group, and hold the same role as in Game 1. As before, this game has 10 consecutive periods, and in each period you must choose between option "0" and option "1". For each period there are two stages:

1. Each member of the group makes a choice between option 0 and option 1.
2. When all participants have made their choice, a screen is displayed. This screen provides you with information on the choices of the other members of your group, and on your earning for the period.

The payment of each player depends on his/her role (player A, player B or player C), on his/her choice, and on the choices made by other members of his/her group. Table 15 provides the gain of each player, according to his/her role and the combination of option 0 or option 1 chosen in the group. Since there are three players in the group who each chooses either option 0 or option 1, we obtain a total of 8 possible combinations. Each row in the table corresponds to a given combination, which is provided in the second column. For example, line 7 states that players B and C have chosen option 1, while player A has chosen option 0. For this combination of choices, the gain of player A is 60 ECUs, the gain of Player B is 30 ECUs, and the gain of Player C is 38 ECUs.

Payment

The payment for this game is equal to the sum of the gain in the first period and of the gain corresponding to one period that is randomly drawn among the remaining 9 periods.

Game 3

At the beginning of the game, the server will randomly create pairs (groups of 2 members). You cannot identify the other member of your pair, and he/she cannot identify you. In each pair, one member is assigned the role of player E and the other is assigned the role of player R. You do not know whether you are player E or player R.

[41]This game setting corresponds to the low inequality payoff treatment.

Table 15

	Combinations	Payoffs		
	(A, B, C)	A	B	C
1	(0, 0, 0)	60	60	60
2	(1, 0, 0)	60	60	60
3	(0, 1, 0)	60	29	60
4	(0, 0, 1)	60	60	10
5	(1, 1, 0)	61	60	60
6	(1, 0, 1)	61	60	32
7	(0, 1, 1)	60	30	38
8	(1, 1, 1)	62	61	60

There are 11 questions in the game. For each question you must choose between option X and option Y. Each option corresponds to a payoff split between you and the other member of your pair.

There are two stages in the game:

1. Each member responds individually to each of the 11 questions provided in table 16 which describes options X and Y for each question in the game.
2. The server reveals whether you are player E or player R. In each pair, the gain of each player will depend on the choices made by player E only.

Payment

At the end of the game, one question will be randomly drawn among the 11 questions. Your gain for this question will constitute your payment for the game.

Example:

At the end of the game, question 3 is drawn randomly.

If you are player E and you have chosen option X for this question, then your gain is 2 ECUs and the gain of the other member of your pair (player R) is 2 ECUs too. If you have chosen option Y your gain is 10 ECUs, and the gain of the other member of your pair is 0 ECU .

If you are player R and the other member of your pair (player E) has chosen X, then your gain is 2 ECUs, and if he has chosen Y then your gain is 0 ECU.

Game 4

In this game, there are two roles: player E and player R.

Player E has an endowment of 10 ECUs, which he/she must distribute between himself/herself and player R. Player R must then decide whether he/she accepts or rejects the distribution chosen by player E. If he/she accepts, the distribution is implemented and it determines the earning of each player. If he/she rejects, then each of the two players gains 0 ECU.

Table 16

Questions	Options	Your choice	
1	Option X: You earn 0 and your paired partner earns 0	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
2	Option X: You earn 1 and your paired partner earns 1	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
3	Option X: You earn 2 and your paired partner earns 2	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
4	Option X: You earn 3 and your paired partner earns 3	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
5	Option X: You earn 4 and your paired partner earns 4	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
6	Option X: You earn 5 and your paired partner earns 5	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
7	Option X: You earn 6 and your paired partner earns 6	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
8	Option X: You earn 7 and your paired partner earns 7	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
9	Option X: You earn 8 and your paired partner earns 8	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
10	Option X: You earn 9 and your paired partner earns 9	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O
11	Option X: You earn 10 and your paired partner earns 10	Option X	O
	Option Y: You earn 10 and your paired partner earns 0	Option Y	O

The game takes place in 3 stages:

1. Each participant is assigned the role of player E and must choose a distribution of the 10 ECUs.
2. Each participant is assigned the role of player R and must decide, for each of the 11 possible distributions ([10, 0], [9, 1], [8, 2] … [1, 9], [0, 10]), whether he/she accepts or rejects the distribution.
3. The server randomly forms pairs of participants, and for each pair the server randomly assigns the roles of player E and of player R. A screen will provide information on your role.

Payment

Your payment will depend on your decisions and on the decisions made by the other member of your pair.

If you are assigned the role of player E, your payment depends on whether player R accepts or rejects your distribution choice. If player R has accepted the distribution you have chosen, then this distribution is implemented. If player R has rejected it, each member in the pair earns 0 ECU.

