Are People Willing to Pay to Reduce Others' Incomes?

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September 12, 2000

Abstract

This paper studies utility interdependence in the laboratory. We design an experiment where subjects can reduce ("burn") other subjects' money. Those who burn the money of others have to give up some of their own cash. Despite this cost, and contrary to the assumptions of economics textbooks, the majority of our subjects choose to destroy at least part of others' money holdings. We vary experimentally the amount that subjects have to pay to reduce other people's cash. The implied price elasticity of burning is calculated; it is mostly less than unity. There is a strong correlation between wealth, or rank, and the amounts by which subjects are burnt. In making their decisions, many burners, especially disadvantaged ones, seem to care about whether another person 'deserves' the money he has. Desert is not simply a matter of relative payoff.

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1. Introduction

An economic agent has interdependent preferences when her utility depends not only on her own material payoff, but also on that of other agents, either positively (say, because of altruism or group identity) or negatively (say, because of envy or feelings of unfairness). Interdependent preferences have been used to explain anomalies of various kinds, related for example to demonstration effects in consumption (Duesenberry, 1949) and upward-sloping demand curves (Bernheim and Bagwell, 1989), wage determination (Frank, 1985), bargaining (Roth, 1995), growth (Mui, 1995) and public goods (Sally, 1995). A special issue of the *Journal of Public Economics* (1998, vol. 70, n. 1) has recently been dedicated to the economics of status. Various policy implications derive from the idea of interdependent preferences, for example for tax determination (Frank, 1997), unemployment and wage rigidities (Agell, 1999), and the relative importance of unemployment and growth (Oswald, 1997).

This paper presents a new experimental test for negatively interdependent preferences. We construct a laboratory experiment. First, we create a wealth distribution by the means of a betting stage and an arbitrary gift (an unfair "advantage") to some subjects; and then subjects are anonymously allowed to reduce ("burn") other subjects' money. In the experiment we vary the price of burning, namely, the amount of their own money that subjects must relinquish to burn down other people's money. We study how people respond to these different prices. The aim is to find a way to measure, conditional on the activities of other people, the extent of negative interdependence.

Contrary to the predictions of the normal economics textbook, which stresses a narrow self-interest, we find significant evidence of burning. Two thirds of our laboratory subjects burn others – even though they have to give up some of their own money to do so. This suggests that agents display negatively interdependent preferences. They may do so for at least two reasons: either because of envy or concerns for fairness. Across prices, the price elasticity of burning varies from -

0.07 up to - 0.9. Burning does not decline much with price. It starts to do so noticeably only when a price of 0.25 is reached (meaning I have to give up 25 cents to burn one dollar of your money holdings). There is a strong correlation between wealth, or rank, and the amounts by which subjects are burnt: most subjects -- especially disadvantaged ones (i.e., those who do not receive “gifts”) -- burn the wealthiest among the other players at least as much or more as the second in wealth, and the second in wealth at least as much or more as the third. We call this finding *rank egalitarianism*. While most models of envy or inequality aversion make predictions compatible with rank egalitarianism, Bolton and Ockenfels’ (2000) model does not, and is thus inconsistent with our data. Other models of interdependent preferences are also considered in our analysis: specifically, beyond the basic envy model (as formalised, for example, by Clark and Oswald, 1998), we refer to the inequality aversion models by Fehr and Schmidt (1999) and Charness and Rabin (2000), the intentionality model[1] by Levine (1998), and the type distributions calibrated under different datasets and assumptions by Andreoni and Miller (1998) and Offerman et al. (1996).

Our data present anomalies that all currently available rational choice models of negatively interdependent preferences seem unable to explain. First, there is no correlation between the ‘price’ (that is, how much a subject gives up to reduce another’s cash) and the decision to burn. Second, some simple econometrics allows us to determine that, while advantaged subjects burn disadvantaged and advantaged subjects alike, disadvantaged subjects care *only* about whether money has been received deservedly or otherwise. They appear to use this fact in deciding whether to burn. Rational choice models cannot explain either the significance of desert, or whether it matters differently according to whether a subject is advantaged or disadvantaged.

We conclude that two factors shape negatively interdependent preferences. One is *desert*. In our laboratory setting, this produces a form of rank egalitarianism. The other is *reciprocity*. It tends

[1] This is a standard model of interdependent preferences, with the complication that the weight on the interdependent component of the utility function is dependent on whether the agent believes the other players to be more or less altruistic or envious towards her.

to induce advantaged players to target money earned by betting proportionally more than money earned by gifts -- in the expectation that in any case they will be burnt themselves. In the next section we label this as the categorisation effects hypothesis: it will be discussed further at the end of the paper.

