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**MONEY BURNING AND STEALING IN THE LABORATORY:
HOW CONFLICTING IDEOLOGIES EMERGE**

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Money Burning and Stealing in the Laboratory: How Conflicting Ideologies Emerge

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Abstract

Three experiments on utility interdependence are discussed. Subjects received money by betting and sometimes by arbitrary assignments. They could then pay to reduce and, possibly, redistribute and steal money. In one case, only the decisions of a randomly determined dictator were implemented. The behavior of 80% of burners and redistributors was rank egalitarian. However, arbitrarily disadvantaged and advantaged subjects developed conflicting views of desert: the former targeted arbitrarily assigned money; the latter were equally or more aggressive against earned money relative to money assigned arbitrarily. Entitlements mattered, but they mattered differently to different people, depending on their sources of income.

Keywords: interdependent preferences, fairness, desert, egalitarianism, ideology.

JEL classification: C72, C91

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Entitlements matter. In determining how fair an allocation is, and making choices that may affect one own purse, it matters whether an economic agent thinks that you deserve the money you have or not (e.g., Hoffman and Spitzer, 1985; Hoffman et al., 1994). So it has been found, in the specific context of dictator and ultimatum games. Nevertheless, the new wave of ‘general’ models of interdependent preferences has virtually ignored the evidence on desert (e.g., Bolton and Ockenfels, 2000; Falk and Fischbacher, 1999): the focus has rather been on capturing simple altruism, envy or relative payoff considerations, sometimes combined with a role for intentionality. One notable exception is Konow (1998): his model of play in dictator games allows a role for entitlements in determining the dictator’s perception of a fair allocation.

This paper reviews the results of three experiments that use a new design to study utility interdependence and the role of desert. Subjects receive money by betting and possibly by an arbitrary allocation procedure. In the random dictator design, subjects can then reduce (‘burn’) the earnings of any player, but only the decisions of one of them, chosen randomly after all decisions are made, are implemented. In a second experiment, the decisions of all subjects are implemented. In a third experiment, subjects can also redistribute money (including their own) and, in about half of the sessions, steal money from others. In all three experiments, those who burn or engage in any other activity have to give up some of their own cash, and, in two of the experiments, the price of activity is varied across sessions.

We ask ourselves whether, in this new context, perceptions of desert affect the behavior of people who are given a chance to burn, and possibly redistribute and steal, money. We find that it does.

However, subjects developed conflicting views of fairness - what we might label as different ‘ideologies’ - simply as the outcome of being arbitrarily advantaged or disadvantaged. Arbitrarily disadvantaged subjects targeted virtually exclusively the money earned arbitrarily (i.e., with the arbitrary allocation). Arbitrarily advantaged subjects did not care about how money was earned, and, when stealing was allowed, were twice as aggressive against money earned by betting than against money assigned arbitrarily. Entitlements mattered, but they mattered differently for different people, depending on their sources of income.

The single most important other behavioral regularity in our data is that people cared about how money was distributed among other subjects. The behavior of about 80% of the burners and redistributors was consistently rank egalitarian: this means that, in sessions of four players, they reduced the earnings of the wealthiest among the other subjects at least as much or more than that of the second wealthiest, and that of the second wealthiest at least as much as or more than that of the poorest.

Section 1 describes the experimental design; sections 2-4 present the results; section 5 discusses the concept of ideology and considers objections to the design; section 6 concludes.

1. Experimental Design

The experiments were divided in two parts. The first part, the betting stage, was conducive to the creation of an uneven wealth distribution, and to the eventual manipulation in the perceptions of desert. The second part, the burning/redistribution stage, allowed subjects to decide whether to burn and, possibly, redistribute and steal money. Practice preceded both stages. Strict anonymity was preserved throughout: partitions prevented subjects from seeing each other; no talk was allowed between subjects; at the end of the experiment, subjects left the room one at a time, in an order designed to minimize the chances of seeing each other as they were leaving; subjects knew this in advance. Also, the final decision was one-shot. For these reasons, reputational considerations were not involved: implicit collusion would have been hard, and more so if it should have involved side-payments after the end of the experimental session¹.

It is worth stressing that the terminology used in the experiment was neutral throughout: this involved avoiding not only loaded terms such as ‘stealing’ and ‘burning’, but also avoiding any statement suggesting that earned money was deserved (unlike, say, Hoffman and Spitzer, 1985).

The experimental currency was the ‘doblón’. Each doblón was convertible at the end of the experiment in U.K. pounds at the rate of 0.6 pence for doblón. Not considering the redistribution stage, where gains could only be reduced, the average gains were designed to be between 1000 and 1800 doblóns (i.e., between 6 and 10.8 £). When stealing was allowed, this provided a chance to increase one own

¹ An effort should have been made by subjects who left earlier to wait for subjects who left later. We never saw anything like it in the proximities of the experimental site.

earnings substantially in the redistribution stage, by an average 22 pounds or more. Subjects got 3 pounds for participation, in addition to any other earning. The overall experiment lasted 45 minutes on average. Payment was done at the end of the experiment. The rest of this section describes each experiment individually, from simplest to most complex².

1.1 Experiment 1

The Betting Stage

Practice. In each of the ten rounds of practice, 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 except for two things: 1) the scores of all players (labelled as 1, 2, 3 and 4) were displayed on each screen and updated at the end of each round; 2) players 1 and 2 - chosen as such only because of the alphabetical priority of their last names - were assigned (and could bet up to) 130 doblons each round rather than 100, and all this was common knowledge.

The Burning Stage

At the start of the burning stage, players 1 and 2 were given an additional arbitrary gift of 500 doblons. After some practice, they could take a decision to reduce (hence, ‘burn’) the scores of any subject - including their own - by paying a price. However, they knew that only the decision of one player, chosen at random after everyone had made their choices, would be implemented. In other words, only the ‘burning’ choices of a ‘random dictator’ would actually affect subjects’ winnings; similarly, only if she was the dictator did a subject actually pay the price. This meant that subjects could choose their optimum amount of burning independently of any expectation about the burning activity of other players³. This is because *either* their own decision would be implemented and then the other players’ would not, *or* the decision of another player would become operative, in which case theirs would not.

² The chronological order was the reverse (see section 2).

³ The strategic independence of subjects’ actions was stressed in the verbal instructions.

The price of burning was determined according to a marginal price schedule. According to the condition, each doblon burnt cost 0.02, 0.05, 0.1 or 0.25 doblons to the burner. By paying the corresponding price, subjects could burn part or all of the score of each of the other subjects, whether obtained in the betting stage or, in the form of additional gifts, at the start of the burning stage.

Subjects were shown the grid displayed in Figure 1. From left to right, it showed: a) the initial scores of all players, and the endowment each player had received (e.g., 1800 for the arbitrarily advantaged subjects), in red cells; b) green cells in which they could put numbers to eliminate earnings from other players; c) a red column 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 then clicking View, subjects could see how column c would be updated with the aggregate outcome of those numbers, without making any real decision. Subjects were actively encouraged to practice for some time. They could do this by putting in combinations of numbers and clicking View, to get a grasp of what they could do, and to see how their own cash holdings would be diminished by reducing the scores of other subjects.

Subjects could practice around with people’s money as they liked as long as they clicked the View button, without taking any real decision. Most subjects spent considerable time making trial allocations on their screen, including subjects who then chose not to burn. When subjects were happy with their choices, they followed a step-by-step procedure to make their final decision. Subjects had to wait until everyone had made their decision, whether of zero or positive burning.

