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**INEQUALITY AND PROCEDURAL FAIRNESS IN A MONEY  
BURNING AND STEALING EXPERIMENT**

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# **Inequality and Procedural Fairness in a Money Burning and Stealing Experiment**

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## **Abstract**

This paper presents the results of an experiment where an unequal wealth distribution was created and then subjects could act to change this wealth distribution. Subjects received money by betting and possibly by arbitrary (“undeserved”) gifts; they could then pay to reduce, redistribute and, in half of the sessions, steal money from others. The experimental results are incompatible with some standard models of interdependent preferences. Over 80% of redistributors were rank egalitarian, but how subjects perceived the problem significantly affected their redistribution activity: perceptions of fairness were not simply a matter of relative payoff, and changed according to whether a subject was undeservedly advantaged or otherwise.

*Keywords:* interdependent preferences, altruism, envy, procedural fairness, deservingness, categorization.

*JEL classification:* C72, C91.

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This paper describes the results of an experiment on the economics of inequality and procedural fairness. Models with interdependent preferences (such as altruism, envy and inequality aversion) make predictions on what distributional outcomes agents prefer for themselves and other agents. Interdependent preferences have been incorporated in rational choice models to explain a variety of empirical anomalies, for example in relation to consumption and wage determination (Frank, 1985), and public goods contribution (Offerman et al., 1996). Various policy implications derive from the idea of interdependent preferences, for example for tax determination (Frank, 1997) or the relative importance of unemployment and growth as policy goals (Oswald, 1997).

There are many but contradictory theories of preferences over distributional outcomes. In addition, evidence exists suggesting that how a decision problem is perceived (categorized) matters in determining behaviour apparently driven by interdependent preferences. For example, in deciding how much to contribute to a public good, it matters whether a “cooperative” or “entrepreneur business-strategy frame” has been induced (Elliott et al., 1998). It matters whether an agent is perceived as deserving a bigger share of the cake being bargained (e.g., Hoffman et al., 1994): agents care about the procedure by which earnings are obtained, i.e. they care about procedural fairness. The evidence from dictator games (where the dictator chooses how to split the cake and the other “player” has no say) shows that the dictator will give more if she believes the recipient is deserving a gift (Eckel and Grossman, 1996) and less if she believes she has “earned” the money she has (Todd, 2001): procedural fairness cuts both ways.

In the experiment presented in this paper, subjects received money by betting and possibly by an arbitrary allocation procedure that induced changes in the perceptions of procedural fairness. By paying a price, they could then eliminate (“burn”) and redistribute money (including their own) and, in about half of the sessions, steal money from others. Only one decision was made, and it was the final decision in the experiment, to avoid reputational considerations. Therefore, strictly speaking, only short run behavior was under study (although practice was provided). With this qualification, the experiment had two objectives. First, it provided a new setting to verify that agents care about distributional outcomes and procedural fairness against the null hypothesis of self-interest. Second, it verified the explanatory

power of competing hypotheses concerning interdependent preferences and their distribution in the population.

We find that the observed redistribution patterns were incompatible with self-interest, pure or impure envy or altruism, and Levine's (1999), Offerman et al.'s (1996) and Charness and Rabin's (2002) distributions of preference types. Over 80% of the subjects engaging in redistribution activity were rank egalitarian: they cared about reducing the scores of richer subjects at least as much or more than the poorer ones. This result supports models of distributional fairness that make subjects care about the others' individual payoffs (e.g., Charness and Rabin, 2002, or a non-linear version of Fehr and Schmidt, 1999, but not Bolton and Ockenfels, 2000), although these also are not without problems.

How subjects perceived (categorized) the problem, especially in relationship to the fairness of the procedure, significantly affected their redistribution activity. For example, almost half of the advantaged subjects in the Stealing condition could be classified as self-interested when the procedure was unfair, but none otherwise.

Section 1 presents the experimental hypotheses and design. Section 2 analyzes the results. Section 3 discusses some possible limitations of the design. Section 4 concludes.

## **1. The Experiment: Hypotheses And Description**

### *1.1. Introduction and Hypotheses*

In each session, (typically) four subjects participated first to a betting and then to a redistribution stage. The betting stage was instrumental to the creation of an unequal wealth distribution. In addition, both during the betting stage and at the start of the redistribution stage, in half of the conditions (the "Non Desert", nD, conditions) additional money was publicly given to some subjects according to some arbitrary criterion, discussed below. In the D ("Desert") conditions prizes based on performance were assigned to make the wealth distribution at least as skewed as in the nD conditions. At a fixed cost of 10% of one's own initial gains, the redistribution stage allowed both for redistribution (also to oneself in the "Stealing", S, conditions) and for "burning" (elimination) of anyone's earnings. Practice took place before both stages, and a short questionnaire was administered in the end, before payment to subjects.

Apart from the questionnaire, the experiment was fully computerized. Strict anonymity was preserved throughout, and the final decision was one-shot (so no issue of reputation was involved).

The experiment used a  $2 \times 2$  factorial design crossing the arbitrary assignment of additional money (the Desert factor) with the possibility of stealing (the Stealing factor). However, it is also useful to consider whether a subject was “advantaged” (whether by prizes or arbitrary additional endowments) or not (A/nA). So there were eight possible combinations of Advantage (A/nA), Stealing (S if allowed, nS if not allowed) and Desert (D if Desert, nD if Non Desert).

The experimental instructions are reproduced in the appendix: they are similar to those published in Zizzo and Oswald (2001). The differences between that experiment and the one described in this paper concerned: 1) a different pricing system; 2) the constant presence of arbitrary assignments of additional money in the Zizzo and Oswald experiment (i.e., procedural fairness was not manipulated across conditions); 3) the possibility only of burning in that experiment.

We can now formulate hypotheses concerning the outcome of the redistribution stage<sup>1</sup>.

*H0 (Pure Self-Interest)*: self-interested subjects should do nothing in the nS condition (since it is costly) and steal everything from everybody else in the S conditions. Even if there are “trembles” out of these dominant strategies, they should be statistically of the same magnitude across conditions.

*H1 (Pure Envy)*: since stealing brings about a greater relative advantage than burning, in the S conditions envious subjects should steal everything and burn nothing, exactly as for self-interested subjects. In the nS conditions, either subjects are not envious enough to incur the cost of burning or, if they are, they are best off burning everything of everybody else. Desert should not affect behavior.

*H2 (Pure Altruism)*: purely altruistic subjects should not steal and burn. They might redistribute some of their own gains, though this is costly more than one-to-one due to the fixed price of activity and, in addition, there is a free riding problem. We would expect more redistribution from advantaged subjects. Again, the D factor should not matter.

*H3 (Distributional Preferences)*: while the experimental design allows sharp predictions for pure altruism and envy, this is less so for distributional preferences such as inequality aversion. Here expectations are crucial, as subjects would like to tailor their activity on the basis of what others will do.

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<sup>1</sup> The data from the betting stage are analyzed in Zizzo (2001).

We may be tempted to think that poor, typically disadvantaged subjects<sup>2</sup> should engage in proportionally larger burning and stealing than the rich, typically advantaged subjects<sup>3</sup>. However, if advantaged subjects think that the others will make them poor, it is unclear that they should engage in less activity. Nevertheless, a sharp prediction does follow from models of inequality aversion such as Fehr and Schmidt (1999) and Bolton and Ockenfels (2000), or other models of distributional preferences such as Charness and Rabin (2002): desert should not matter<sup>4</sup>.

In addition, in Bolton and Ockenfels (2000), the agent cares only about having an equal share of payoff herself - not, directly, on the other players' payoffs -. Other models, such as Charness and Rabin (2000) and a non-linear version of Fehr and Schmidt<sup>5</sup> (1999), do not share this feature: we would expect that higher ranked subjects are burnt and stolen to a greater extent.

*H4 (Impure Altruism: Warm Glow and Cold Frisson)*: in the S condition, the average amounts (of the sum of the other players' scores) left unstolen should be the same across (D/nD) conditions, and equal to some k%, out of a "warm glow" or "cold frisson" from not stealing. We would also expect that higher ranked subjects be not made object of more burning and stealing. We would obviously not expect any burning out of altruism, and, once again, the D manipulation should not matter.

