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Abstract

In this paper, we use a laboratory experiment to analyze the relationship between equity and coordination success in a game with Pareto ranked equilibria. Equity is decreased by increasing the coordination payoffs of some subjects while the coordination payoffs of others remain unchanged. Theoretically, in this setting, difference aversion may lead to a positive relationship between equity and coordination success, while social welfare motivations may lead to a negative relationship. Using a within-subject experimental design, we find that less equity unambiguously leads to a higher level of coordination success. Moreover, this result holds even for subjects whose payoffs remain unchanged. Our results suggest that social welfare motivations drives the negative relationship between equity and coordination success found in this experiment. Moreover, our data suggest 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.

Keywords: Coordination game, equity, efficiency, difference aversion, social welfare motivation.

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1 Introduction

Coordination between economic agents in markets, contracts, firms, governments and organizations is a necessary condition to reach efficiency in most economic activities. At the same time, equity often plays a crucial role in the acceptability of economic decisions, raising the issue of the tension between efficiency and equity considerations.

To improve our understanding of the importance of the equity efficiency trade-off, it is thus crucial to investigate how it comes forward in coordination problems. In other words, does equity affect efficiency through its influence on the agents' ability to coordinate? Does the prospect of unevenly distributed larger coordination gains decrease or increase the frequency 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 the link between equity and efficiency using a laboratory experimental coordination game with Pareto ranked equilibria. Equity is decreased by increasing the coordination payoffs of some subjects while the coordination payoffs of others remain unchanged. Theoretically, in this setting, difference aversion (see Fehr and Schmidt 1999) may lead to a positive relationship between equity and coordination success, while social welfare motivations may lead to a negative relationship. Using a within-subject experimental design, we find that less equity 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 decrease in equity. These results suggest that social welfare motivations drives the negative relationship between equity and coordination success. Moreover, our data suggest 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.

The literature on the effect of payoff asymmetry on coordination success in Pareto ranked coordination games is relatively scarce.[1] A strand of the literature focuses on the relationship between payoffs heterogeneity and coordination in Battle of the sexes experimental games, i.e coordination games with no Pareto dominant equilibrium. Crawford et al. (2008) show that introducing a small degree of heterogeneity in a symmetric Battle of the sexes game has a negative effect on coordination. However, this pattern reverses when payoff asymmetry becomes sufficiently large.[2] 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 abstract from the effect of a change in payoff heterogeneity.[3] As in the present paper, Chmura et al. (2005) analyze coordination games with a Pareto dominant equilibrium and focus on variations in the subjects' payoffs (at this equilibrium).[4] They argue that the existence of beliefs about other subjects' difference aversion is consistent with the observed subjects' behaviors. Their results are however difficult to interpret since they use a between setting with a relatively small number of subjects per treatment[5] 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 setting in order to control for subjects and group characteristics that may influence subjects' play, and we show that subjects' behaviors are consistent with social welfare motivations and not with difference aversion.[6]

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). The game admits multiple Nash equilibria (thus raising coor-

[1]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).

[2]Parravano and Poulsen (2015) analyze the role of stake size on coordination frequency on the label salient strategy in symmetric and asymmetric coordination games with no Pareto dominant equilibrium.

[3]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.

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

[5]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.

[6]This is not to say that subjects are not averse to differences. However, our results suggest that the effect of difference aversion preferences is weaker than the effect of social welfare preferences.

dination 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 heterogeneous. 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 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 (10 rounds) plays the two treatments (in different orders).[7]

Our first main result is that groups reached the 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 and time. These two results suggest that subjects have social preferences consistent with social welfare motivations rather than with difference aversion. A third important result is that groups that first play the treatment with high inequality in coordination payoffs coordinate on the efficient outcome more frequently. This suggests that the equity efficiency trade-off is affected differently depending on whether coordination gains decrease or increase.

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. Section 5 concludes.

[7]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.