1.2 Experiment 2

The structure of Experiment 2 was identical to that of Experiment 1, with only one exception: there was not a random dictator, i.e. the decisions of all subjects were implemented (simultaneously). When all subjects had made their decisions, the score of each of them was reduced by the cost of her own burning (if any) plus the sum of the burning activities of all subjects (including herself). Since we could not allow subjects to lose money out of the experiment, scores reduced below zero were automatically raised to zero⁴.

⁴ We shall comment on this in section 5.

Most of the sessions of Experiment 2 were done one year before Experiment 1. In about half of the sessions performed, verbal instructions were added further stressing that any activity was costly and that the decision to be taken was the only one and final. When Experiment 1 was performed, similar verbal instructions were also provided; also, the printed instructions for the burning stage were given on a sheet of paper, not only on the computer screen; five extra sessions (in the 0.25 price condition) were also run using the design of Experiment 2 but the instructions presentation format of Experiment 1. This was in order to check that structural changes in the design, rather than improvements in providing the instructions, were responsible for any difference between Experiment 1 and Experiment 2. We shall refer to the 1998 Experiment 2 sessions as Experiment 2/1998, and to the 1999 sessions as Experiment 2/1999.

Experiment 2/1998 had a questionnaire at the end, which was used to check the understanding of the instructions. Later we shall refer to the ‘understanding sample’ as the sample containing only the subjects whose questionnaire answers did not display misunderstandings.

1.3 Experiment 3

Experiment 3 had some distinctive features relative to the other experiments. First, it had a redistribution stage in place of the burning stage. In the redistribution stage, subjects could not only burn but also redistribute money (including their own). In about half of the sessions (the ‘Stealing’, S, condition), subjects were allowed to redistribute money also to themselves - i.e., to steal it (the victims could not be stolen by more than they had); in the other half (the ‘non-Stealing’, nS, condition), stealing was not allowed, although any other redistribution could still take place, including giving some of one own money to any other subject. Second, the added complexity that this greater menu of choices implied was partially counterbalanced by a simpler price system. Rather than a marginal price, there was a fixed price for activity, equal to 10% of one own score: subjects could do any activity they wanted, as long as they paid the fixed price and did not reduce the score of any player below zero. The price was not varied across sessions. As in Experiment 2 but unlike Experiment 1, all choices were implemented simultaneously after everyone had made their decision (whatever that might be), and there was a short ‘understanding-checking’ questionnaire at the end. Third, Experiment 3 manipulated the perceptions of desert: about half of the

sessions (the ‘non-Desert’, nD, condition) had the same arbitrary gifts for players 1 and 2, chosen as such just for the alphabetical priority of their last names, as in the other experiments⁵; in the other half (the ‘Desert’, D, condition), there were no arbitrary gifts, but prizes based on performance were awarded to make the wealth distribution at least as skewed as in the nD condition. More specifically, in the Desert condition everyone in the betting stage got 100 doblons per round, and subjects were told that the two top earners⁶ at the end of the stage (e.g., after the 10th round) would gain a prize of 30% of their current earnings + 500 doblons; there were no gifts to players 1 and 2 at the start of the redistribution stage.

The experiment used a 2x2 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/nS) and Desert (D/nD).

Figures 2 and 3 show how the computer screen looked like in the A,S,nD (Advantaged, Stealing, non-Desert) and in the A,nS,nD (Advantaged, non-Stealing, non-Desert) condition, respectively. 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; b) green cells for burning money, as in the other experiments; c) blue cells where 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 oneself in the Stealing condition); 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, with the same function as in Experiments 1 and 2, i.e. allowing subjects to practice.

It is worth noting that the stage was not divided in two steps, one in which subjects chose to pay the redistribution fee and one in which, if they had paid, they made redistribution decisions. Rather, subjects could make any redistribution decision they liked, and, if this was non-zero, the computer would simply deduct the redistribution fee from their gains.

⁵ As discussed below, for technical reasons, five sessions were with only three subjects. In these sessions, only player 1 was arbitrarily advantaged.

⁶ In the sessions with three subjects, only the top earner got the prize.

When everyone had made their choices, payment followed, using the same procedure as in Experiment 2 - namely, if any score had been reduced below zero, it was raised to zero. In practice, this occurred only once.

2. Some Preliminary Results: Effect of Price and Limited Stealing

This section gives an overview of experimental findings, leaving the discussion of the two most interesting ones – rank egalitarianism and conflicting perceptions of desert – to the next two sections. The results include: significant burning but also, when stealing is allowed, tens of pounds left *unstolen*; a negative price effect, but never a significant one in relation to the decision to burn; virtually zero actively self-damaging activity; when stealing is allowed, evidence for crowding-out of burning by stealing, but no evidence of Pareto-efficient collusion; in the random dictator design, subjects who bet more were less object of burning.

2.1 Experiment 1

72 subjects participated to Experiment 1⁷, taking place in Oxford in the November–December of 1999. Most of the participants to this (and the other) experiments were students, and they were allowed to participate to one session (and one experiment) only. Table 1 presents some summary information on the structure of this and the other experiments and the demographics of the participants.

47.89% of the subjects chose to burn, and burnt an average 1099.15 doblons each (6.59 pounds). Figure 4 shows how the amounts burnt varied with the marginal price. There is a visible, though statistically weak, negative correlation between the price and the average amounts burnt (e.g., Spearman $\rho=-0.17$, $P<0.08$, one-tailed; things do not improve with parametric tests⁸). Define the variable Anyburn=1 when a subject decides to burn, and 0 otherwise: the correlation between Anyburn and price is correctly signed but insignificant ($\rho=-0.129$, $P=0.14$, one-tailed). There was no statistically significant difference in amounts burnt or Anyburn between advantaged and disadvantaged subjects: for example, there was just one percentage point of difference between advantaged and disadvantaged subjects deciding to burn (47.22 vs.

⁷ One observation was discarded, in correspondence to the only subject, among the 315 participating to the three experiments, who started the burning stage with zero doblons and so could not make any choice.

⁸ While here and below we use the Spearman measure of correlation, the same qualitative results can be obtained using the Pearson, unless specified otherwise.

48.57%: in a t test, $t=0.23$, $d.f.=69$, $P=0.91$, two-tailed). Parallel to this finding is a lack of correlation between score of a subject and her burning activity, or her decision to burn. Self-burning was virtually non-existent (only 2 subjects self-burnt, and they did so for trivial amounts - 17.5 doblons on average).

In two-tailed tests, subjects who bet more in the first stage appeared possibly more likely to burn less⁹ (Pearson's $r=-0.2$, $P<0.1$; Spearman's $\rho=-0.18$, $P=0.14$), but the reverse effect was stronger: subjects who bet more were less likely to be object of burning activities ($r=-0.22$, $P<0.07$; Spearman's $\rho=-0.3$, $P<0.02$).

2.2 Experiment 2

116 subjects participated to Experiment 2/1998, taking place in Warwick in the late July of 1998. Experiment 2/1999 was run at the same time as Experiment 1, and had 20 observations.

62.5% of the subjects chose to burn an average of 1281.54 doblons each. In order to make a proper comparison between these values and those in Experiment 1, we need to consider that a different mix of marginal prices was used in the two experiments - namely, Experiment 2 was 'more expensive' on average (see Table 1) -, and that the changes in the design may be confounded with the improvements in the presentation of the instructions. These problems can be addressed by focusing on the 0.25 price condition, where the statistics from Experiment 2/1999 can be suitably compared with those in Experiment 1 and 2/1998. In Experiment 2/1998, burners burnt an average 816.33 doblons, vs. 591.88 in Experiment 1 and 872.89 in Experiment 2/1999: an F test is insignificant ($F=0.201$, $d.f.=2$, $P=0.819$). Hence, improvements in instructions did not reduce the average amounts burnt by the burners. Neither did the structural changes in the design: even lumping together the Experiment 2 observations, the average burning by the burners was 835.19, and this is insignificantly different from the corresponding value in Experiment 1 ($t=0.57$, $d.f.=24$, $P=0.574$, two-tailed). In conclusion, if one looks at the 0.25 price condition to control for instruction effects, there was not a significant difference among the different experiments, notwithstanding the potential game theoretical significance of having simultaneous choices in place of a random dictator¹⁰.