*H5 (Combinations)*: it is easy to think of within-subject (still more terms in the utility function) or between-subject (preference heterogeneity) combinations of the above factors fitted to explain, *ex post* rather than *ex ante*, intermediate patterns in the data. The models by Falk and Fischbacher (2001) and

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<sup>2</sup> The Pearson correlation between wealth and being advantaged was 0.841.

<sup>3</sup> There are two reasons for this. First, utility depends also on material payoff, so a subject will be less aggressive against herself than against the other subjects: since there are either one or two advantaged subjects per session, it matters significantly whether one of the two is you, in making your redistribution decision. Second, rich, advantaged subjects may expect their scores to be reduced - perhaps stolen - by the other (say) inequality averse players, and not bother about reducing them themselves - while the poor, disadvantaged subjects do not have this problem.

<sup>4</sup> One might reply that it should according to economists, such as Konow (1996), who embrace inequality aversion while at the same time allowing a role for desert. Nevertheless, by stressing a key role for the way the problem is perceived (e.g., the defensibility of trying to get a more equitable or inequitable outcome in Konow, 2000), this kind of work is better subsumed under H6 below than under H5. Charness and Rabin (2002) do talk of deservingness, but their notion of deservingness refers to intentions not perceived entitlements.

<sup>5</sup> The basic Fehr and Schmidt (1999, sections 1-5) model has linear inequality aversion terms, which make irrelevant how wealth is distributed among the other players. However, in the extended version of their model with concave inequality aversion terms (briefly discussed in their section 6), large deviations from inequality bring greater disutility than small deviations, and so the distribution of wealth among other players matters.

Charness and Rabin (2002) have utility functions embodying both distributional preferences and a care for the intentions held by the other player in deciding how the agent will be playing towards her. Levine (1998) includes a role for intentions in determining how nice an agent is (the nicer you are, the nicer I am), and has a heterogeneous preference distribution that makes the model consistent with its data. According to his distribution, some 70% of the population is self-interested or envious, and even more (some 86%) behave as such. Being conservative, we expect stealing of everything (when possible) by at least 70-75% of the population. On the basis of an allocation task, Andreoni and Miller (1998) suggested a different type distribution: they classified 34-35% of their sample as having a “Leontief utility function” ( $\min[\text{own}, \text{other's utility}]$ ). 43-44% was considered “selfish”, and 21-22% purely altruistic with weight 1 on the other’s utility. Charness and Rabin (2002) conjectured that about 70% of the population have preferences similar to “Leontief utility functions” (what they label “quasi-maximin preferences”), 20% are characterized by inequality aversion (their “difference aversion”) and 10% are envious (their “competitive preferences”). It is an interesting feature of their distribution that there are no purely self-interested agents. Offerman et al. (1996) hypothesized still another, and rather different, distribution of types. According to their computations, 65% of the subjects are self-interested, 27% altruists, only 1% envious, and 6% choose at random.

A common prediction of all these type distributions is that desert due to the way agents earn their money should not matter, nor should it interact with other factors.

H1 through H5 summarize various predictions from rational choice models. As such they differ from:

*H6 (Categorization Effects)* When we talk about how agents perceive a decision problem, we are talking about how they *categorize* it. In general, categorization of X is how an agent represents X (Smith, 1995). In relation to interdependent behaviour, Zizzo and Oswald (2001) distinguished three logical steps in the categorization process that produces interdependent behavior<sup>6</sup> (or otherwise). These are: a) the *perception* of the decision problem, such as the definition of the material payoff structure or of one’s own position in the game; an implication of this may be considered b) the *priming* (i.e., activation) of one or more categories specifically relevant to address decision problems that may involve

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<sup>6</sup> The neurotransmitter serotonin may be involved in this process (Zizzo, 2002a).

interdependent preferences (let us label them as “social categories” as a shortcut); the outcome will be c) the *activation* of interdependent preferences and production of behavior.

In the case of our experiment, subjects may, because of the existence of the advantage, perceive the game differently according to the experimental condition. This different game perception implies that subjects may prime differently two social categories, one based on desert and one on reciprocity. Desert may imply greater burning/stealing/redistribution activity in the nD than in the D condition; reciprocity might imply that, in the nD condition, A subjects may feel entitled to reciprocate and burn/steal more, out of the fear of being burnt/robbed more. Thus, H6 predicts that we should expect greater redistribution activity in nD compared to D conditions, and that interactions between different factors may be present.

Let SB (= “Self-interest Boundary”) be the proportion of subjects doing nothing in the nS conditions (i.e., not sacrificing 10% of their own initial earnings), and not stealing at least for 10% of their own earnings in the S condition. We call this proportion “Self-interest Boundary”, because it yields an upper bound to the proportion of subjects who are self-interested, even assuming some limited trembling. According to H0,  $SB = 1$  across conditions. According to H1-H5, SB should not change according to the Desert factor or any interaction of this with other factors.

### 1.2. *Detailed Description*

The experiment was performed in Oxford between June 23 and July 1, 1998. 32 sessions of 4 subjects were planned, but (due to no show-ups and a computer breakdown at the start of a session) five sessions were run with three subjects. Since three of these sessions were in a particular experimental condition (Desert and Stealing allowed), an extra session (with 4 subjects) was run in this condition. Therefore, the final number of sessions was 33, and the sample size was of 127 subjects. Subjects were mostly students. They could participate in one session only.

The experimental currency was the “doblon”. Each doblon was convertible at the end of the experiment in U.K. pounds at the rate of 0.6 pence for doblon. Not considering the redistribution stage, where gains could only be reduced, the average gains were designed to be between 1000 and 1800 doblons (i.e., between 6 and 10.8 pounds). However, as we shall see below, in about half of the sessions the possibility of “stealing” (redistributing other people’s money to oneself) provided a chance to



increase one's own earnings substantially in the redistribution stage, by an average 22 pounds or more. Subjects got 3 pounds for participation, in addition to any other earnings. The overall experiment lasted 45 minutes on average.

Every effort was made to ensure anonymity among players. The possibility of two subjects knowing each other was minimised in a variety of ways (for example, undergraduates from the same Oxford college were not paired). Subjects were seated as soon as they arrived, and screens prevented view among them. A player number (1, 2, 3 or 4) corresponded to each seat, and seats were assigned according to the alphabetical order of the participants.

The experiment presented four stages: a practice stage, a betting stage, a redistribution stage (starting with further practice), and a payment stage. The wording of the instructions was neutral (words such as "burning" or "stealing" were not used).

*Practice Stage.* In each of the ten rounds of the practice stage, players received 100 doblons, and had to choose how much of the 100 doblons to bet (i.e., a number between 0 and 100). The computer then randomly generated a number between 1 and 3. If a 1 was drawn, subjects kept the original amount (100) and gained twice the amount they had bet. If a 2 or 3 were drawn, they lost the amount they had bet. The amounts gained in the practice stage did not count towards final actual gains.

*Betting Stage.* The betting stage was identical to the practice stage except for two things: 1) the scores of all players (labelled as 1, 2, 3 and, if any, 4) were displayed on each screen and updated at the end of each round; 2) in the nD condition, players 1 and 2 - chosen as such only because of alphabetical priority - were assigned (and could bet up to) 130 doblons each round rather than 100<sup>7</sup>, and this was common knowledge; in the D condition, subjects were told that the two top earners<sup>8</sup> at the end of the stage (e.g., after the 10<sup>th</sup> round) would gain a prize of 30% of their current earnings + 500 doblons.

*Practice and Redistribution Stage.* In the nD condition, players 1 and 2 were given an additional gift of 500 doblons at the start of the following stage. The kind of computer display faced by subjects is illustrated in Figure 1.

(Insert Figure 1 about here).

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<sup>7</sup> Only player 1 in sessions with only three subjects.

<sup>8</sup> Only the top earner in sessions with three subjects.

Subjects were shown a grid displaying, from left to right: a) red cells with the initial scores of all players, and the endowment each player had received (e.g., 1800 for advantaged, non deserving - A,nD - subjects); b) green cells in which they could put numbers to eliminate earnings of any player; c) blue cells in which they could put numbers to redistribute earnings from the player on the row of the grid to the player on the column of the grid (including to oneself in the S conditions); d) red cells listing the scores of each player after any activity of the subject (but not that of the other subjects). A button called “View” was provided on the screen. By putting numbers in the various cells and clicking View, subjects could make practice. They could see column d updated with what would happen as the aggregate outcome of those numbers, without making any real decision. Subjects were actively encouraged (both in the written instructions and with a verbal reminder) to do practice (for at least ten minutes in the verbal reminder), by putting various combinations of numbers and clicking View, to get a grasp of what they could do. Most subjects appeared to follow the advice, and sometimes spent more time. When subjects were happy with their decisions, they could follow a step-by-step procedure to make their final decision.