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 now introduce the games that are used in the different treatments of the experiment. 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 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.[8] Each agent's decision is binary: choose 0 or choose 1.[9] All agents decide simultaneously. We consider two cases, one where there is a high degree of inequality in payoffs, which corresponds to Table 1, and one where there is a low degree of inequality in payoffs, which corresponds to Table 2. In Appendix A we explain how the two tables are obtained by relying on the setting introduced in Bernstein and Winter (2012). This game structure is such that (i) the efficient outcome is always part of the equilibrium set (ii) the benefits from coordination increase from one case

[8] We consider groups of three agents when designing the experiment, as there was an initial risk that players assigned role C may consistently choose to not participate.

[9] 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.

to the other (iii) the coordination payoffs of some agents increase (namely, the agents who are assigned roles A and B), while the coordination payoff of others is unaffected (namely, the agents who are assigned role C).

Table 1: Payoff matrix faced by each individual subject

	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, instance means that (1, 1, 0) combination is reached, thus A gets 90, B gets 60 and C gets 60.

Table 2: Payoff matrix faced by each subject

	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, instance means that (1, 1, 0) combination is reached, thus A gets 61, B gets 60 and C gets 60.

An important feature is 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.

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. For instance, in the two treatments, if player C decides to play 0, she gets 60. If she decides to play 1, she gets 60 if both subjects A and B also play 1, but she only gets 38 if subject A plays 0 instead of 1 and she only gets 32 if subject B plays 0 instead of 1. Choosing 0 is as such a weakly dominant strategy for player C. Second, subjects may have social preferences. We follow Charness and Rabin (2002) who argue that the two main broad categories of social preferences models are "difference aversion" and "social welfare".

We now develop three sets of predictions based on these alternative assumptions. 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 payoff differences. We finally provide predictions assuming that subjects are motivated by the possibility to increase social welfare.

A1: *Standard preferences:* Assume that the subjects have standard preferences and that they take strategic risk into account. In this case, one can make a clear comparison of the two treatments for player B. In Table 1, if subject B plays 0 she gets 60. If she plays 1 she gets 87 if both subject A and C also play 1, but she only gets 29 if both subject A and C play 0 and only 56 if subject A plays 0. In Table 2, if subject B plays 0 she gets 60. If she plays 1 she gets 61 if both subject A and C also play 1, but she only gets 29 if both subject A and C play 0 and only 30 if subject A plays 0. Also, the payoffs of player C are unchanged between the two treatments and player A's dominant strategy is always to play 1 and she gets a larger coordination payoff in the “High ineq.” treatment. This suggests that player B is less exposed to strategic risk in the “High ineq.” treatment. However, players A and C have dominant strategies and thus, strategic uncertainty should not affect their decisions differently in the two treatments (that is, subject A should always choose 1, while subject C should always choose 0). We can then make the following prediction:

- Subject B participates more frequently in the “High ineq.” treatment.
- Participation of subject A and of subject C is equally frequent in the “High ineq.” treatment and in the “Low ineq.” treatment.

A2: *Difference aversion:* Now, assume that all the players have some aversion to differences between subjects’ payoffs. One can make the following predictions:

- All subjects participate less frequently in the “High ineq.” treatment. Indeed, payoff differences are larger at the efficient outcome in the “High ineq.” treatment than in the “Low ineq.” treatment, which may induce a lower participation rate if subjects exhibit some form of difference aversion.
- If one consider *Disadvantageous inequality aversion*, subjects B and C participate less frequently in the “High ineq.” treatment, as these subjects receive (much) lower payoffs than subject A in this treatment.
- If one consider *Advantageous inequality aversion*, subjects A and B participate less frequently in the “High ineq.” treatment, as they receive (much) higher payoffs than subject C in this treatment.

A3: *Social welfare motivation:* Last, assume that the subjects put some weight on social welfare. Since the payoffs subjects A and B are strictly larger at the efficient outcome in the “High ineq.” treatment and the payoff of subject C is unchanged, we can predict the following:

- All the subjects participate more frequently in the “High ineq.” treatment.

These three sets of predictions will allow us to discriminate between these three kinds of social preferences (none, difference aversion and social welfare 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 et al., 2004) 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.[10] Each session lasted about one hour.

Upon arrival in the experimental room, each subject were asked to seat in a personal box, in which they seated in front of a computer on a desk. Instructions (see Appendix D) were circulated and loudly read 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 in 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)

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

and that their gains will be converted into euros using the exchange rate of 1 € $\simeq$ 11 ECUs.[11]

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 one and two, respectively. Block one 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 two refers to the three additional modules.