The results of Experiment 2 mirror those of Experiment 1 quite closely. The correlation between price and average amount burnt is very similar ($\rho=-0.163$, $P<0.06$, one-tailed). The correlation between

⁹ The decision to burn was not affected, though.

price and decision to burn (Anyburn) is identical, and again insignificant ($\rho=-0.129$, $P=0.141$, one-tailed); the correlation may actually be inflated by the lower Anyburn in Experiment 2/1999, with its 0.25 price sessions; if we restrict ourselves to Experiment 1/1998, the correlation is weaker ($\rho=-0.104$, $P=0.187$, one-tailed), and the Pearson measure virtually zero ($r=-0.004$, $P=0.49$, one-tailed). Again, wealthier and advantaged subjects did not burn more, or take more a decision to burn, than their poorer and disadvantaged counterparts. The average betting in stage 1 was not correlated with less burning or being less object of burning. Only two subjects self-burnt, though by a greater amount on average than in Experiment 1 (200.5 on average). Finally, the picture does not change if we focus on the ‘understanding’ sample of Experiment 2/1998 ($n=106$): for example, the same fraction burnt (62.26%), and burnt approximately the same (1250.21 doblons).

2.3 Experiment 3

127 subjects participated to Experiment 3. The experiment was performed in Oxford in the June-July of 1998. 32 sessions of 4 subjects were planned, but, for technical reasons, five sessions were run with three subjects; an extra session was run in the condition most affected by this. Everyone paid the redistribution fee when stealing was allowed, while 66.67% did so when stealing was not allowed. We define ‘other redistribution’ as any redistribution that was not to oneself (i.e., not stealing): redistribution *between* other subjects and *from* oneself. Figure 1 displays the average proportion of redistribution made across conditions, as a fraction of the scores of each player. Stealing was substantial when possible, but always much lower than 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. This result is incompatible both with self-interest and with pure envy, which makes the same prediction of full stealing when this is allowed. Only 2 out of 32 subjects who did something in the non-Stealing condition burnt everything out of everybody else, and this also is incompatible with pure envy, as a purely envious subject paying the redistribution fee would be best off doing so.

¹⁰ Differences in the fraction of burners were also insignificant across conditions.

There was a strong crowding-out effect of burning and other redistribution by stealing when this was available, with $\rho = -0.348$ ($P < 0.01$, two-tailed) between stealing and burning, and $\rho = -0.424$ ($P < 0.0005$, two-tailed) between stealing and other redistribution. The burning ratio was only 8.12% when stealing was allowed, jumping to an average 20.20% in the non-Stealing condition; the other redistribution ratio was 6.21% in the non-Stealing condition, and virtually zero (1.8%) in the Stealing condition. The crowding-out effect is robust to the choice of the ‘understanding’ rather than the full sample ($n=108$). It suggests that some of the stealing in the Stealing condition was not driven by self-interest. In the Stealing condition, 32.81% of the subjects simultaneously stole and burnt. Since stealing is a superior action not only for self-interested subjects but also for any subject with negative interdependent preferences, this would again suggest that some activity was driven by fairness considerations, aimed at obtaining a defined wealth distribution rather than necessarily the best (absolute and relative) position for oneself.

One might wonder if there are any significant predictors of the payment of the redistribution fee apart from the opportunity for stealing. Let *Anychange* be equal to 1 when the subject paid the redistribution fee (equal to 10% of one’s own score) that characterized Experiment 3, and to 0 otherwise.

An F test on *Anychange* using *Stealing*, *Advantage* and *Desert* as factors does not show any significant effect, apart from that of *Stealing*. This does not change if we consider only the ‘understanding’ sample ($n=108$). Age, sex, economics background or average betting in Stage 1 were also not significant predictors of redistribution fee payment, in both the full and ‘understanding’ samples. In the non-Stealing condition, and the full sample, older people appeared more likely to choose to burn ($\rho = 0.258$, $P < 0.05$, two-tailed) and to redistribute ($\rho = 0.295$, $P < 0.02$, two-tailed), but this result is not robust to the choice of sample. Subjects who bet more in Stage 1 possibly stole more ($\rho = 0.182$, $P < 0.05$, two-tailed) and burnt less ($\rho = -0.21$, $P < 0.02$, two-tailed), but these results are not robust to the choice of correlation measure ($r = 0.125$ and $r = -0.119$, both $P > 0.15$, two-tailed) or of sample (in the ‘understanding’ sample, significance is lower).

Define AR as ‘aggregate redistribution’, i.e. the sum of any burning, stealing and other redistribution activity by the subject. Wealthier people might have engaged in slightly less aggregate redistribution, i.e.

between score and AR $r=-0.141$ ($P<0.12$, two-tailed) and $\rho=-0.16$ ($P<0.1$, two-tailed), but once again the result is not robust to the choice of sample.

In line with the other experiments, self-damaging was sporadic at best: seven subjects did so for limited amounts in the non-Stealing conditions; only three did so in the Stealing condition. The numbers drop to six and two, respectively, in the ‘understanding’ sample.

Finally, we can test the hypothesis that, to exploit the fact that final scores below zero were automatically raised to zero, subjects colluded to achieve a Pareto efficient outcome in which, say, three subjects burnt everything of a fourth subject (presumably splitting up the gains after the experiment, but this is inessential for the test). If this were the case, we would expect the Stealing condition to be characterized by a significant fraction of ‘bankrupt’ subjects, i.e. subjects whose score was reduced to below zero during the redistribution stage. However, no subject went bankrupt in the Stealing condition¹¹. Hence, there is not support for the collusion hypothesis.

3. Rank Egalitarianism

Did, in aggregate, people appear to care about how money was divided among other subjects? We shall now try to show that the answer is positive, and robustly so across experiments: this empirical finding is in agreement with Charness and Rabin’s (2000) but not Bolton and Ockenfels’ (2000) model of interdependent preferences¹².

Define ReduceObject as the amount by which a subject’s score is reduced; in the case of Experiments 1 and 2, this coincides with the amount by which she is burnt. There is a significant negative correlation between wealth and ReduceObject, i.e. richer people got reduced more (Experiment 1: $\rho=0.418$; Experiment 2: $\rho=0.33$; Experiment 3: $\rho=0.321$: in all cases, $P<0.0005$, two-tailed). To investigate this correlation further, let ORank=1 for the richest among the other players, 2 for the second richest, and 3 for the poorest; let OReduce=1 for the subject whose score a player reduces most, equal 2 for the second most

¹¹ Only one did in the non-Stealing condition.

¹² It is incompatible with the basic Fehr and Schmidt’s (1999, sections 1-5) model with linear inequality aversion terms; however, it would be compatible with their extended model with concave inequality aversion terms (briefly discussed in

reduced and 3 for the least reduced¹³. There is a strong and robust positive correlation between ORank and OReduce: in Experiment 1 $\rho=0.645$, in Experiment 2/1998 $\rho=0.578$, in Experiment 2/1999 $\rho=0.772$, in Experiment 3 $\rho=0.806$; in all cases, $P<0.0005$.