*Payment Stage.* When everyone had made their decisions for the redistribution stage, a computer calculated the gains of each subject as her initial gains plus the sum of the activities made by each player (if the final balance was negative, it was automatically increased to 0). The final score of each player was displayed on her computer screen only. Subjects were asked to fill a short questionnaire that asked for basic questions such as the motivation behind their choices, and which indirectly verified the subjects’ understanding of the experiment. They were then asked to sign a pledge of confidence on the content of the experiment plus a receipt, and were paid their earnings, if any, plus the 3 pounds for participation. Players were paid one at a time, in an order designed to ensure that a subject walking out of the room could not see or be seen by the others. They were asked to stay seated until paid.

## 2. Results

*Evaluation of  $H_0$ .* Figure 2 displays the average proportion of redistribution made across conditions, as a fraction of the scores of each player.

*(Insert Figure 2 about here).*

Stealing is substantial when it is allowed, but always much lower than 100%. It is unlikely to be motivated only by self-interest, since, when we move from a S to a corresponding nS condition, burning regularly increases: the burning ratio is only 8% when stealing is allowed, but jumps to an average 20.20% in the nS condition. Since burning appears an (imperfect) substitute for stealing, some stealing is likely to be motivated by negative interdependence. Figure 2 appears to show a lack of predictive power by the self-interest hypothesis. Figure 3 displays the SB across conditions and confirms this impression: in one condition the SB is about 46%, in six it is in the 10-25% range, in one it is equal to 0%.

*(Insert Figure 3 about here).*

However, in going to more formal statistical testing, we may be wary of data that include the answers of subjects who misunderstood the instructions. Perhaps, eliminating them, the self-interest hypothesis can be rescued. We can use the questionnaires to weed out people whose answers show imperfect understanding of the game they were playing. This removes 19 subjects, so the final testing sample is  $n = 108$ . Figures 4 and 5 display the data for the restricted sample.

*(Insert Figures 4 and 5 about here).*

H0 fares no better under  $n = 108$  than it does otherwise. A nonparametric sign test rejects the hypothesis that  $SB = 1$  at  $P < 0.0005^9$  ( $Z = 8.307$ ). H1 also cannot explain the significance of the Desert factor, as it will be analysed below, or the SB variability shown by Figures 3 and 5.

*Evaluation of H1-H5 vs. H6.* One of the most striking features from the figures is the difference in behavior between A,nD,S and A,D,S subjects. Advantaged non-deserving subjects stole some 75% of the gains on average, but advantaged deserving subjects only about 35%. Even more surprising, 45-50% of the subjects in the first group appear to be below the Self-Interest Boundary, but none of the latter is. Since the only difference between the two conditions is the Desert manipulation, this appears evidence against H1-H5.

Since the decision was one-shot, and we are concerned with the evaluation of rational choice models, it may be useful to eliminate the cases in which, from the questionnaires, it appears evident that subjects misunderstood the instructions. Again, this leads us to consider a sample of  $n = 108$ .

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<sup>9</sup> This result is robust to the usage of the full sample ( $n = 127$ ).

Define ARR as “aggregate redistribution ratio”, i.e. the sum of any burning, stealing and other redistribution activity by the subject, divided by the sum of the scores of the other players. An F test on ARR using Desert, Stealing and Advantage as factors is significant at the 1% level (d.f. = 7,  $F = 8.901$ ,  $P < 0.0005$ ). The Stealing factor is significant ( $F = 45.6$ ,  $P < 0.0005$ ), and so is Desert ( $F = 5.999$ ,  $P < 0.02$ ); the interaction term Stealing  $\times$  Desert  $\times$  Advantage is significant at the 5% level ( $F = 4.538$ ,  $P < 0.05$ ). No other term (including the main Advantage factor) is significant, not even at the 10% level. The significance of Desert and of Stealing  $\times$  Desert  $\times$  Advantage is replicated if an F test on aggregate redistribution (AR) in absolute terms is performed.

The usage of nonparametrics shows that the significance of Desert is not sensitive to the usage of parametric tests. As predicted by the categorization effects hypothesis H6, the Spearman correlation coefficient between Desert (= 1 in nD conditions, 0 otherwise) and ARR is significantly positive ( $\rho = 0.16$ ,  $P < 0.05$ ). The same nonparametric positive correlation between Desert and AR can be found ( $\rho = 0.16$ ,  $P < 0.05$ ). The results are also robust to the use of the full sample of  $n = 127$  (with an F test, Stealing gives  $F = 49.035$ ,  $P < 0.0005$ ; Desert gives  $F = 5.043$ ,  $P < 0.05$ ; Stealing  $\times$  Deserving  $\times$  Advantage gives  $F = 5.201$ ,  $P < 0.05$ ).

In conclusion, the data support the prediction, made only by H6, that the procedure by which subjects earned money matters. It not only triggered a significantly higher aggregate redistribution, but A,nD,S subjects seemed to expect their position to be much more vulnerable than A,D,S subjects because of the unfair source of their advantage. We might conjecture that they reacted by feeling justified to reciprocate and “defend” themselves as much as possible by stealing much more in return.

There is further evidence running against predictions of specific models.

*Pure Envy.* H1 cannot explain why the stealing ratio is significantly below 1. The average amounts left unstolen per subject varied from 5.12 pounds in the A,nD,S condition to 12.53 in the A,D,S condition. These are obviously large amounts relative to the scale of experimental gains. Moreover, the nS condition prediction that, according to the degree of envy, either a subject should burn nothing or should burn everything is not supported by the data: only 2 out of 32 subjects who did something in the nS condition burnt everything out of everybody else.

*Distributional Preferences.* We found that the Advantage factor is insignificant in an F test on the aggregate redistribution ratio. Figures 2 or 4 show that such insignificance is not driven by greater gifts by advantaged subjects: such gifts should appear as “other redistribution”, and the “other redistribution” by nA subjects is often greater, not smaller, than that provided by A subjects. Nevertheless, as we discussed earlier, it is possible that expectations on others’ behavior drives the lack of significance of the Advantage factor, even with inequality averse or Leontief utility subjects.

We can test Bolton and Ockenfels’ (2000) claim that subjects care only about their own relative share of the cake, and not on how gains are distributed among the other subjects. Consider a player deciding whether and how much to change the earnings of the other players. The other players can be ranked according to their gains: assign 1 to the top ranked, 2 to the second ranked and 3 to the third ranked, if any<sup>10</sup>. Call this variable Orank. Now let Positive equal 1 if the player increases the gains of another player, and 0 otherwise. According to Bolton and Ockenfels (2000), we should expect no correlation between Orank and Positive. If the subject feels she ought to give some of her share (which may happen if she has a greater share than equitable relative to the size of the cake as a whole), she is indifferent to whom to give. However, if one considers the sample of everyone who made some change in the gains of the other players, there is a significant positive correlation between Orank and Positive ( $n = 303$ ; Spearman’s  $\rho = 0.335$ ;  $P < 0.0005$ ).

Now assign 1 to the person whose gains are reduced most by a player, among the other subjects; 2 to the second most reduced; 3 to the least reduced<sup>11</sup>: call this variable Ochange. If Bolton and Ockenfels are right, we may expect a positive correlation between Orank and Ochange because, if players choose randomly how to divide their optimal amount of changes, they might still reduce the amounts of the richer players more on average. Moreover, if they can steal and want to steal a lot, they may be forced to steal more from the rich people, anyway. For example, in the limit case of someone stealing everything from everybody, there would be a perfect correspondence between Orank and Ochange, since the richest gets stolen most, the second richest second most, and the poorest the least. This “ceiling effect” may bias the results when stealing is allowed.

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<sup>10</sup> In case of ties between first and second place, a value of 2 was assigned; in case of tie between second and third, a value of 3.