Table 3: Orders in the experiments

	Block 1		Block 2			Survey
Order 1 (Decreasing ineq.)	*High ineq.*	*Low ineq.*	*MD*	*Ult*	*HL*	yes
Order 2 (Increasing 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,[12] that may bias subjects' decisions.

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, i.e the three additional modules. Subjects first played a modified dictator game (Blanco et al., 2011). Then subjects played an ultimatum game (Güth et al., 1982; Camerer and Thaler, 1995; Thaler, 1988). Finally, they played

[11]Using ECU allows to provide simple forms of payoffs (avoiding decimal numbers).
[12]See Kahneman and Tversky (1979) or Druckman (2001) for more details on this topic.

a multiple price-list lottery game (Holt and Laury, 2002). The dictator game allows us to estimate individuals' degrees of aversion toward advantageous inequality (Fehr and Schmidt, 1999) as well as a proxy for subjects' altruism. The ultimatum game allows us to estimate subjects' degrees of aversion toward disadvantageous inequality (Fehr and Schmidt, 1999), and the multiple price-list lottery game allows us to estimate a measure of their risk aversion. Further details concerning these modules and the estimates of subjects' preferences can be found in Appendix B.

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 B (Table 8).

Before going further, let us discuss two important choices we made in this experiment.

First, we use a within setting for analysis purposes (each group plays the two treatments). A within setting 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 and by the same vein the precision of the statistical tests (e.g. see Charness et al. 2013). However, we have to deal with the possibility that order effects are present,[13] 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. In practice, group's receives a randomly given order of the treatments before each session. In this experiment, since we had only two treatments, counterbalancing was quite simple. Block 1 in Table 3 is build to counterbalance the orders: each group either began by playing the "High ineq." treatment and then played the "Low ineq." treatment, which we refer to as the decreasing inequality order (Order 1) or it began by playing the "Low ineq." treatment and then played the "High ineq." treatment, which we refer to as the increasing inequality order (Order 2).

Second, we use a partner setting. Indeed, groups were formed and roles were assigned at

[13]See Schuman et al. (1981) for further details about order-effects in experiments.

the beginning of the experiment and they remained unchanged during all the experiment. 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, we employ the following straightforward method. The total payoff of a subject (for Block 1) was computed as follows: select one of the two treatment randomly and take the sum of the payoff of the first period plus the payoff of one randomly selected period out of the nine remaining periods. Thus, 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 period 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).[14] 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 basic 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. Subjects chose to participate 52.3% of the time. Groups reached the efficient outcome (i.e. they played (1, 1, 1)) 20.5% of the time. The outcome corresponds to another Nash equilibria 42% of the times. 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)) 14.5% of the times, and the Nash equilibrium

[14] See also Heinemann (2008) for more details on how to measure wealth effects.

in which subjects A and B participate but not C (i.e. they played (1,1,0)) 17% of the time.

Table 4: Descriptive statistics: outcome

Variable	Mean	Std. Dev.	Min.	Max.	N
Indiv. participation	0.523	0.500	0	1	5400
Group coord. (1, 1, 1)	0.205	0.404	0	1	1800
Nash Eq. (0, 0, 0)	0.101	0.302	0	1	1800
Nash Eq. (1, 0, 0)	0.145	0.165	0	0.333	1800
Nash Eq. (1, 1, 0)	0.173	0.292	0	0.667	1800

Notes: The sample consists of 270 subjects, 90 groups who played 2 treatments with 10 repetition each.

Individual characteristics from the survey (age, gender) and estimated thanks to the three modules (risk aversion, inequality aversion and altruism) are presented in Appendix B (Table 8).

4.2 The effect of inequality on coordination

In this section, we present a set of results on the relationship between the level of inequality and the frequency of coordination success.