One problem with this result is that, even if subjects choose randomly how to divide their optimal amount of burning or other redistribution if feasible, there may still be a significant correlation between ORank and OReduce simply because no one can be burnt below a score of zero: since richer subjects have more, they might be burnt by a greater amount on average. We may label this as a ‘bankruptcy constraint effect’. In the Experiment 3 conditions where stealing is allowed, things are made worse by a ‘ceiling effect’: self-interested subjects who want to steal everything from everyone else *will* appear rank egalitarian, since they will steal the most from the richest among the other players, less from the second richest, and the least from the poorest. Both effects may cause a spurious correlation between ORank and OReduce. In trying to assess how strong this spurious correlation is likely to be, I 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 OReduce (call it OCarlo) based on the Monte Carlo simulation. This procedure was followed 90 times: 30 times for each experiment. As expected, there was a significant correlation between ORank and OCarlo: the mean correlation was $\rho=0.277$ (S.E.=0.01) for Experiment 1, $\rho=0.157$ (S.E.=0.01) for Experiment 2/1998, $\rho=0.325$ (S.E.=0.034) for Experiment 2/1999 and $\rho=0.381$ (S.E.=0.012) for Experiment 3¹⁴. These values help explaining the differences among the actual ρ values found empirically, i.e. why in Experiment 2/1998 the actual ρ was lowest and in Experiment 3 was highest. Nevertheless, the simulated correlations are much lower than those in the range found empirically (0.578-0.806). Hence, a correlation appears genuinely to exist between rank and activity of which players are object, in the random dictator as well as in the simultaneous choice design.

their section 6), since in this case large deviations from inequality bring greater disutility than small deviations, so (10 to self, 8 to player A and 6 to player B), say, would be worse than (10 to self, 7 to A and 7 to B).

¹³ For both ORank and OReduce, 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.

¹⁴ Even looking at the conditions where stealing was not allowed, and so eliminating the ‘ceiling effect’ bias, the correlation was still 0.695 and so significantly different from the Monte Carlo distribution correlation (e.g., in a t test, $t=16.265$, d.f.=29, $P<0.0005$).

The relationship between rank and redistribution becomes more striking when one considers 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 the second richest, and that of the second richest at least as much or more than that of the poorest subject: 82.35%, 76.05% and 100% of the burners were rank egalitarian in Experiment 1, 2/1998 and 2/1999, respectively; in Experiment 3, if we just look at the non-Stealing condition to minimise the ceiling effect, we find that 83.33% of the subjects who engaged in any activity appeared rank egalitarian¹⁵.

In conclusion, about 80% of burners and redistributors cared about how money was divided among the other subjects, a finding inconsistent with Bolton and Ockenfels (2000). This result appears robust across experiments. It appears also robust across different prices: for example, in Experiment 1 the correlation between OBurn and ORank goes from $\rho=0.633$ for $p=0.02$, to 0.564 for $p=0.05$, to 0.815 for $p=0.1$, to 0.582 for $p=0.25$, with no apparent obvious trend¹⁶.

4. Emergence of Conflicting Perceptions of Fairness

In this paper we define an ideology as a way of perceiving what is fair or unfair. We now show that not only perceptions of fairness can be affected by perceptions of desert, determined by the process by which subjects earned the money they have; but also that, as a result of this process, conflicting perceptions of desert, and hence conflicting ideologies, emerge. This is true notwithstanding the overall success of a rank egalitarian rule in describing subjects' behavior.

A first test that desert matters can be obtained simply by looking at the amount of aggregate redistribution - defined as the sum of stealing, burning and other redistribution - in the various conditions of Experiment 3. An F test on aggregate redistribution using Desert, Stealing and Advantage as factors is significant at the 1% level (d.f.=7; $F=5.643$, $P<0.0005$). Desert is significant¹⁷ ($F=4.038$, $P<0.05$), and so is the interaction term Stealing x Desert x Advantage ($F=3.767$, $P<0.06$); similar results obtain with the

¹⁵ The fraction increases to 90.48% in the Stealing conditions.

‘understanding’ sample. Hence, the non-Desert manipulation generally induced more redistribution activity. Figure 1 can help interpreting the interaction term: A,nD,S subjects engaged in significantly more activity, particularly stealing, than A, D, S subjects. The only difference between the two groups is that the former had obtained their advantage through an arbitrary procedure.

We can now devise a more sophisticated test to measure by how much, in all three experiments, each subject’s score, earned by betting or assigned arbitrarily, is comparatively reduced by advantaged and disadvantaged subjects. The intuition is to regress the score earned by betting and the score earned arbitrarily on a measure of reduction activity by either advantaged or disadvantaged subjects. To be more precise, define *ScoreObj* as the score of a subject who is object of burning or other reduction activity. Arbitrarily advantaged subjects receive an extra 300 doblons in the betting stage, and a gift of 500 doblons in the burning/redistribution stage; so let *ArbitrarilyAssigned*=800 if a subject is arbitrarily advantaged, and 0 otherwise. Let *BettingEarned* = *ScoreObj* – 800. If a subject believes that gains earned randomly and gains earned arbitrarily are equally fair or unfair, then in a multinomial regression we should have the coefficient on *ArbitrarilyAssigned* equal to that on *BettingEarned*: subjects would burn both in the same way. Conversely, if a subject believes that gains earned arbitrarily are less fair than gains earned randomly, the coefficient on *ArbitrarilyAssigned* should be higher¹⁸.

We now need to define a suitable measure of reduction activity. There is a scaling problem in doing this, because, if a subject is advantaged, there is only one other advantaged subject that can reduce her score, whereas there are two disadvantaged subjects; similarly if the subject is disadvantaged¹⁹. One way to address this problem is by defining a new variable, *Reduce*, as follows. Consider a subject *x*, who is either advantaged or disadvantaged. If *x* is advantaged, we compute two data-points of *Reduce*: the first is the *sum* of the amounts burnt from disadvantaged subjects; the second is *double* the amount burnt from the other advantaged subject. If *x* is disadvantaged, we compute two *Reduce* data-points analogously: the first is the

¹⁶ The Spearman correlation between price and $\rho(\text{OBurn}, \text{ORank})$ is equal to 0.

¹⁷ So is Stealing ($P < 0.0005$) and, in the full sample only, Advantage ($P < 0.1$).

¹⁸ At the start of the burning/redistribution stage, some advantaged subjects may have actually lost money enough that *BettingEarned* is negative, and negative earnings cannot be burnt. This might bias the coefficient on *BettingEarned* downwards. However, in practice *BettingEarned* was negative only for one observation (in Experiment 2/1998).

¹⁹ In principle, subjects could reduce their own score and so this would not be true. However, in section 2 we saw that active

sum of the amounts burnt this time from advantaged subjects; the second is *double* the amount burnt from the other disadvantaged subject.

We start from considering Experiments 1 and 2²⁰. Table 2 presents multinomial Tobit regressions on Reduce, using a few explanatory variables: BettingEarned, ArbitrarilyAssigned, Price, AvgBet (the average betting in stage 1), a couple of instructions format variables (discussed shortly), Dictator (=1 in Experiment 1 with a random dictator, and 0 otherwise) and a few interaction variables. Voice is a dummy equal to 1 for the second half of Experiment 2 and the whole of Experiment 1; VoiceExp2 is a dummy equal to 1 for the second half of Experiment 2 only. Table 2 presents two sets of regressions: those on the reduction activity by arbitrarily disadvantaged and those on that by arbitrarily advantaged subjects. In both cases, the reduction to more parsimonious specifications is accepted using likelihood-ratio tests, so we can focus on Model 2D for the disadvantaged and 2A for the advantaged²¹.