In trying to assess whether the correlation between Orank and Ochange was spurious, we drew numbers randomly from a uniform distribution, multiplied them by the score of each player faced by the decision-maker, and then computed a fictional Ochange (call it Ocarlo) based on the Monte Carlo simulation. This procedure was followed 30 times. As expected from the first bias discussed before, there was a significant positive correlation between Orank and Ocarlo: the mean Spearman correlation was  $\rho = 0.38$  (min = 0.24; max = 0.49; S.D. = 0.066). However, this is significantly less than the correlation that we actually find in the data ( $\rho = 0.806$ ).

(Insert Figure 6 about here).

Even looking at the conditions where stealing was not allowed (see Figure 6), and so eliminating the “ceiling effect” bias, the correlation was still 0.695 and so significantly different from the Monte Carlo distribution correlation (in a t test,  $t = 16.265$ , d.f. = 29,  $P < 0.0005$ ; in a nonparametric sign test,  $Z = 5.295$ ,  $P < 0.0005$ ). Therefore, a correlation appears to exist between rank and activity of which players are object: subjects seem to care that specifically richer subjects are hit more by their activity. The Bolton and Ockenfels’ model fails to take this into account.

Another way to look at the relationship between rank and redistribution is to consider the number of people who satisfy what we might call a *rank egalitarian* relationship. We consider a subject as satisfying a rank egalitarian relationship if she reduces the score of the richest of the other subjects at least as much as or more than that of the second richest, and that of the second richest at least as much or more than that of the poorest subject. If we just look at the non Stealing condition, to minimize the “ceiling effect” bias, we find that 83.33% of the subjects who engaged in any activity appeared rank egalitarian<sup>12</sup>.

*Pure and Impure Altruism, Cold Frisson.* There is significant burning, particularly in the nS condition (where on average 20.20% of earnings were burnt): this cannot be explained by either pure or impure altruism. Moreover, the amounts left unstolen show substantial variability across A/nA and D/nD conditions, ranging from  $k = 25\%$  of the cake for A,nD subjects, to 42-45% for nA subjects, to

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<sup>11</sup> Ties were treated as for Orank (see previous footnote).

<sup>12</sup> The fraction increases to 90.48% in the Stealing conditions.

64% for A,D subjects: this runs against the hypotheses of a warm glow or cold frisson. So does our finding of a strong correlation between rank and being victim of redistribution activity.

*Combination of H1-H6, and Levine's Distribution.* Combinations of the above models could be used to try to fit the data better, but they would still be unable to explain the relevance of desert and its interaction with the other factors. There is specific evidence against Levine's (1998) distribution, and Figures 3 and 5 show why: the prediction that 70% (or more) of the subjects would steal everything does not hold. SB is significantly below 0.7, as a binomial test points out [with the full sample,  $SB = 34.65\%$ ; with the "understanding" sample  $n = 108$ , average  $SB = 34.26\%$ ; either way,  $\text{Prob}(SB = 0.7) < 0.0005$ ]. At the same time, though, as the figures show, in seven conditions out of eight SB is well above 0, thus failing to provide support for Charness and Rabin's (2002) suggested distribution.

Offerman et al.'s (1996) distribution of types is also rejected. Giving is much less common than burning or stealing, and the prediction that 65% of the subjects are self-interested fails a binomial test, once again at  $P < 0.0005$ . Only 1-7% of the subjects should burn in the non Stealing condition, but 49.21% did. In the Stealing condition, no more than 73% should have stolen – we would not expect the altruistic subjects to steal -, but 95.31% of the subjects actually stole something. Binomial tests easily show the significance of these differences ( $P < 0.0005$ ).

Andreoni and Miller's (1998) distribution would appear the least off the mark. It allows for a fraction of inequality-averse subjects, and so successfully predicts the aggregate tendency for rank egalitarianism in our data. The aggregate average SB (43.66%) is not too distant from the true value, considering that, due to the different structures of our experiments, we employ different criteria to fix the exact boundaries of what to consider self-interested: in a binomial test, it is rejected at "only" the 5% significance level (e.g.,  $P = 0.026$  with  $n = 127$ ). Moreover, Andreoni and Miller's (1998) model can predict the correlation between rank and having one own score being stolen and burnt, as discussed earlier. Nevertheless, the Andreoni and Miller's distribution cannot explain the variability in the SB distribution displayed by Figures 3 and 5. The significance of desert and its apparent effect on the expectations formulated by the advantaged subjects in the Stealing condition cannot be accommodated in this framework, at least as long as it conceives desert without paying due consideration to procedural fairness concerns.

In conclusion, models consistent with rank egalitarianism and, among the type distributions of preferences, Andreoni and Miller (1998) present the most adequate fit of our data in a rational choice framework. Nevertheless, all the rational choice models of interdependent preferences we discussed cannot explain certain features in our data. Moreover, Levine's (1998) and Charness and Rabin's (2002) attention to intentions might not be misplaced: what made advantaged subjects in the Stealing condition behave differently according to the source of their advantage might have been what they thought that their disadvantaged counterparts would have done. Expectations were sensitive to the way the problem was perceived.

We cannot exclude that extensions of the distributional preferences framework to allow for cognitive processing may go a long way explaining the data (e.g., Konow, 2000). All we can say is that consideration of outcomes alone is not enough, and that categorization effects, such as those entailed by the perception of procedural fairness, affected behavior.

*Economics training.* It is known from public goods experiments that economists tend to make marginally worse citizens, by contributing less (e.g., Frank et al., 1993). In my experiment, training in economics or game theory was not significantly correlated to aggregate redistribution. However, there was a significant positive correlation between such training and the stealing ratio (Spearman  $\rho = 0.191$ ,  $P < 0.02$ ). Since economics training affects how a subject perceives the decision problem (e.g., modifying the expectations on how the other players will behave), this also possibly reflects a categorization effect.

### **3. Limitations**

The study presented in this paper has two main limitations. First, the final decision was not repeated many times, and so an opportunity to learn a “more rational” response was not provided. Many economists would consider rational choice predictions in the short run slightly beside the point. However, this is not the case in general: Andreoni and Miller's (1998) experiment is also static and without repetition or feedback on the given task, and yet the authors stress the conformity of their findings with rational choice. Unlike our experiment, they do not even have a practice stage.



In our experiment repetition is more difficult to implement than in standard bargaining experiments, because of the larger sample size and the importance of having a new wealth distribution at the start of every redistribution stage. Hence, since the design is new, I decided to start from the simplest experimental design possible to avoid reputation effects: a one-shot decision. Undoubtedly, further research must look into repetition.

Nevertheless, it is unclear that repetition would necessarily eliminate an explanatory role for categorization effects (see Cookson, 1997). Moreover, the existence of a practice stage, the statistical analysis with the reduced sample of “surely understanding” subjects and a manipulation made in the money burning experiment described in Zizzo and Oswald (2001) all ensure that the results are not a by-product of misunderstanding of the instructions. In about half of the sessions of the Zizzo and Oswald experiment, we added verbal instructions stressing that any activity was costly and that the decision to be taken (in the Zizzo and Oswald equivalent of the redistribution stage) was the only one and final. We also tried individually to explain subjects exactly what they were doing, whenever they wanted to go on to the final decision, in order to check their full understanding of the consequences of their actions. We found that this “understanding-checking” manipulation was always insignificant.

One might also argue that the study of decisions in the short run is a better mirror of many economic decisions than providing intensive learning incentives across ten or one hundred rounds, which may be unlikely in the real world in many cases. Therefore, how subjects assimilate a decision problem to more familiar others can be of independent interest. In addition, we found patterns in the data (such as rank egalitarianism or the role of desert) that cannot be explained by random behavior alone: this suggests that at least some subjects took the experiment seriously.

The second limitation of the design is that, due to the role of expectations, we could not test distributional preferences theories as strictly as pure altruism or envy. Zizzo (2002b) addressed this concern by having an experiment that was very similar to Zizzo and Oswald’s (2001) money burning design, but where the decisions of only one player chosen randomly was implemented, after everyone has made their decisions. Rank egalitarianism carried over when this “random dictator” design was used. But obviously further research is required.

Other objections to the experimental design are less serious. For example, one might object that 10% of one's own earnings might be too small a price (typically some 1 or 2 U.K. pounds) to be taken seriously. However, this observation neglects that, in the Stealing condition, the amounts left unstolen were much larger.

Another only marginally relevant objection is that the "prize race" of the D conditions may have induced a "competitive frame" that carried out in the redistribution stage, at least for some subjects. Since this bias is in the direction of understating the impact of desert, its elimination may only strengthen the results of this paper. Similarly, the objection that the inducement of desert was too weak in my experiment, based as it was on gains of chance relative to arbitrary distributions, only strengthens the conclusions of this paper: it shows that procedural fairness concerns mattered even if introduced in this minimal way.