We first compare the frequency of each outcome in each treatment. Table 5 provides our results. The first row provides the frequency of individual participation in the two treatments. Subjects chose to participate (i.e. they played 1) 47% of the time in the Low inequality treatment and 57% of the time in the High inequality treatment. The other rows provide the frequency of occurrence of the various Nash equilibria in the two treatments. The Table also reports the results of Wilcoxon signed-rank tests of equality of the distributions of the frequency of occurrence of each Nash equilibrium at the group level in each treatment (we report the z-score and the p-value in brackets). Groups achieved coordination on the Pareto dominant equilibrium 16% of the time in the Low inequality treatment and 25% of the time in the High inequality treatment. Maybe less surprisingly, the Nash equilibrium in which subject A and B participate and subject C does not was significantly more frequent in the High inequality treatment than in the Low inequality treatment (16% in the Low inequality treatment versus 19% in the High inequality treatment), the Nash equilibrium in which subject A is the only one to participate was significantly more frequent in the Low inequality treatment than in the High inequality treatment (16% versus 13%), and the Nash equilibrium in which none of the subjects participate was more frequent in the Low inequality treatment (13%) than in the High inequality treatment (8%). The largest difference between the two treatments seems to be for the equilibrium in which all the subjects participate. This result suggests that coordination on the Pareto dominant equilibrium is facilitated in the High inequality treatment.

This first result is confirmed by Figure 1. We plot a time series of the frequency of coordination on the Pareto dominant equilibrium for each treatment and each of the 10 periods. The graph confirms that the frequency of coordination 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 9.7% and the standard deviation of the difference is small (0.013). The difference between the treatments in the first period - for which the subject knew they will get payments for sure - is 10%, which is not much larger than the average.

Table 5: Treatment comparison: outcome

	Low ineq.		**High ineq.**			
Variables	**Mean**	**Nb units**	**Mean**	**Nb units**	z	[p-value]
Indiv. participation	0.473	270	0.572	270		
Group coord.	0.157	90	0.253	90	-2.037**	[0.042]
Nash Eq. (1, 1, 0)	0.155	90	0.191	90	-1.880*	[0.060]
Nash Eq. (1, 0, 0)	0.162	90	0.128	90	1.982**	[0.047]
Nash Eq. (0, 0, 0)	0.126	90	0.077	90	1.798*	[0.072]

Notes: z is the z-score of a Wilcoxon signed-rank test of equality of the distributions. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Panel regressions estimates of a linear probability model that links the treatments and group coordination success also confirm this result. The analysis is performed at the group-period level. The right hand side variable is High ineq., a dummy which is 1 if the group plays the High inequality game and 0 if the group plays the Low inequality game in the current period. The outcome variable is a dummy variable which is 1 if the group achieves

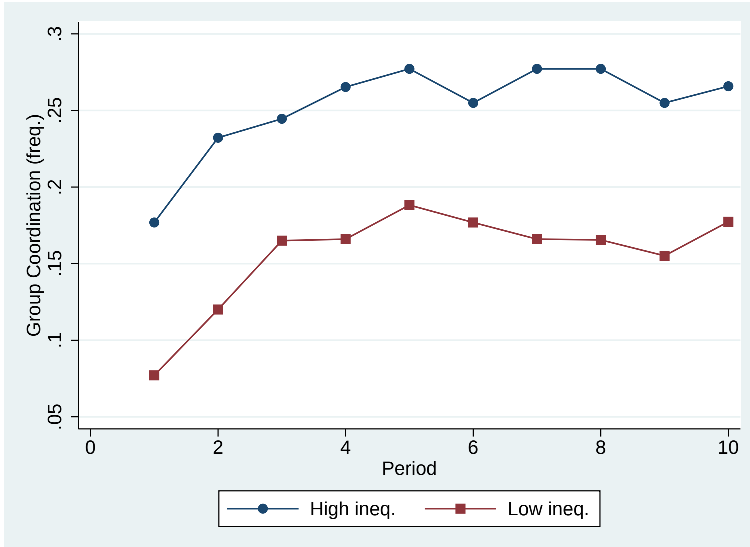

Figure 1: Treatment comparison by period

coordination on the Pareto dominant 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 regression, 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 likelihood that a group achieves coordination is significantly higher in the High inequality treatment. The increase is as high as 9.7% compared to the Low inequality treatment. In column (2) we only include the first periods and the result is very similar.