2. Experiment Description: An Overview

The experiment was mainly performed at Warwick University in the July of 1998. All sessions were with 4 subjects. A total of 29 sessions was performed: the total sample was 116 subjects. The sample was made mostly but not only by students. Some ancillary staff, such as switchboard operators, took part. The average age of the participants was 25.24 (S.D.=5.03); the minimum age was 18, the maximum 48. An experimental currency was used, the "doblon", convertible in U.K. pounds at the end of the experiment at the rate of 0.6 pence per doblon. The experiment instructions can be found in the appendix. Five extra sessions in the 0.25 price condition were run in Oxford in the November of 1999, with instructions printed on paper and not only on the computer screen, and one or two presentational changes that will be mentioned below.

In each session, four subjects began with a betting stage. This gave them some money. They then proceeded to a burning stage. The betting stage helped to create an unequal wealth distribution. During the betting stage and the start of the burning stage, additional money was given to some subjects according to some arbitrary criterion, discussed below. The gifts were public knowledge: these amounts going to some of the players in the room were announced on everyone's computer screens. In the burning stage, subjects could eliminate ("burn") other people's money by paying some of their own winnings, at the rate (marginal price) of 0.02, 0.05, 0.1 or 0.25 experimental currency units per each unit eliminated, according to the experimental condition. When the experiment was designed, we had assumed, wrongly as it transpired, that a tax of 0.25 would tend to extinguish jealous burning. At the start of the burning stage, advantaged subjects had an average of

11 pounds sterling, and disadvantaged subjects about 6 pounds. As a guarantee to the subjects, the experiment stipulated that a minimum pay-out of 3 pounds would be received by each participant. This was to be paid in addition to any winnings remaining after the final burning stage.

The subjects were first allowed some trial rounds with no real earnings. Practice preceded both stages. In the 1998 sessions, a short questionnaire was administered in the end, before payment to subjects. This was to find out how the subjects felt they were behaving. Apart from its use of the questionnaire, the experiment was computerised. Standard experimental guidelines applied: decisions were anonymous, anonymity was preserved, and instructions were as neutral as possible (for example, the word "eliminate" was used, with no mention of the term "burn"). In about half of the 1998 sessions and all the 1999 sessions, we added a verbal clarification (indexed with a dummy variable labelled Voice in the statistical analysis presented below). It stressed some points (already in the written instructions), such as the fact that subjects could not possibly gain any money themselves by eliminating other people's winnings. No instruction was given to the subjects telling them to burn, or suggesting that we wished them to burn. The final decision was one-shot, so no issue of reputation was involved[2]. No indication was given to the subjects that they would ever be recalled to play the experiment in the future.

Some effort was made to ensure anonymity among players. On each occasion, the four subjects were kept apart as much as possible. They were seated as soon as they arrived at the laboratory. Large separating screens prevented them from seeing each other. Players were taken out separately at the end. There was almost no audible speaking in the room. A player number (1, 2, 3 or 4) was allocated to each seat. Seats were assigned according to the alphabetical order of the participants' family names. This fact was common knowledge. We viewed it as important in showing that the "unfairness" of the favoured treatment to some subjects was random. The

[2] Obviously, in future research it may be interesting to study the effect of game repetition.

advantaged subjects were always players 1 and 2, that is, the two players top in the alphabetical order.

3. Experiment Description: The Details

Each session lasted 45 minutes on average. It was divided into four stages: a practice stage, a betting stage, a burning stage (together with practice for the burning), and a payment stage

Practice Stage. In each of the ten rounds of the practice stage, players received 100 doblons. They had to choose how much of the 100 doblons to bet (i.e., a number between 0 and 100). The doblons cumulated visibly on their screen if they were not spent. The computer then randomly generated a number between 1 and 3. If a 1 was drawn, subjects kept the original amount and gained twice the amount they had bet. If a 2 or 3 was drawn, the person lost the amount he or she had bet. The screen reported the score of the subject after each round, i.e. the amount gained so far. At this juncture, no information on the scores of the other players was reported. The amounts gained in the practice stage did not count towards final actual gains, and this was also common knowledge.

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 of the ten rounds; 2) there was an experimental manipulation affecting the way money was distributed. Subjects 1 and 2 received an advantage. They got (and could bet up to) 130 doblons each round rather than the 100 of the other players, and this was common knowledge.