#### 4. Conclusions

This paper presents a new experimental test on preferences towards wealth distributions and procedural fairness. I construct a laboratory experiment in which subjects earn money by betting and, in about half of the sessions, by receiving undeservedly assigned gifts. Subjects are then told the experiment is finishing and offered a last decision. They are anonymously allowed to eliminate, redistribute and, in about half of the sessions, steal other subjects' money. To do this, they have to pay a price (that is, give up some of their own cash).

A large fraction of subjects was not purely self-interested. Significant amounts of money were left unstolen. When stealing was not allowed, about 20% of the earnings were burnt. Over 80% of the subjects engaging in any activity were rank egalitarian. There was a strong correlation between wealth, or rank, and the amounts by which subjects were burnt: a majority of the subjects was *rank egalitarian*. Bolton and Ockenfels' (2000) model does not predict rank egalitarianism, and is therefore not consistent with our data. Instead, other theories of distributional preferences such as Charness and Rabin's (2000) and a non-linear version of Fehr and Schmidt (1999) can explain rank egalitarianism.

Apart from testing among different theories of distributional preferences, the experiment allowed a new test of other theories and distributions of interdependent preferences: specific predictions made by

pure and impure models of altruism and envy were rejected. We considered the distributions of preference types estimated by Andreoni and Miller (1998), Levine (1998), Offermans et al. (1996) and Charness and Rabin (2002), and found the greatest support for Andreoni and Miller's.

However, the rational choice models considered in this paper cannot easily explain why perceptions of procedural fairness affected behaviour, in such a way that the fraction of subjects that can be classified as self-interested varied dramatically across conditions, from 0% to almost 50%. Perceptions of desert mattered, even if introduced in an arguably minimal way, and undeservedly advantaged subjects might have engaged in defensive stealing when this was allowed.

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## Appendix. Experimental Instructions.

*There were 4 versions of instructions, according to the experimental condition. Condition 1: Non Desert, Stealing allowed. Condition 2: Non Desert, Non Stealing. Condition 3: Desert, Stealing. Condition 4: Desert, Non Stealing. Small changes had to be made in the three sessions with only three subjects.*

### STAGE 1 INSTRUCTIONS

In this experiment you will use the computer to read information and make decisions.

Typically you will be asked to enter a number in one or more cells - such as that on the bottom-left corner of this screen - and to click some buttons. To input or change numbers, click the mouse pointer in the cell. You will then be able to type or erase numbers in the cell using the keyboard. Please always remember to type numbers as digits (say, 50) rather than as letters (say, fifty). You can give commands to the computer by clicking on the grey buttons at the appropriate times. Examples on the current screen are OK, Confirm, Cancel and Help. Note that only Help is currently highlighted, meaning that you can only click on Help right now (but please wait until you have read these instructions!). To press a button, click on it with the mouse pointer. Always click on Help to pass to the next screen of instructions.

**IMPORTANT:** please do NOT try to exit the experiment program even temporarily. Do NOT tamper with the computer in any other way (such as turning it off or removing the floppy disk). On various occasions you will be asked to click a button to check whether the other players have made their choices and the computer has made the necessary computations. Please, do NOT click the button continuously. Wait at least 10 seconds between attempts. You are NOT allowed to speak to any other participant in the experiment at any time. Further, if you need to speak to the experimenter, you should do quietly. If you have a query which the instructions are unable to solve, please raise your hand and we'll do our best to solve it - either on a piece of paper or with a low voice.

The above rules are essential for a smooth and speedy completion of the experiment. If you violate them, you may force everyone to lose much additional time, and you may be asked to leave the room and lose ALL gains AND the participation token. Thanks a lot!!!

The experiment is divided into four stages. The first stage is for practice. The second and third are the real experiment. The fourth stage is for the payment. We are going to use an experimental currency, the doblon. Your final doblon gains (except those of the practice stage) will be converted into UK pounds in the payment stage, at the rate of 0.6 pence per doblon. Unlike those earned later in the experiment, the doblons earned in the practice stage will NOT count towards your final gains and will NOT be convertible for money - the practice stage is only for practice, not to let you earn money! However, the doblons gained in the real experiment (stages 2 and 3) and which you still have by the end of stage will be converted into UK pounds in the payment stage. During the experiment your gains may go down as well as up. However, no player's balance will ever be allowed to fall below zero.

Moreover, whatever your final doblon gains from stage 2 and 3, you will be given an additional payment of 3 pounds for participation in stage 4.

### WELCOME TO THE PRACTICE STAGE!

There are 10 rounds. Each round you receive 100 doblons for practice and you can choose to bet any amount of them, i.e. you can choose to bet between 0 and 100 doblons each round. Please write your choice in the left-down box of this screen.

To go ahead with your choice, press the OK button of the main screen and then Confirm. If you are not sure about your choice, even after having pressed OK, but before having pressed Confirm, press Cancel. After having pressed OK and Confirm, the computer randomly generates a number between 1 and 3. If you get 2 or 3, you lose the money you bet. If you get a 1, you win: you keep the original amount of money you bet and gain double the amount (for ex., if you bet 100, you get 200 overall).

Example 1: Jill receives 100 doblons. She bets 50 doblons. Assume she wins. Then she retains the 50 doblons she bet (50), plus the money she did not bet (50), plus she earns  $2 \times 50 = 100$  doblons more. So she earns a total of 200 doblons from the round. Now assume she loses. Then she is left with only the money she did not bet, that is with 50 doblons.

Example 2: Jamie receives 100 doblons. He bets 0 doblons. He wins  $2 \times 0$  if a 3 is drawn, and loses 0 otherwise, so, whatever the number, he is left with 100 doblons.

Jane receives 100 doblons. She bets all of them. She wins  $2 \times 100$  if a 3 is drawn, and loses 100 otherwise. So her overall winning from the round is 300 if she wins, and 0 otherwise.

Click Help to make this screen disappear and the first round start. Click Help another time to make the instructions appear again. Note: while these instructions are in view, you won't be able to take decisions.

## STAGE 2 INSTRUCTIONS.

WELCOME TO STAGE 2 OF THE EXPERIMENT!!! In this stage you will play bets for real money, and this is why your score is 'restarting' from zero.

*Non Desert Conditions Only:* [Players have been assigned a number according to the alphabetical order of their last names. Players 1 and 2 get 130 doblons each round. Players 3 and 4 get 100 doblons each round. Each round you can bet from 0 up to the amount you receive each round (100 or 130). Put the number of doblons you are betting in the box in the bottom-left corner of the screen.]

All players are given 100 doblons each round. Each round you can bet from 0 up to the amount you receive each round (100). Put the number of doblons you are betting in the box in the bottom-left corner of the screen.

*Desert Conditions Only:* [The two players who at the end of all ten rounds will have the highest overall winnings, will get a prize equal to 30% of their earnings plus an additional 500 doblons. [If two (or more) players are tied for one prize, who gets the prize between them will be decided entirely randomly].]

To go ahead with your choice, press the OK button and then Confirm. If you are not sure about your choice, even after having pressed OK, but before having pressed Confirm, press Cancel. You can NOT change your choice for the round after having pressed BOTH OK AND Confirm.

After having pressed OK and Confirm, the computer randomly generates a number between 1 and 3. If a 1 is drawn, you win: you keep the money you bet and earn double the amount. If you get 2 or 3, you lose the money you bet.

To pass to the next screen, press the Help button.

There are ten rounds. After having pressed Confirm, and before passing to the following round, the computer will check whether the other players have made their choices. Once everybody has made her choice, the updated winnings of each player will appear on the screen.

Example: Jill receives 100 doblons. She bets 50 doblons and wins. Therefore she retains the 50 doblons she bet (50), plus the money she did not bet (50), plus she earns  $2 \times 50 = 100$  doblons more. So she earns a total of 200 doblons from the round. Now assume she loses. Then she is left with only the money she did not bet, that is with 50 doblons.

*Non Desert Conditions Only:* [In the meanwhile, Jamie receives 130 doblons. He bets 0 doblons. He wins  $2 \times 0$  if a 1 is drawn, and loses 0 otherwise, so, whatever the number, he is left with 130 doblons.