The results of the present section provide important information as regards the motivations that drive the subjects' behavior. These results are not consistent with Difference aversion models (see prediction A2 in Section 2). Indeed, if the subjects are averse to differences, the likelihood that they coordinate on the Pareto dominant equilibrium should be larger in the Low inequality treatment than in the High inequality treatment. However, the results of the present section are not inconsistent with standard preferences models (if subjects consider strategic uncertainty). Indeed, standard preferences models predict that subject B is more likely to participate in the High inequality treatment, while subjects A and C are equally likely to participate in the two treatments (see prediction A1 in Section 2). The results are also not inconsistent with social welfare motivation models, since these models predict that all the players are more likely to participate in the High inequality treatment (see prediction A3 in Section 2).

In order to discriminate between standard preferences models and social welfare motivation models, we go one step further in the next section.

Table 6: Inequality and group coordination (fixed effects)

	Dependent variable: Group Coordination	
	(1) All periods	(2) First periods only
High ineq.	0.0969***	0.0998**
	(0.035)	(0.039)
Model	LPM	LPM
Group FE	YES	YES
Period FE	YES	YES
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. High ineq. is the outcome of the dummy for treatment when it is equal to 1. Coordination equals 1 when groups select the most efficient Nash equilibria. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level in order to adjust standard errors for serial correlation.

4.3 Evidence on the role of social preferences

In this section, we deepen our analysis and focus on individual participation decisions. 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 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 regression, 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 column (1), we find that a subject is 9.9% more likely to participate 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 subjects with role A, B and C are all more likely to participate in the High inequality treatment.[15]

The results obtained in this section are striking. 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 A and C are more likely to participate under the High inequality treatment is not consistent with standard preferences models (see prediction A1 in Section 2). This is however consistent with social welfare motivation models that predict the qualitative results of the present section.

We cannot rule out that other models could explain these results. We can, however, argue that it is difficult to find other models that predict that all the subjects are more likely to participate in the High inequality treatment.

One may find alternative explanations for the case of subjects with role A. 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.

An alternative strategy to test the effect of individual preferences on individual decision

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

Table 7: Inequality and individual decision (fixed effects)

	Dependent variable: Participation decision	
	(1)	(2)
High ineq.	0.099***	
	(0.028)	
High ineq. × subject A		0.052**
		(0.024)
High ineq. × subject B		0.147***
		(0.046)
High ineq. × subject C		0.099***
		(0.036)
Model	LPM	LPM
Indiv. FE	Yes	Yes
Period FE	Yes	Yes
Obs.	5,400	5,400
Nb of subjects	270	270
R^2	0.59	0.59

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. High ineq. is the outcome of the dummy for treatment when it is equal to 1. Participation decision equals 1 when subjects select "participation". × indicates interaction variable. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level.

is to 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, altruist or not, more or less risk averse. However, we do not find evidence of such heterogeneous treatment effect. We relegate the description of this analysis to Appendix B. In order to test whether individual preferences played a role in the decisions of the subjects, we also estimated the same models as in Table 7 without individual fixed effects in order to include individual preferences measures (see Table 10 in Appendix B). 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.

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 frequency of coordination depending on the order of the treatments.

We first plot time series of the frequency of group coordination for each order in Figure 2. Groups that played the High inequality treatment then the Low inequality treatment (order 1) reached the Pareto dominant equilibrium 28.5% of the time in the High inequality treatment and 19.8% of the time in the Low inequality treatment. Groups that played the Low inequality treatment and then the High inequality treatment (order 2) reached the Pareto dominant equilibrium less often in each treatment. They reached it 11.4% of the time in the Low inequality treatment and 22% of the time in the High inequality treatment.