Practice and Burning Stage. Players 1 and 2 were given an additional gift of 500 doblons at the start of the following stage. Subjects were shown a grid displaying, from left to right: a) the initial scores of all players, and the endowment each player had received (e.g., 1800 for advantaged, non deserving - A,nD - 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, without making any real decision, how column c would be updated with the aggregate outcome of those numbers. Subjects were actively encouraged (both in the written instructions and with a verbal reminder) to practise (for at least ten minutes in the verbal reminder). They could do this by putting in combinations of number and clicking View, to get a grasp of what they could do, and to show them how their own holdings of cash would be diminished by reducing other people's. Most subjects appeared to understand and follow the advice, and sometimes spent considerable time making trial allocations on their screen. When subjects were happy with their choices, they followed a step-by-step procedure to make their final decision.

Payment Stage. When everyone had made her decision at the burning stage, a computer calculated the gains of each subject. These were her initial winnings plus an adjustment depending on the sum of the burning activities of each player (if a person's final money balance was negative, it was automatically increased to 0[3]). The closing monetary value of the player was displayed on her computer screen, and only her screen. In the 1998 sessions, subjects were then asked to fill in a short questionnaire, which asked basic questions such as the motivation behind their choices. The answers indirectly verified the subjects' understanding of the experiment. Subjects were 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 would not see or be seen by the others.

[3] This correction may have introduced a distortion in the analysis: in particular, if people expected their own gains to go below zero (i.e., if they expected to become 'bankrupt'), then their burning activity would become costless: after all, 30.01% of the burners got to negative balances at the end of the burning stage, so the bias may be potentially significant. If the bias existed, we would expect a) 'bankrupt' subjects to burn more than 'non bankrupt' subjects, and b) proportionally more bankrupt subjects among the burners than among the non-burners. However, in the 1998 sessions, a) the bankrupt burners burnt only slightly more than bankrupt ones (an average 1394 vs. 1241 doblons): the difference is not significant in a t test, not even at the 0.1 level; b) bankruptcy obtained at a virtually identical rate among burners and the non burners (26.32 vs. 27.5%, respectively). In the 1999 sessions, only two subjects went bankrupt: one was a burner, but for only 90 doblons. The distortion appears absent in our data.

They were asked to stay seated until paid. They were not encouraged to wait outside the building to meet those who had been their fellow subjects.

4. Experiment Hypotheses

H0 (Pure Self-Interest, Pure and Impure Altruism). Stealing is not possible, so purely self-interested agents should never burn. Altruists should also never burn.

One possible objection to any finding of positive burning is that subjects simply misunderstand the instructions. This objection was addressed in two ways. First, for the 1998 sessions, statistical testing was also conducted on a reduced sample of the subjects (n=106). The sample was obtained by removing those who appeared not to have understood the instructions. Second, in about half of the 1998 sessions (16 sessions out of 29), we provided some additional standardised verbal clarification (see the appendix). We also tried individually to explain to 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 shall refer to this verbal and individualised integration of the instructions as the Voice manipulation. If misunderstandings seriously compromise the seriousness of our findings, we would expect Voice to lead to lower burning. In the 1999 sessions, Voice was also provided, but there was no individual checking of instructions understanding.

H1 (Envy, Inequality Aversion, Intentionality). There are some common predictions from any rational choice model allowing for negatively interdependent behaviour. First, we would expect less burning the greater the marginal price. Second, we would expect that, the greater the price, the more the people who decide to burn. Third, we would not expect any difference in burning activity towards a doblon earned by betting and a doblon earned by an undeservedly awarded advantage. The intuition is that, if desert does not matter, subjects should treat a doblon earned by another player as gift or by betting in the same way.

The Bolton and Ockenfels (2000) model of inequality aversion predicts that people do not care about how money is divided among other subjects, i.e. they are not egalitarian. This is not the case of other models of interdependent preferences, specifically that of Charness and Rabin (2000) and a concave-utility version of Fehr and Schmidt's[4] (1999).

H2 (Type distributions). It is quite possible that people have different interdependent preferences: perhaps, say, some people are envious, others are altruistic and still others self-interested. We shall consider three recently proposed type distributions: Offerman et al.'s (1996), Levine's (1998) and Andreoni and Miller's (forthcoming). According to Offerman et al.'s distribution, we should expect no more than 7% of the people to burn; according to the Andreoni and Miller model, no more than 34.5%. This bound is basically non-existent for the (rather nasty) Levine's type distribution, where as much as 86% of the subjects could burn. This does not mean that our data will not bear on the validity of the Levine model, for this is a rational choice model still making at an aggregate level the predictions discussed above under the heading H1.