Jane receives 130 doblons. She bets all of them. She wins  $2 \times 130$  if a 1 is drawn, and loses 130 otherwise. So her overall winning from the round is 390 if she wins, and 0 otherwise.

Assume that Jill wins and Jane loses. Then, before passing to the following screen, on Jane's screen the new amounts, identified by number, of the other players will appear. For example, if Jamie is Player 1, it will appear that Jamie got 130 doblons more by the end of the round.]

*Desert Conditions Only:* [In the meanwhile, Jay made the same bet but lost, so is left with 50 doblons; Jamie bet 0 doblons and so retains his 100 doblons; Jane bets 100 doblons and loses, so she is left with 0 doblons.

Assume now that after the 10 rounds of play, Jill has 1200 doblons, Jamie 1050, Jane 950 and Jay 800. Then Jill wins a further prize equal to the 30% of 1200 (i.e., 360) plus 500 doblons - a total of 860 doblons -, while Jamie gets a prize of 815 doblons.]

Click Help to make this screen disappear; a small label reminding your income per round will appear and you'll be able to start. Click Help again to make the instructions appear again. Note: while these instructions are in view, you won't be able to take decisions.

### STAGE 3 INSTRUCTIONS.

*Non Stealing Conditions Only:* [In this stage, you are allowed to eliminate part or all of the winnings of any player - yourself included -, and/or to transfer part or all of them from any player (again, yourself included) to any but NOT to yourself.]

*Stealing Conditions Only:* [In this stage, you are allowed to eliminate part or all of the winnings of any player (yourself included), and/or to transfer part or all of them between players (again, yourself included).]

*Non Desert Conditions Only:* [Players 1 and 2 get a GIFT of 500 doblons. Our compliments to players 1 and 2. Players 3 and 4 don't get any gift.]

To do any activity of elimination or transfer of winnings, you have to pay a price equal to 10% of your total gains.

*Non Desert Conditions Only:* [The total gains are the gains a player had until now, from income we gave her (including gifts) and from winnings.]

*Desert Conditions Only:* [The total gains are the gains a player had until now, from income we gave her and from winnings.]

Total gains do NOT include the participation token. In other words, the price of elimination and transfer is NOT proportional to the sum of total gains + participation token, but only to total gains. Further, the participation token can NOT be subject to any elimination or transferring activity.

Each row represents a player - the one in the first column from the left. The second column from the left specifies the total amount of doblons we gave each player (=total endowment to the player) in stage 2 and 3.

*Non Desert Conditions Only:* [It includes the 1000 or 1300 doblons each player received in stage 2 - in 10 rounds of 100 or 130 doblons each -, plus, if any, the 500 doblons gift previously discussed.]

*Desert Conditions Only:* [It includes the 1000 doblons each player received in stage 2. It does not consider winnings dependent on betting choices and outcomes.]

The third column from the left has the total gains of the corresponding row player. It may be higher or lower than the endowment, according to the stage 2 performance. The first column from the right displays the total gains after your activity. To update this column, press View (it is also updated automatically when you press OK). All these columns have a RED background. You cannot put any number yourself in any red cell.

You can plug and change numbers in the GREEN and SKY-BLUE cells. To eliminate gains, put the number of doblons gained by a player (and that you want to eliminate) in the green cell of the corresponding row. To transfer earnings from a player to another, put the number of doblons you want to transfer in the cell which is in the row of the first player and in the column of the other player. (*This grid was made of sky-blue cells*). You cannot at any time reduce the total gains of any player after your activity to below zero. Within such limit, once you pay the fixed price for engaging in eliminating and/or transferring activity, you can engage in any amount of elimination and/or transfer you wish, as long as you can pay the price.

Before taking a final decision, you are encouraged to spend some time plugging numbers in the cells and viewing the outcome by pressing View, just to get a better understanding of how things work out.

Once you are happy with your choices, press OK and then Confirm. Press Cancel after OK if you change your mind. Once you press Confirm, you can NOT change your mind anymore.

IMPORTANT: all players have these same instructions in front of them right now.

The final gains of each player are determined as the SUM of the activity of elimination and transfer of winnings made by ALL players. However, if such final gains are below zero, they are automatically raised to zero.



Any activity of transfer and elimination of gains will remain entirely ANONYMOUS both during and after the experiment. After everybody has taken her decisions, a screen with the final winnings (final gains from this stage plus participation token) will appear.

Please stay seated. Payment will be done one at a time and each player will be asked to leave before payment is made to another player. This is to reinforce complete anonymity.

*Desert Conditions Only:*

EXAMPLES: Assume there are two players, Jim (assume player 1) and Joe (assume player 3). Jim starts with 2000 doblons, whereas Joe starts with 1000 doblons.

*Non Desert Conditions Only:*

EXAMPLES: Assume there are two players, Jim (assume player 1) and Joe (assume player 3). Jim receives a 1000 doblons gift and starts with 2000 doblons, whereas Joe starts with 1000 doblons.

Ex. 1: Neither does any activity. Then Jim retains his 2000 doblons and Joe 1000.

Ex. 2: Joe puts 2000 in the green cell in the player 1 row. Jim does nothing. Then Joe gets 900 doblons (for he pays  $10\% \times 1000 = 100$ ) and Jim 0.

<Ex. 3:> Assume now that there is also Jane, who has 500 initial total gains.

<If Joe puts 500 in the green cell of player 1 and 400 in the sky-blue cell corresponding to the player 1 row and Jane's column.> (Due to a typo, the example in brackets was incomplete in the computer instructions, but subjects were told verbally to ignore it.)

*Stealing Conditions Only:* [Ex. 4: Jim transfers 500 of Joe's doblons to himself; Joe transfers 1000 of Jim's doblons and 250 of Jane's to himself; Jane transfers 1000 of Jim's doblons to herself and 1000 to Joe.

Then Jim's balance is 2000 (initial total gains) - 200 (price for activity: 10% of initial total gains) - 1000 (transferred away by Joe) - 1000 (transferred away by Jane) + 500 (transferred from Jim) + 250 (transferred from Jane) = 550 doblons.

Joe's balance is 1000 - 100 (price for activity) - 500 (transferred away by Jim) + 1000 (transferred from Jim by Joe) + 1000 (transferred from Jim by Jane) = 2400 doblons.

Jane's balance is 500 - 50 (price for activity) + 1000 (transferred from Jim) - 250 (transferred away by Jim) = 1200 doblons.]

*Non Stealing Conditions Only:* [Ex. 4: Jim eliminates 500 of Joe's doblons and 250 of Jane's; Joe eliminates 1000 of Jim's doblons; Jane eliminates 1000 of Jim's doblons and transfers another 1000 to Joe.

Then Jim's balance is 2000 (initial total gains) - 200 (price for activity: 10% of initial total gains) - 1000 (eliminated by Joe) - 1000 (eliminated by Jane) = -200, hence 0 since a negative balance is not allowed.

Joe's balance is 1000 - 100 (price for activity) - 500 (eliminated by Jim) + 1000 (transferred by Jane) = 1400 doblons.

Jane's balance is 500 - 50 (price for activity) - 250 (eliminated by Jim) = 200 doblons.]

PLEASE TAKE YOUR FINAL DECISION WITH CARE. Both your and the other people's winnings depend on such decision.

To make a more careful choice, we encourage you again to try out various combinations and use View to see what would happen as the outcome of your activity.

This is the last screen of instructions, and once you click help again you'll be able to actually start working. However, feel free to browse your way through the instructions screens again at any time.

PLEASE START WORKING NOW.

First, make some PRACTICE clicking on View to see what happens when you make a choice.

Second, press OK if you are satisfied with your choice and press OK on the message box that will appear.

Third, press Confirm if you are positively sure about your choices. Otherwise press Cancel.

Click Help to get the instructions back on this screen.

Fig. 1. Screens From the Redistribution Stage

PLA YER	TOTAL ENDOW MENT TO PLAYER	TOTAL GAINS	ELIMINATE FOLLOWING AMOUNT IN DOBLONS FROM ROW PLAYER	TRANSFER THE FOLLOWING AMOUNT IN DOBLONS FROM PLAYER ON THE ROW				TOTAL GAINS OF EACH PLAYER AFTER YOUR ACTIVITY(update pressing VIEW)
				To 1	To 2	To 3	To 4	
1	1800	1643	0		0	0	0	1643
2	1800	1865	0	0		0	0	1865
3	1000	1231	0	0	0		0	1231
4	1000	895	0	0	0	0		895

INSTRUCTIONS/HELP SCREEN 9/9 (click Help to get next screen).