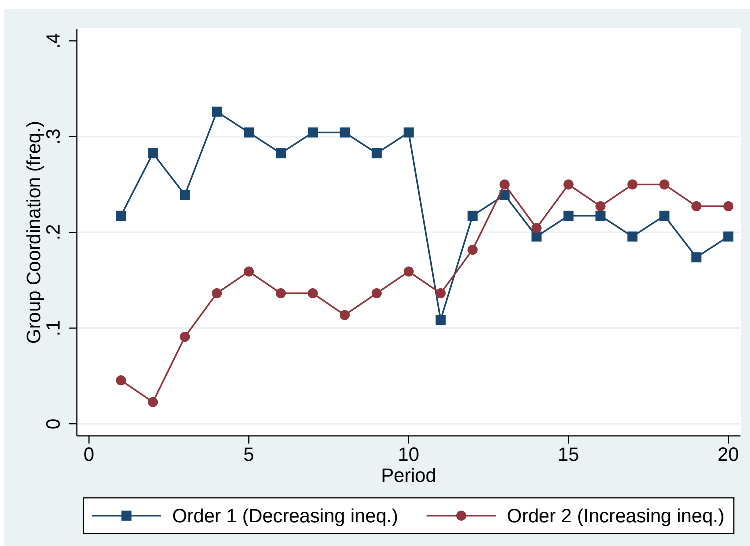

Figure 2: Order comparison (between sessions)

To make this comparison more salient, we plot one graph for each treatment in Figure 3. The left hand side plot represents the average frequency of coordination by period in the Low inequality treatment for each order. The right hand side plot represents the average frequency of coordination by period in the High inequality treatment for each order. For almost all period, the average frequency of coordination is larger for decreasing inequality (order 1) than for the increasing inequality order (order 2).

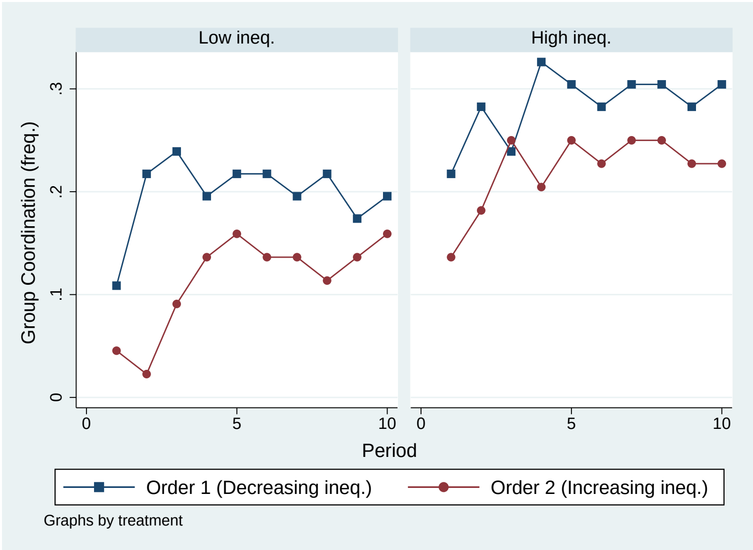

Figure 3: Order comparison by treatment

These results suggest that the sequence of treatments matters. Groups facing first the High inequality treatment then the Low inequality treatment reach the efficient outcome more often in *both* treatments compared to the groups facing first the Low inequality treatment then the High inequality treatment.

5 Conclusion

Coordination is often required to reach an efficient outcome, and whether equity concerns facilitate efficient coordination or make it more difficult is a question that has surprisingly 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 they play the strategy that corresponds to this outcome more frequently even when their individual payoffs are unchanged. This suggests that subjects are motivated by social welfare rather than by difference aversion considerations. We control for order effects that seem to exist in this setting. Thus, decreasing inequality and the coordination payoffs of some of the subjects (in other words, facing the High inequality treatment first) facilitates coordination compared to increasing inequality and the coordination payoffs of these subjects (facing the Low inequality treatment first).

Our results suggest that larger levels of welfare for some but not all increases coordination success while increasing the levels of welfare of some but not all through time decreases coordination success.

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Appendices

A Specification of the payoff structures

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 to not participate then he gets a payoff of c, which corresponds to the outside option.

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.[16] Specifically, this incentive structure is given by $(v_A, v_B, v_C) = (c, \quad c - w_B(A), \quad c - w_C(A) - w_C(B)) = (60, 29, 10)$. 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 $(c + w_A(B) + w_A(C), c - w_B(A) + w_B(A) + w_B(C), c - w_C(A) -$

[16] As such the efficient outcome is always an equilibrium of the induced coordination game, and the change in payoffs resulting from incentives does not drive differences from one treatment to the other, since this change is the same for both cases.

$w_C(B) + w_C(A) + w_C(B)) = (111, 87, 60)$.

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 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.