Arbitrarily disadvantaged subjects target arbitrarily assigned money almost twice as much as they do randomly earned money: for them, arbitrarily assigned money is undeserved, or at least more relatively so than earned gains; on the latter respect, it is interesting that, in the simpler and cleaner structure of Experiment 1 where they need not take expectations of other players' behavior into account, they actually prize subjects who bet a lot. This last finding sheds light on the correlational results of section 2 on betting and being object of burning. Moreover, the coefficients on BettingEarned and ArbitrarilyAssigned are outside the 95% confidence intervals of one another. The restriction that the two coefficient be identical is rejected in a likelihood-ratio test [$\chi^2(1)=4.74$, $P<0.05$].

An entirely different picture emerges by looking at arbitrarily advantaged subjects. These subjects are either equally or possibly more aggressive towards earned than arbitrarily assigned money²². Advantaged and disadvantaged subjects differ in their perceptions of what is fair to burn. Intriguingly, in Experiment 1

self-damaging was almost non-existent.

²⁰ We exclude the two Reduce data-points in correspondence of the single subject in Experiment 1 with a score of 0 at the start of the burning stage, since clearly Reduce is fixed at 0 in his case (as he could not be burnt below zero). An alternative is to exclude the entire session from the sample, but this does not change the main result as it is presented below.

²¹ The coefficients on Dictator, Voice, VoiceExp2 and Dictator x AvgBet should be considered jointly in model 2D. They suggest a small net increase (60.5) in burning in the Voice=1 condition of Experiment 2/1998. When there is a random dictator, the net effect is positive unless the mean bet is sufficiently large (e.g., 100).

²² The difference is not statistically significant.

disadvantaged subjects appear to be less sensitive to price changes than advantaged subjects, notwithstanding the greater average wealth of the latter.

Table 3 extends the regression analysis to the non-Desert condition of Experiment 3. The dependent variable is again Reduce²³; the explanatory variables are BettingEarned, ArbitrarilyAssigned, AvgBet, and StealingFactor, a dummy equal to 1 when stealing is allowed and 0 otherwise. Again, the reduction to the most parsimonious specifications (3D[^] and 2A[^]) is accepted using likelihood-ratio tests²⁴.

The main results from Experiment 1 and 2 appear robust. Disadvantaged subjects target arbitrarily assigned money exclusively in model 3D[^]; even focusing on model 2D[^], the restriction that the coefficients on RandomlyEarned and ArbitrarilyEarned be identical is rejected in a likelihood-ratio test [$\chi^2(1)=4.74$, $P<0.05$]. Conversely, advantaged subjects target money earned by betting about twice as much as arbitrarily assigned money, a difference significant at the 0.1 level in a likelihood-ratio test [$\chi^2(1)=3.06$, $P=0.08$]. For both 2D[^]/3D[^] and 2A[^] the coefficients on ArbitrarilyAssigned and RandomlyEarned are outside the 95% confidence intervals of one another.

It is instructive to compare these results with those from the Desert condition of Experiment 3 (Table 4). As one of the explanatory variables, we use Advantage=800 for the advantaged subjects, corresponding to (and in place of) the ArbitrarilyAssigned variable of the non-Desert condition. The picture is only apparently similar, for neither in the regressions in Table 4 nor in other specifications was it possible to obtain Advantage and BettingEarned as jointly significant (the standard errors are too large). They suggest the presence of significantly more variability *within* each group of subjects (advantaged and disadvantaged) than in the non-Desert condition. A reduction to the most parsimonious specification (by likelihood-ratio tests as usual) reaches the opposite results as those in the non-Desert condition. Disadvantaged subjects appear to burn on the basis of the money earned randomly – they consider the advantage justified. Advantaged subjects instead focus on the advantage, possibly because the other advantaged subject is the closest in terms of relative position, though this is just a conjecture.

²³ The five sessions with 3 subjects are excluded from the sample, because they would require changes in how the reduction activity is measured.

²⁴ Not unexpectedly, stealing makes a large difference.

The results from the Desert condition show us, by contrast, that the conflicting ideologies emerging in the non-Desert condition of Experiment 3, and in Experiments 1 and 2, are unlikely to be due just to either the estimating technique or the average wealth differences between arbitrarily advantaged and disadvantaged subjects²⁵. Rather, conflicting ideologies emerge simply out of an arbitrary manipulation of winnings, favouring some subjects against others. Arbitrarily disadvantaged subjects had a sense that money earned by betting was deserved in a way that money earned by arbitrary gifts was not; arbitrarily advantaged subjects thought the two as equally deserved or undeserved, and, in Experiment 2 and the Stealing condition of Experiment 3, considered fair to be far more aggressive against money earned by betting.

5. Discussion, Limitations and Extensions

In this paper we make no attempt to link our operational concept of ideology, interpreted as perceptions of fairness affecting behavior, to the way that sociologists tend to use this term²⁶. However, there is a sense in which our findings on the emergence of conflicting perceptions of desert are in analogy with a connotation in which the word ‘ideology’ has been adopted. In Marx and Engels (1965 [1846]) it is the socio-economic environment an economic agent finds herself in that determines her perceptions of what is fair, rather than vice versa. In our experiment, in a much more modest scale of course, we equally found that the ‘social’ position the agent finds herself in - whether she receives arbitrary gifts or otherwise - strongly affects her perceptions of what is fair, as they are revealed in her behavior. This may perhaps be related to the experimental research on self-serving conceptions of fairness (Babcock et al., 1995). In our experiments, the adopted conception of fairness is not self-serving in the sense of being necessarily in the self-interest of the subject. For example, when stealing is allowed, it would be in the subject’s best self-interest to steal everything, and so to make no difference between earned and arbitrarily assigned money, whether or not she had been advantaged: this was not what was observed. Nevertheless, it is self-serving in the sense of

²⁵ It also shows us that the conflicting ideologies are unlikely to be due just to group identity concerns (e.g., Brewer and Kramer, 1986), leading to target outgroup members more than peers.

²⁶ For a review, see Thompson (1990).

justifying one own current sources of earnings, possibly against the sources of earnings by other groups of subjects.

Of course, our finding on the emergence of conflicting ideologies needs to be qualified with the recognition that, anyway, rank egalitarianism was a good predictor of overall behavior. Rank egalitarianism is a rather weak criterion, however, based as it is purely on a set of inequalities among amounts burnt: there is still plenty of scope for conflicting perceptions of desert within a general rank egalitarian framework. There are other qualifications and objections that need to be addressed.

Playing God. A referee pointed out, in relation to Experiment 3, that players might have paid the redistribution fee to purchase the consumption good of ‘playing god’ and to avoid boredom. In its least subtle form of ‘boredom from waiting’, the criticism appears based on the assumption that stage 2 was divided in two steps, one where subjects decide whether to pay the fee and one in which, if they have paid, they ‘play god’ or otherwise they just have to wait. However, stage 2 was not divided in two steps: subjects could practice with people’s money as they liked as long as they clicked the View button, without taking any real decision; when they did make their decision, they had to wait for the other players whatever their decision had been. In practice, many subjects did play around with numbers for as much time as they liked, and then chose to play zero when it came to the actual decision. We did not detect any obvious trend for people choosing zero to finish earlier than people not choosing zero.

Also, in Experiments 1 and 2 there was a marginal price, not a redistribution fee system, and so the purchase of a consumption good of ‘playing god’ should have been much less salient, as it cannot be conceptually separated from making actual burning decisions: yet, we saw that the main results of our experiments - the emergence of conflicting ideologies and the success of rank egalitarianism - are robust to the choice of design.

Perhaps the most unequivocal prediction that would follow from the consumption good hypothesis is that of a strong negative correlation between the decision ‘to play god’ (=engage in activity) and the price of activity. However, it will be recalled from section 2 that in neither Experiment 1 or 2 (where the price

was varied) did we find a statistically significant, let alone strong, correlation between decision to burn (Anyburn) and price.