PLEASE TAKE YOUR FINAL DECISION WITH CARE. Both your and the other people's winnings depend on such decision. To make a more careful choice, we encourage you again to try out various combinations and use View to see what would happen as the outcome of your activity.

This is the last screen of instructions, and once you click help again you'll be able to actually start working. However, feel free to browse your way through the instructions screens again at any time.

**You are Player 1**

VIEW OK

Cancel

CONFIRM! Help

PLA YER	TOTAL ENDOW MENT TO PLAYER	TOTAL GAINS	ELIMINATE FOLLOWING AMOUNT IN DOBLONS FROM ROW PLAYER	TRANSFER THE FOLLOWING AMOUNT IN DOBLONS FROM PLAYER ON THE ROW			TOTAL GAINS OF EACH PLAYER AFTER YOUR ACTIVITY(update pressing VIEW)
				To 2	To 3	To 4	
1	1800	1643	0	0	0	0	1643
2	1800	1865	0		0	0	1865
3	1000	1231	0	0		0	1231
4	1000	895	0	0	0		895

INSTRUCTIONS/HELP SCREEN 9/9 (click Help to get next screen).

PLEASE TAKE YOUR FINAL DECISION WITH CARE. Both your and the other people's winnings depend on such decision. To make a more careful choice, we encourage you again to try out various combinations and use View to see what would happen as the outcome of your activity.

This is the last screen of instructions, and once you click help again you'll be able to actually start working. However, feel free to browse your way through the instructions screens again at any time.

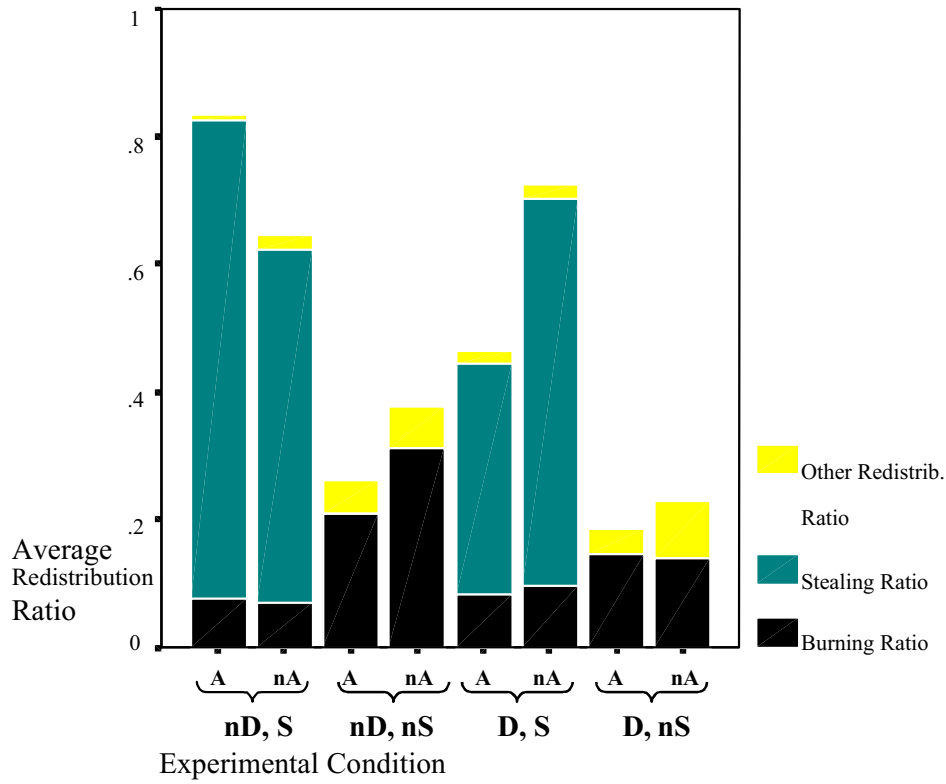
**You are Player 1**

VIEW OK

Cancel

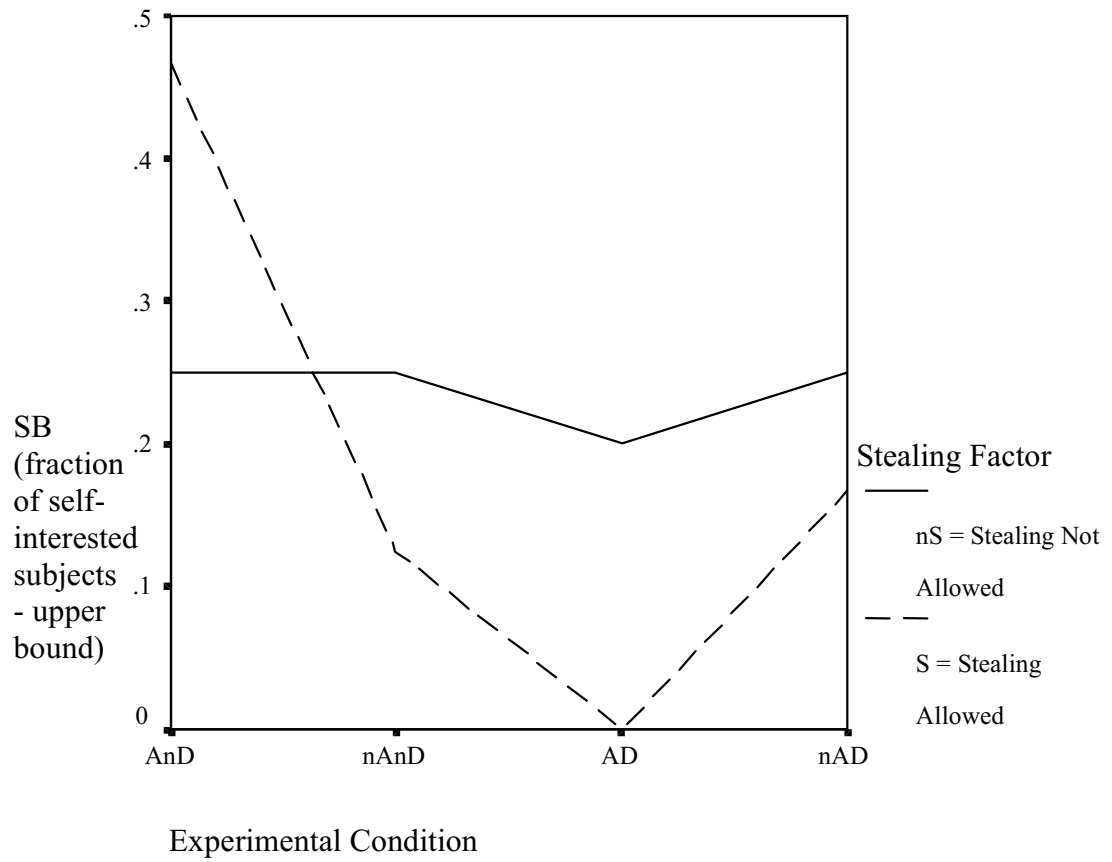
CONFIRM! Help

The computer displays on the top and the bottom of the page refer to the Stealing and the Non Stealing conditions, respectively.