B.1 Measures of individual preferences

B.1.1 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 14. 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.[17]

B.1.2 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 C.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.[18] Therefore,

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

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

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, 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 C.1 and Table 13 in Appendix D) 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 $\beta_i = 0$.

B.1.3 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 13 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.

B.2 Descriptive statistics

Table 8 provides descriptive statistics about individual characteristics. Notice that instead of groups, we focus here on the subjects. Our sample contains 270 subjects aged around 27, in which 49% of people are men and 51% are women. They are mainly risk-averse and also inequality averse with mean coefficients corresponding to $\bar{r} = 0.49$, $\bar{\alpha} = 1.65$ and $\bar{\beta} = 0.49$ respectively. Regarding our definition of altruism, we observe that slightly more than a half of the population is altruistic.

Table 8: 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	0.488	0.574	-1.71	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
Subject A and altruist	0.522	0.5	0	1	270
Subject B and altruist	0.578	0.494	0	1	270
Subject C and altruist	0.511	0.5	0	1	270

Notes: The sample consists of 270 subjects, 90 groups playing 2 treatments with 10 repetitions each.

B.3 Effect of individual preferences

In this section, we provide estimates of heterogeneous treatment effects at the subject level (Table 9), and correlations between individual characteristics and individual participation decisions (Table 10). The results are discussed in Section 4 in the body of the paper.

Table 9: Heterogeneous effect: individual characteristics

	Dependent variable: Participation decision					
	(1)	(2)	(3)	(4)	(5)	(6)
High ineq.	0.117***	0.162*	0.131*	0.095***	0.080**	0.163**
	(0.039)	(0.086)	(0.072)	(0.034)	(0.039)	(0.067)
High ineq.×Risk aversion	-0.030					
	(0.033)					
High ineq.×Disadv. ineq. aversion		-0.072				
		(0.083)				
High ineq.×Adv. ineq. aversion			-0.034			
			(0.074)			
High ineq.×Altruist				0.008		
				(0.043)		
High ineq.×Gender					0.039	
					(0.038)	
High ineq.×Age						-0.002
						(0.003)
Model	LPM	LPM	LPM	LPM	LPM	LPM
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,400	5,400	5,400	5,400	5,320	5,400
Nb of subjects	270	270	270	270	270	270
R^2	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. High ineq. is the outcome of the dummy for treatment when it is equal to 1. Participation decision equals 1 when subjects select "participation". × indicates interaction variable. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level.

C 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.

C.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 (s, s) and unequal distribution $(10, 0)$, with s an integer lying in $[0, 10]$. During

Table 10: Individual characteristics instead of indiv. fixed effects

	Dependent variable: Participation decision					
	(1)	(2)	(3)	(4)	(5)	(6)
High ineq.	0.099***	0.099***	0.099***	0.099***	0.099***	0.099***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)
Risk aversion		-0.092**	-0.092**	-0.092**	-0.092**	-0.102**
		(0.041)	(0.040)	(0.041)	(0.041)	(0.042)
Disadv. ineq. aversion			-0.010	-0.010	-0.010	-0.011
			(0.013)	(0.013)	(0.013)	(0.013)
Adv. ineq. aversion				0.006	-0.008	-0.038
				(0.076)	(0.122)	(0.125)
Altruist					0.011	0.029
					(0.067)	(0.069)
Age						-0.001
						(0.002)
Gender						-0.010
						(0.053)
Model	LPM	LPM	LPM	LPM	LPM	LPM
Indiv. FE	No	No	No	No	No	No
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,400	5,400	5,400	5,400	5,400	5,320
Nb of subjects	270	270	270	270	270	266
R^2	0.01	0.02	0.02	0.02	0.02	0.02

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level. High ineq. is the outcome of the dummy for treatment when it is equal to 1. × indicates interaction variable. LPM stands for Linear Probability Model. Reported standard errors are clustered at the group level.

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.

C.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.

C.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 14 in Appendix D. 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.

D 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[19]

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.

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

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 11 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.

Table 11

	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

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.

Game 2[20]

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.

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

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 12 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.

Table 12

	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

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.

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 13 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

Table 13

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

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.

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 14 below. Payments are in euros.

Payment

One of the 10 questions will be randomly drawn. A second draw will determine your

Table 14

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 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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