One-shot play. 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 not very useful. However, this is not the case in general, and in particular it does not apply to the experimental research on dictator games: for example, 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 experiments, they do not even have a practice stage.

In our design, 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/burning stage. Hence, since the design is new, the simplest experimental design avoiding reputational effects was used: a one-shot decision. Undoubtedly, further research must look into repetition.

Nevertheless, it is unclear that repetition would necessarily eliminate different perceptions of fairness, in the same way in which they may not eliminate framing effects (Cookson, 2000). The existence of a practice stage, the statistical analysis with the ‘understanding sample’ and the improvements made in the presentation of the instructions up to the last experiment all ensure that the main results are not a by-product of misunderstanding. 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.

Beliefs elicitation. The present experiments did not elicit beliefs *ex ante*, unlike recent research on public goods contribution and trust games (e.g., Dufwenberg and Gneezy, 2000; Croson, 2000): this may have been helpful to draw implications on expectations. Beliefs elicitation is an avenue worth pursuing in further research; however, the lack of it is not a fatal flaw of the experiments, for four reasons.

First, subjects did not have to take into account expectations on other players’ behavior in the random dictator experiment (Experiment 1). Yet, the evidence on conflicting ideologies and rank egalitarianism

emerged equally well in this as in the other experiments. Hence, the main conclusions of this paper are not affected by the lack of beliefs elicitation.

Second, Croson (2000) found that beliefs elicitation seriously distorted subjects' behavior. Since our design is new and our focus is not on studying the effect of beliefs elicitation, the only safe way of avoiding the bias is not having beliefs elicitation.

Third, beliefs elicitation would have made the task substantially more difficult for the subjects. Usually the elicitation is done by requesting a single real-valued number of some kind (e.g., a probability). However, here one should have asked at least three numbers (one in relation to the activity of each other player), and perhaps many more (in Experiment 3, in relation to how much each subject expected to burn, steal and redistribute). This may have made an already complex task still more complex; worse, it might have shifted the attention of the subjects from the actual redistribution/burning task to that of predicting other subjects' behavior correctly.

Fourth, psychologists distinguish between implicit and explicit knowledge (see Zizzo, 2000); a classic example of implicit knowledge is that of the expert billard player who would be unable to explain the laws of physics on which his expertise is based (Friedman, 1953). While it is extreme to claim that verbal responses are useless, it is equally extreme to claim that they are equally or more reliable than behavioral responses, and that therefore an experimental analysis based purely on behavioural responses cannot yield useful information. The issue is all but academic: for example, in Dufwenberg and Gneezy (2000) there is a partial conflict between verbal and behavioral analysis. Given the novelty of the design, it appeared adequate to start from a purely behaviorally grounded analysis.

Money at stake. One might object that the redistribution fee in Experiment 3 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, in many cases above 10 pounds. Also, Experiments 1 and 2 studied a wide range of prices, including the sizable 0.25 marginal price²⁷.

²⁷ A much higher marginal price faces the objection that, for B's every doblon that A burns, it is true that A becomes better off relative to B; however, this is more than offset by the loss in relative standing faced by A, relative to C and D, given the high price. Hence, not even an envious subject should burn.

The No-Loss Constraint. In Experiments 2 and 3, we were forced to increase negative final balances to zero, for obvious ethical and practical constraints - we could not ask subjects to pay us money -. This was clearly undesirable, but it does not affect our conclusions on rank egalitarianism or conflicting perceptions of desert: Experiment 1 gave the same results, while not suffering from this limitation. Further, we discussed in section 2 (and footnote 1) how there is no evidence that, when stealing was allowed, the no-loss constraint induced subjects to collude.

6. Conclusion

This paper investigates perceptions of fairness and negative interdependent preferences using a new experimental design, tested on over 300 subjects. Subjects earn money by betting or by the means of arbitrarily assigned gifts. They are then allowed to eliminate and sometimes redistribute and steal money of any player: subjects' choices are either implemented simultaneously, or only those of a randomly determined dictator get implemented. Different price schedules for burning and redistribution activity are tested.

Two robust findings emerge across different experimental conditions, using purely behavioral data. About 80% of burners and redistributors were rank egalitarian: they cared about how money was divided across other subjects. This result is incompatible with Bolton and Ockenfels (2000). However, within the broad rank egalitarian framework, arbitrarily advantaged and disadvantaged subjects developed conflicting 'ideologies', interpreted as different perceptions of fairness affecting behavior.

Arbitrarily disadvantaged subjects targeted arbitrarily assigned money; arbitrarily advantaged subjects did not care about how money was gained, and, when stealing was allowed, were twice as aggressive against money received and won in the betting stage, and which had *not* been assigned arbitrarily. To a significant degree, the emergence of conflicting and polarized ideologies was simply the outcome of being in a different situation with respect to one's own sources of earnings: entitlements mattered, but they mattered differently for different groups of people.

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Fig. 1 – Screen From the Burning Stage of Experiments 1 and 2

PLAYER	TOTAL ENDOWMENT TO PLAYER	TOTAL GAINS	ELIMINATE FOLLOWING AMOUNT IN DOBLONS FROM ROW PLAYER	TOTAL GAINS OF EACH PLAYER AFTER YOUR ACTIVITY(update pressing VIEW)
1	1800	1643	0	1643
2	1800	1865	0	1865
3	1000	1231	0	1231
4	1000	895	0	895

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.

You are Player 1

VIEW OK

Cancel

CONFIRM Help

Fig. 2 - Screen From the Redistribution Stage of Experiment 3, with Stealing Allowed

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

Fig. 3 - Screen From the Redistribution Stage of Experiment 3, with Stealing Not Allowed

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.