If you are assigned the role of player R, your payment depends on your decision to accept or reject the distribution chosen by player E. If you have accepted the distribution chosen by player E, then this distribution is implemented. If you have rejected this distribution, each member in the pair earns 0 ECU.

Example 1

You are player E. In stage 1 you chose to keep 7 ECUs and to offer 3 ECUs to player R.

If player R has decided to accept this distribution, then this distribution is implemented, you earn 7 ECUs and player R earns 3 ECUs.

If player R has rejected this distribution, then each member in the pair earns 0 ECU.

Example 2

You are player R. In stage 1 player E in your pair has chosen to keep 7 ECUs and to offer 3 ECUs.

If at stage 2 you decided to accept this distribution, then this distribution is implemented, you earn 3 ECUs and player E earns 7 ECUs.

If you rejected it, then each member in the pair earns 0 ECU.

Game 5

In this game, your payments depend solely on your individual choices.

There are 10 questions in the game. For each question you must choose one of the two options: option A or option B. Options are shown in Table 17 below. Payments are in euros.

Table 17

Questions	Options	Your choices	
1	Option A: 1 chance out of 10 to earn 2,00€ and 9 chance out of 10 to earn 1,60€	Option A	O
	Option B: 1 chance out of 10 to earn 3,85€ and 9 chance out of 10 to earn 0,10€	Option B	O
2	Option A: 2 chance out of 10 to earn 2,00€ and 8 chance out of 10 to earn 1,60€	Option A	O
	Option B: 2 chance out of 10 to earn 3,85€ and 8 chance out of 10 to earn 0,10€	Option B	O
3	Option A: 3 chance out of 10 to earn 2,00€ and 7 chance out of 10 to earn 1,60€	Option A	O
	Option B: 3 chance out of 10 to earn 3,85€ and 7 chance out of 10 to earn 0,10€	Option B	O
4	Option A: 4 chance out of 10 to earn 2,00€ and 6 chance out of 10 to earn 1,60€	Option A	O
	Option B: 4 chance out of 10 to earn 3,85€ and 6 chance out of 10 to earn 0,10€	Option B	O
5	Option A: 5 chance out of 10 to earn 2,00€ and 5 chance out of 10 to earn 1,60€	Option A	O
	Option B: 5 chance out of 10 to earn 3,85€ and 5 chance out of 10 to earn 0,10€	Option B	O
6	Option A: 6 chance out of 10 to earn 2,00€ and 4 chance out of 10 to earn 1,60€	Option A	O
	Option B: 6 chance out of 10 to earn 3,85€ and 4 chance out of 10 to earn 0,10€	Option B	O
7	Option A: 7 chance out of 10 to earn 2,00€ and 7 chance out of 10 to earn 1,60€	Option A	O
	Option B: 7 chance out of 10 to earn 3,85€ and 3 chance out of 10 to earn 0,10€	Option B	O
8	Option A: 8 chance out of 10 to earn 2,00€ and 2 chance out of 10 to earn 1,60€	Option A	O
	Option B: 8 chance out of 10 to earn 3,85€ and 2 chance out of 10 to earn 0,10€	Option B	O
9	Option A: 9 chance out of 10 to earn 2,00€ and 1 chance out of 10 to earn 1,60€	Option A	O
	Option B: 9 chance out of 10 to earn 3,85€ and 1 chance out of 10 to earn 0,10€	Option B	O
10	Option A: 10 chance out of 10 to earn 2,00€ and 0 chance out of 10 to earn 1,60€	Option A	O
	Option B: 10 chance out of 10 to earn 3,85€ and 0 chance out of 10 to earn 0,10€	Option B	O

Payment

One of the 10 questions will be randomly drawn. A second draw will determine your payment based on the option (A or B) that you have chosen for the question that has been randomly drawn.

Example

Question 3 is randomly drawn.

If you have chosen option A in question 3 then a second draw determines if you earn 2.00 € or 1.60 €. Specifically, the server randomly draws a number between 1 and 10. If this number is 1, 2 or 3 then you earn 2.00 € and if this number is 4, 5, 6, 7, 8, 9 or 10 then you earn 1.60 €.

If you have chosen option B in question 3 then a second draw determines if you earn 3.85 € or 0.10 €. Specifically, the server randomly draws a number between 1 and 10. If this number is 1, 2 or 3 then you earn 3.85 € and if this number is 4, 5, 6, 7, 8, 9 or 10 then you earn 0,10 €.

Conversion rate: 1 ECU = 0.09 €/ 1 €= 11.11 ECUs

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