Fig. 2. *Redistribution Activity For Each Experimental Condition, Full Sample*

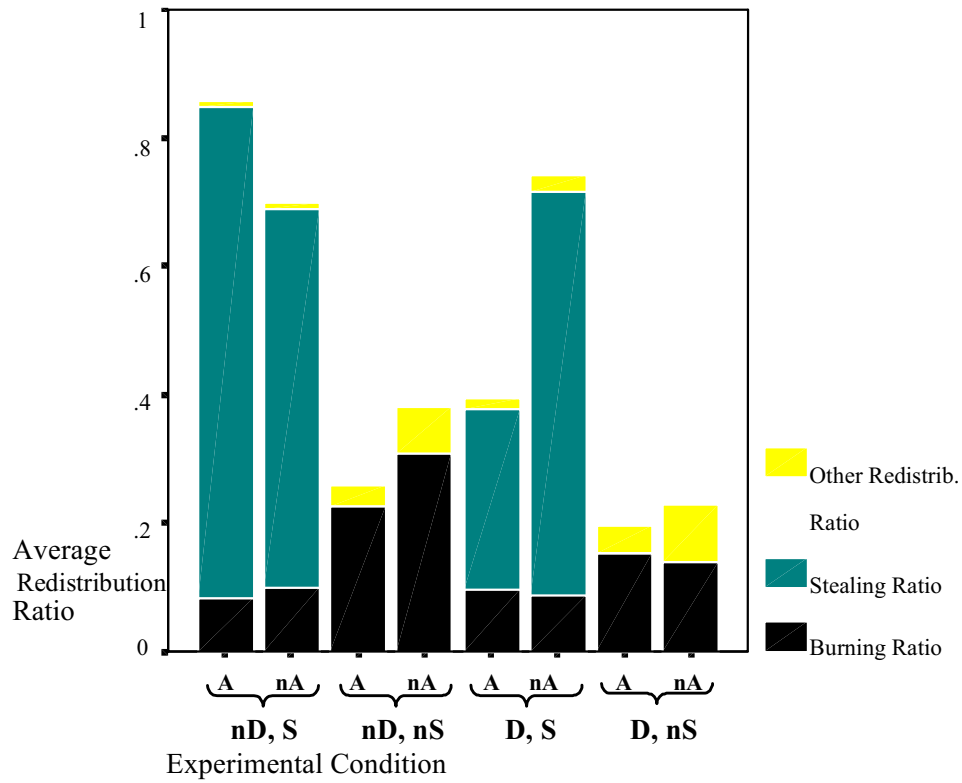
Average Redistribution Ratio (ARR) for each experimental condition (*full sample*,  $n=127$ ). ARR is the sum of the burning, stealing and other redistribution ratio for each subject. The burning ratio is equal to the amount burnt by a player divided by the sum of the scores of the other players in the session, and similarly for the stealing and the other redistribution ratios. Experimental conditions: A, nD, S = Advantaged, Non Desert, Stealing; nA, nD, S = Not Advantaged, Non Desert, Stealing; A, nD, nS = Advantaged, Non Desert, Not Stealing; nA, nD, nS = Not Advantaged, Non Desert, Not Stealing; A, D, S = Advantaged, Desert, Stealing; nA, D, S = Not Advantaged, Desert, Stealing; A, D, nS = Advantaged, Desert, Not Stealing; nA, D, nS = Not Advantaged, Desert, Not Stealing.

Fig. 3. *Upper Bound To Fraction Of Self-Interested Subjects In Each Condition; Full Sample*



Experimental conditions: AnD = Advantaged, Non Desert; nAnD = Not Advantaged, Non Desert; AD = Advantaged, Desert; nAD = Not Advantaged, Desert.

Fig. 4. *Redistribution Activity For Each Experimental Condition, “Understanding” Sample*



Average Redistribution Ratio (ARR) in each experimental condition, for the sample of subjects whose answers in the final questionnaire do not show misunderstandings (*understanding* sample, n=108).

Fig. 5. *Upper Bound To Fraction Of Self-Interested Subjects In Each Condition; “Understanding”*

*Sample*

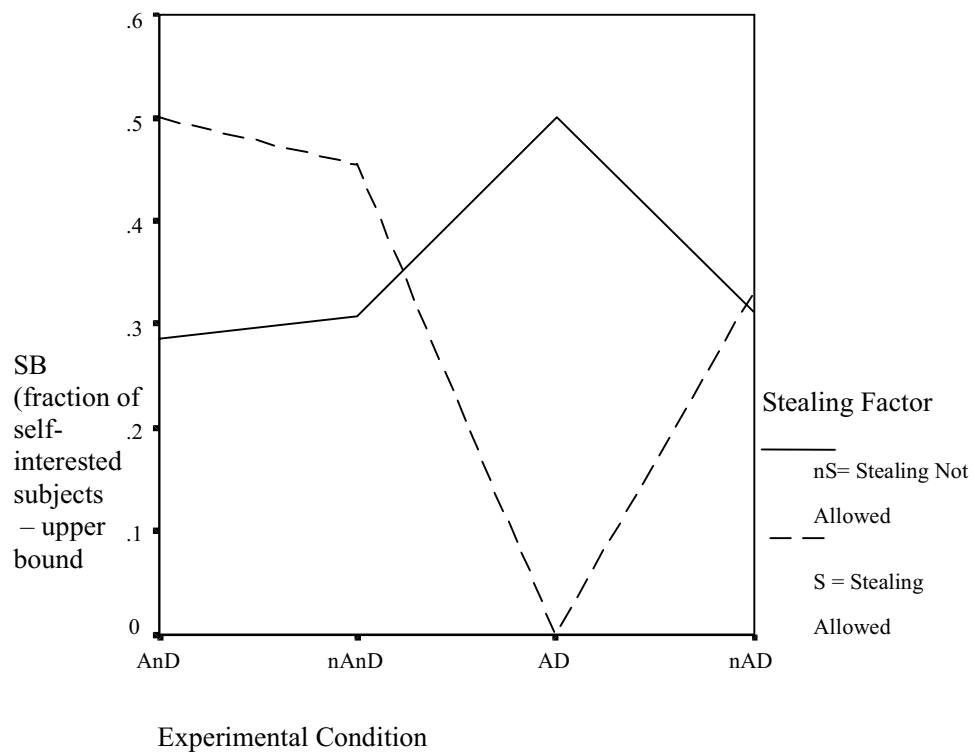
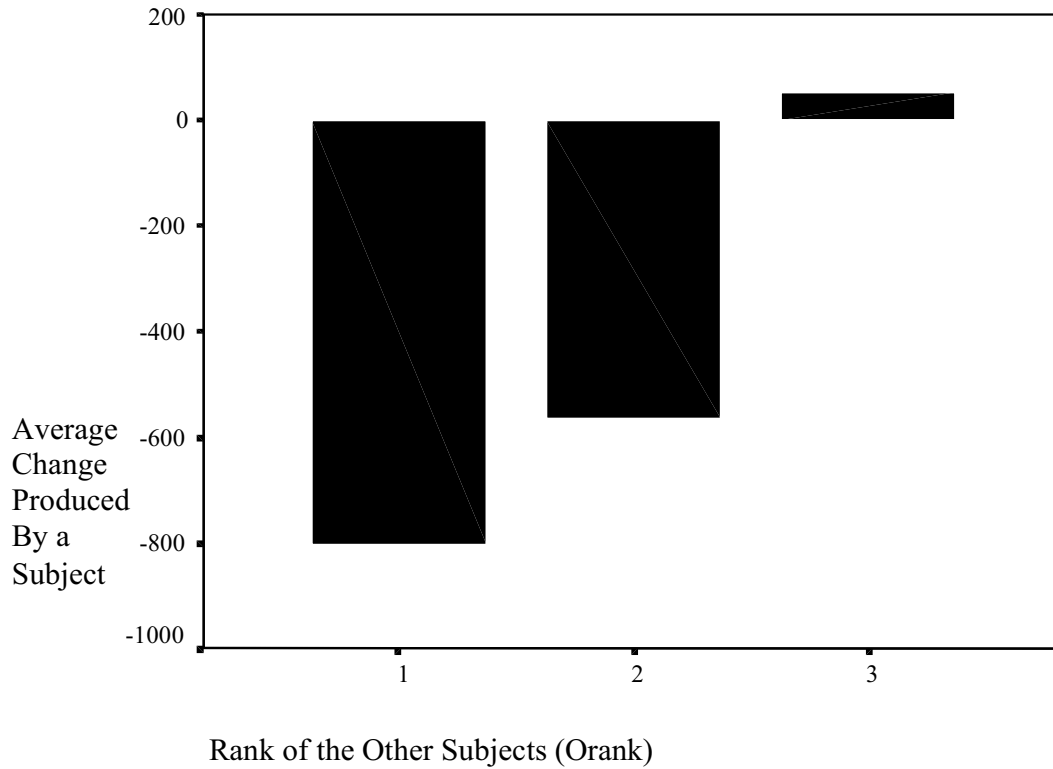


Fig. 6. *Score Changes Produced By A Subject In The Scores Of The Other Players, According To Their Rank, Non Stealing Condition Only*



Average change produced by a subject on the scores of the other subjects, according to their rank, in the non Stealing condition. Orank assigns a value of 1 to the top ranked among the other subjects, 2 to the second ranked and 3 to the bottom ranked.