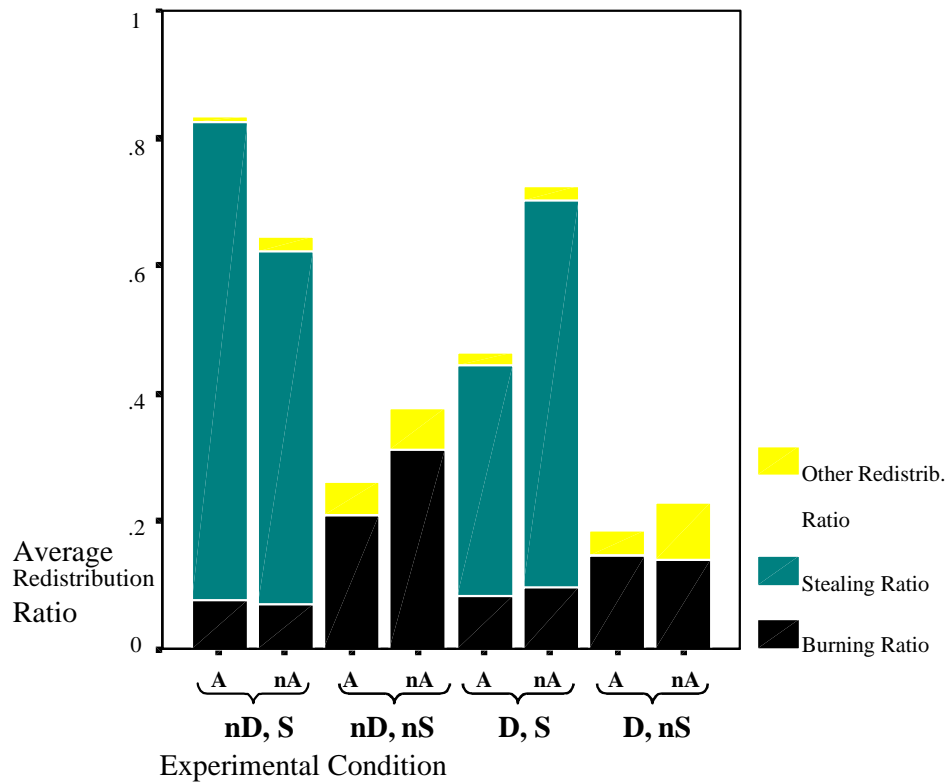
You are Player 1

VIEW OK

Cancel

CONFIRM Help

Fig. 4. Redistribution Activity For Each Condition, Experiment 3, Full Sample



Notes: The average redistribution ratio 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-Deserving, Stealing; nA, nD, S = non-Advantaged, non-Deserving, Stealing; A, nD, nS = Advantaged, non-Deserving, non-Stealing; nA, nD, nS = non-Advantaged, non-Deserving, non-Stealing; A, D, S = Advantaged, Deserving, Stealing; nA, D, S = non-Advantaged, Deserving, Stealing; A, D, nS = Advantaged, Deserving, non-Stealing; nA, D, nS = non-Advantaged, Deserving, non-Stealing.

Table 1
Descriptive Information on the Experiments

Experiment	Number of Subjects	Number of Sessions	Mean Age	% of Males	Last Stage Type of Choice	Questionnaire Provided	Burning Only Possible	Price Schedule	Price Varied	Number of sessions for each marginal price			
										0.25	0.1	0.05	0.02
1	72	18	23.25	64.79	Random Dictator	No	Yes	Marginal	Yes	5	5	4	4
2/1998	116	29	25.24	57.76	Simultaneous	Yes	Yes	Marginal	Yes	7	7	7	8
2/1999	20	5	22.25	50	Simultaneous	No	Yes	Marginal	No	5	0	0	0
3	127	33	24.39	66.93	Simultaneous	Yes	No	Fixed	No	Not applicable			

Table 2
Multinomial Tobit Regressions on Reduce, Experiments 1 and 2

Explanatory Variables	Reduction activity of <i>disadvantaged</i> subjects					
	Model 1D			Model 2D		
	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.
Voice	-472.871*	260.437	0.071	-473.088*	248.296	0.058
VoiceExp2	531.434**	254.165	0.038	533.604**	242.329	0.029
Price	-976.609	822.746	0.237	-957.868	652.535	0.144
ArbitrarilyAssigned	1.025****	0.184	0	0.985****	0.149	0
RandomlyEarned	0.631**	0.258	0.015	0.501**	0.192	0.01
MeanBet	-0.839	2.509	0.739			
Dictator	1096.512*	601.298	0.07	746.035**	302.995	0.015
Dictator x Price	-55.533	1353.826	0.967			
Dictator x ArbitrarilyAssigned	-0.054	0.307	0.861			
Dictator x RandomlyEarned	-0.333	0.396	0.401			
Dictator x MeanBet	-7.938*	4.158	0.058	-8.532**	3.314	0.011
Constant	-717.573**	358.07	0.046	-614.396**	249.044	0.014
Explanatory Variables	Reduction activity of <i>advantaged</i> subjects					
	Model 1A			Model 2A		
	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.
Voice	239.04	378.147	0.528			
VoiceExp2	234.979	365.052	0.521			
Price	-1906.405	1261.317	0.132	-1345.017	974.019	0.169
ArbitrarilyAssigned	0.423	0.263	0.109	0.421*	0.221	0.058
RandomlyEarned	0.804**	0.375	0.033	0.724**	0.296	0.015
MeanBet	-1.565	3.806	0.681			
Dictator	60.586	896.085	0.946			
Dictator x Price	-4088.896*	2357.421	0.084	-5747.819****	1521.857	0
Dictator x ArbitrarilyAssigned	0.058	0.477	0.903			
Dictator x RandomlyEarned	-0.285	0.611	0.642			
Dictator x MeanBet	-0.341	6.306	0.957			
Constant	-525.17	509.32	0.304	-367.707	346.818	0.29

Notes: The dependent variable is Reduce. If a subject is disadvantaged, two data-points of Reduce are computed: the first is the sum of the amounts burnt from advantaged subjects; the second is double the amount burnt from the other disadvantaged subject. (Similarly if a subject is advantaged). The top models include only the 207 Reduce data-points based on the reduction activity of the disadvantaged subjects. The bottom models include only the 207 Reduce data-points based on the reduction activity of the advantaged subjects. LR Test (Model 1D→Model 2D): $\chi^2(4)=0.92$, $P=0.922$. LR Test (Model 1A→Model 2A): $\chi^2(7)=5.79$, $P=0.565$. *, **, ***, ****: significance at the 0.1, 0.05, 0.01 and 0.001 level, respectively.

Table 3
Multinomial Tobit Regressions on Reduce, Experiment 3, Non Desert Condition

Reduction activity of <i>disadvantaged</i> subjects									
Explanatory Variables	Model 1D [^]			Model 2D [^]			Model 3D [^]		
	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.
RandomlyEarned	0.549	0.559	0.33	0.511	0.561	0.366			
ArbitrarilyAssigned	2.121****	0.487	0	2.141****	0.49	0	2.118****	0.492	0
MeanBet	-5.995	5.917	0.315						
StealingFactor	894.745**	392.123	0.026	825.333**	386.995	0.037	762.729*	382.645	0.051
Constant	-670.054	756.116	0.379	-906.768	730.179	0.219	-347.936	371.817	0.353
Reduction activity of <i>advantaged</i> subjects									
Explanatory Variables	Model 1A [^]			Model 2A [^]					
	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.			
RandomlyEarned	2.179****	0.508	0	2.181****	0.507	0			
ArbitrarilyAssigned	1.053**	0.415	0.014	1.051**	0.414	0.014			
MeanBet	0.398	5.27	0.94						
StealingFactor	2110.335****	349.969	0	2114.837****	344.921	0			
Constant	-2354.479****	677.682	0.001	-2337.36****	638.228	0.001			

Notes: The dependent variable is Reduce. Top models: n=60 (Reduce observations based on activity of disadvantaged subjects). Bottom models: n=60 (based on activity of advantaged subjects). LR Test (Model 1D[^]→Model 2D[^]): $\chi^2(1)=1.03$, P=0.31. LR Test (Model 1D[^]→Model 3D[^]): $\chi^2(2)=1.86$, P=0.395. LR Test (Model 2D[^]→Model 3D[^]): $\chi^2(1)=0.83$, P=0.363. LR Test (Model 1A[^]→Model 2A[^]): $\chi^2(1)=0.01$, P=0.934. *, **, ***, ****: significance at the 0.1, 0.05, 0.01 and 0.001 level, respectively.

Table 4
Multinomial Tobit Regressions on Reduce, Experiment 3, Desert Condition

Reduction activity of <i>disadvantaged</i> subjects									
Explanatory Variables	Model 1D ^o			Model 2D ^o					
	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.			
RandomlyEarned	1.612	0.993	0.111	1.905****	0.288	0			
Advantage	0.49	1.449	0.737						
MeanBet	6.209	5.457	0.261						
StealingFactor	1614.363****	335.421	0	1690.01****	337.841	0			
Constant	-2634.46**	1023.24	0.013	-2584.159****	534.864	0			
Reduction activity of <i>advantaged</i> subjects									
Explanatory Variables	Model 1A ^o			Model 2A ^o			Model 3A ^o		
	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.	Coef.	S.E.	Prob.
RandomlyEarned	0.503	1.048	0.634	0.557	1.042	0.595			
Advantage	1.153	1.502	0.446	1.09	1.497	0.47	1.861****	0.417	0
MeanBet	-2.313	5.6	0.681						
StealingFactor	1059.416***	337.577	0.003	1040.918***	334.638	0.003	1016.981***	331.129	0.003
Constant	-816.768	1080.366	0.453	-979.088	1011.143	0.338	-467.476	306.922	0.134

Notes: The dependent variable is Reduce. Top models: n=52 (Reduce observations based on activity of disadvantaged subjects). Bottom models: n=52 (based on activity of advantaged subjects). LR Test (Model 1D^o→Model 2D^o): $\chi^2(1)=1.47$, P=0.48. LR Test (Model 1A^o→Model 2A^o): $\chi^2(1)=0.17$, P=0.68. LR Test (Model 1A^o→Model 3A^o): $\chi^2(2)=0.46$, P=0.795. LR Test (Model 2A^o→Model 3A^o): $\chi^2(1)=0.29$, P=0.592. *, **, ***, ****: significance at the 0.1, 0.05, 0.01 and 0.001 level, respectively.

Supplementary Note A

Model Sample of Printed Instructions (Experiment 1, Price 0.25)

Supplementary Note A contains the printed instructions specifically used in Experiment 1. The betting stage practice instructions were identical across experiments. The betting stage instructions were identical across Experiment 1, 2 and, mostly, the nD conditions of Experiment 3; slight changes had to apply in the Experiment 3 condition sessions: a) if there were three players rather than four; b) in the D conditions. The burning/redistribution stage instructions had suitable changes according to the experimental design and condition, and of course if there were only three players.

The label 'stage' in the instructions does not entirely coincide with the label 'stage' in the paper: whereas in the paper I simply distinguish between a betting stage (stage 1) and a burning/redistribution stage (stage 2), in the experiment it was convenient to distinguish four stages (corresponding to the practice betting, the actual betting, the burning/redistribution and the payment).

BETTING PRACTICE 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

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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 2×0 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 2×100 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.

BETTING STAGE 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.

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.

To go ahead with your choice, press the OK button and then Confirm. If you are not

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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.

In the meanwhile, Jamie receives 130 doblons. He bets 0 doblons. He wins 2×0 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 2×130 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.

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.

BURNING STAGE INSTRUCTIONS

In this stage, you have a chance 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.

Each player is facing the same choices. After everyone has made his or her decision, the computer will determine randomly one player whose choices will be applied to everyone's winnings. **IN OTHER WORDS, ONLY THE CHOICES OF ONE PLAYER, DETERMINED RANDOMLY, WILL BE TAKEN INTO ACCOUNT TO CHANGE FINAL WINNINGS.** Every player has the same probability to be chosen by the computer to determine final winnings.

Since there are 4 players, this means that you have 1 chance out of 4 to be chosen by the computer to change final winnings, if so you wish. 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.

You have to pay a price for any activity of elimination of winnings. The price is 0.25 doblons per doblon eliminated, i.e. it takes 1 doblon (or fraction thereof) to eliminate

4 doblons.

You will be actually charged this amount *only* if you are the player randomly chosen by the computer to change final winnings. If this is not the case, your activity of elimination of winnings will have no impact whatsoever on your winnings. The total gains are the gains a player had until now, from income we gave her (including gifts) and from winnings. Further, the participation token can NOT be subject to any elimination 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. 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.

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 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.

You cannot at any time reduce the total gains of any player after your activity to below zero.

Within such limit, by paying the price, you can engage in whatever amount of elimination you wish.

Also remember that, in the end, only the final decision *of one player*, chosen randomly, will be applied. If, because of the random draw, your final decision gets applied (and, correspondingly, you pay the price), no one else's final decision will affect final winnings.

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.

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: Assume now that there is also Jane, who has 500 initial total gains. Joe puts 2000 in the green cell in the player 1 row. Jim and Jane do nothing. If the computer randomly chooses Joe for eliminating winnings, then Joe gets 750 doblons (for he pays $0.25 \times 1000 = 250$) and Jim 0. If the computer chooses Jim or Jane, everyone stays with his or her initial score, including Joe.

Ex. 3: Jim eliminates 1000 doblons from Joe and 500 doblons from Jane. Jane eliminates 1000 doblons from Jim. Joe does nothing. If the computer chooses Jim for eliminating winnings, Jim gets $2000 - 375$ (for he pays $0.25 \times 1500 = 375$) = 1625 doblons, while Joe and Jane get nothing (since Joe started with 1000 and Jane with 500 doblons, and Jim eliminates all of these earnings). If the computer chooses Jane, Jim gets $2000 - 1000 = 1000$ doblons, Jane gets $500 - 250$ (for she pays $0.25 \times 1000 = 250$ doblons) = 250 doblons, and Joe stays with his initial 1000 doblons. If the computer chooses Joe, everyone gets his or her initial score.

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.

Supplementary Note B
Model Sample of Verbal Instructions (Experiment 1, Burning Stage, Price 0.25)

Supplementary Note B contains the verbal instructions specifically used in the burning stage of Experiment 1 (0.25 price condition). See the notes to Supplementary Note A in relation to the labelling of the burning stage as 'third stage'.

VERBAL INSTRUCTIONS

You are now in the third stage of the experiment. Players 1 and 2 receive a gift of 500 doblons. Players 3 and 4 receive no gift.

In the third stage, you have a chance to eliminate, if so you wish, some or all of the earnings of each player. To do this, you will need to spend some of your earnings at the rate of 0.25 doblons for each doblon you want to eliminate. In other words, you need to spend 1 doblon for each 4 doblons you want to eliminate. Whereas you can eliminate people's earnings, it is not possible for you to increase your own earnings in this stage of the experiment.

Once all the players take their decisions on whether and how much to eliminate, the computer will choose randomly *one player* whose decisions will actually be carried out by the computer in determining final winnings. The decisions of all the other players will be ignored, and, since they are ignored, they will not pay anything for whatever decision they have made.

What does it mean that only the decisions of one player are actually carried out by the computer? It means that, in taking your decisions, you have to take into account that, if *you* are the one whose decisions are carried out, only *your* decisions will be carried out and affect final winnings – and *not* those of other players. In other words, if this is the case, you and you alone will have a chance to change final winnings if you wish and in the way you wish. So, if you are confused, just think at yourself and yourself alone as choosing what the final winnings of every player should be. Also, if you are confused, please raise your hand and we will be more than happy to help.

Each row on the top half of the screen represents a player, both yourself and the other players. In order to eliminate the earnings of a player, you have to insert the number of doblons you want to eliminate in the green cell corresponding to the player whose earnings you want to eliminate.

By clicking the View button, the computer will show what would happen as the outcome of your decisions. Nothing else will happen: in other words, as long as you click the View button, you are not taking any actual decision. We strongly recommend you that you do 5 or 10 minutes of practice by putting numbers in the various cells, clicking View and seeing what would happen if you actually wanted to choose those numbers. The reason we strongly recommend you to practice with the numbers is that only by making practice you will actually be able to make the decision which is best in your opinion.

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It is your choice whether to eliminate winnings or not, but please note that there has to be a number in each cell, even if this number is zero, otherwise an error message will appear. Once you are happy with the numbers you have put, click the OK and then the Confirm button. Please note that, as long as you click the View button, it is only practice you are making. However, when you click OK and confirm, it is the real decision you are making, and, once you have made it, you cannot go back and change it. So please be careful about what you are doing before clicking OK and Confirm.

You will not be allowed to take any decision until you go through all the instruction screens. If you need to write something, please use the scratch paper and not the printed instruction sheets, as these need to be used in later sessions. Again, if you have any doubts, just raise your hand and we will be more than happy to help.