H3 (Categorisation Effects). Subjects -- particularly disadvantaged subjects if the advantaged and the disadvantaged perceive the game differently -- may tend to burn less a doblon someone else has earned by betting than one received because of an unfair advantage[5]. Desert depends not only, possibly, on distributional outcomes, but also on procedural fairness, in relation to the way a certain distributional outcome has been reached. Advantaged subjects may perceive the game differently and prime a "conflicting frame" (e.g., Shafir et al., 1997): namely, a reciprocity category may be primed in the expectation of being burnt.

5. Warwick 1998: Results

[4] We wish to thank Ernst Fehr for pointing out the need for a concave-utility version of their model (i.e., with concave interdependent utility terms): the linear-utility version will not do.

[5] We assume that subjects find fairer money earned randomly than money received by gifts. This is not contradicted by the questionnaire responses, and, as we shall see, it appears supported by the experimental evidence on the relevance of desert. The experiment was advertised as one in which people would play bets, so this may have strengthened the fairness of money earned by betting relatively to money earned by gifts.

Burning. Burning was substantial: each subject had an average 48.7% of her earnings burnt. Further, a majority of the subjects (62.5%) chose to burn, even if it implied a cost to their pocket. These basic results confirm the importance of negative interdependent preferences versus the self-interest, pure or impure altruism hypotheses.

These results are replicated using the "fully understanding" sample of the 1998 sessions[6]: 52.94% of earnings were burnt, and virtually the same fraction of subjects chose to burn (62.26%). The Voice manipulation did not lead to any large or unequivocal alteration in behaviour. The fraction of burners decreased passing from "no Voice" to "Voice", but not by much (e.g., from 67.39 to 58.33% in the "understanding" sample[7]): it is possible that a few subjects burnt because of misunderstandings. However, in the 1998 sessions the average amount burnt increased in passing from no Voice to Voice (e.g., from 46.33 to 58.01% in the "understanding" sample). Since almost six subjects out of ten still burn, even when treated with Voice and filtered on the basis of their understanding, and since the amounts burnt did not decrease with understanding, it is not credible that misunderstandings alone can explain our burning data.

Burning and price. Figure 1 plots how the amount burnt changed with the marginal price in the 1998 sessions, where there was an adequately (though not perfectly) balanced mix of sessions with different prices:

(Insert Figure 1 About Here)

The price elasticity of burning was approximately zero up to a marginal price of 0.1 (with a statistically insignificant increase between 0.02 and 0.05). Using the OLS regression slope on the 0.1 and 0.25 data, it is equal to –0.46 at 0.1 and to –1.15 at 0.25. The Pearson correlation over the entire sample is –0.153, which is on the border of significance (P=0.06 in a one-tailed test). However, the sample is highly non-normal. If we use nonparametric correlation coefficients, it is not possible to reject insignificance (Spearman's ρ=-0.068, P=0.176 in a one-tailed test).

[6] Below we refer to this sample as the "understanding sample".

Furthermore, an F test on the price factor is unable to reject the null hypothesis of zero correlation (P>0.2). Things do not improve if we consider the burn ratio (e.g., burn divided by the score of the victim) as dependent variable. They do improve if we lump together the values up to 0.1, and compare them to the burning with 0.25. In the lumped sample, the average burning is 939.81 doblons, vs. 524.79 in the 0.25 sample: the difference is significant[8]. They also improve if we consider the reduced sample of the "understanding" subjects, on the basis of questionnaire responses (n=106): now Spearman ρ=-0.1, significant at the 10% level in a one-tailed test. Nevertheless, further analysis shows that this significance is driven by the 0.25 sample. It is then not unexpected that things improve if we add the 1999 sessions with a 0.25 marginal price (e.g., Spearman ρ=-0.163, P<0.06, one-tailed).

In conclusion, there is some evidence for a price effect upon the burning decision, but this is not strong, and appears largely driven by the decrease in burning when the price becomes as large as 0.25. As with ultimatum bargaining data (e.g., Roth et al., 1991; Slonim and Roth, 1998), negatively interdependent behaviour can be quite resilient to changes in the monetary incentives. Indeed, the amount of money spent by subjects in burning activity actually *increased* with the price of burning, reaching in the 1998 sessions on average of 14.62% of one own earnings in the 0.25 price condition and raising to an average 20.9% where Voice was provided.

The lack of sensitivity of burning to price in the 0.02-0.1 range is a mixed blessing for rational choice models of negative interdependent preferences. On the one hand, a high elasticity even for so low marginal prices would not augur too well for the relevance of negative interdependent preferences. On the other hand, models of negative interdependent preferences would predict *some* elasticity, so the total insensitivity of burning with respect to price is slightly puzzling. A more

[7] In a binomial one-tailed test, the first difference is significant (P<0.05), the second only marginally so (P<0.1).
[8] The Levene's test for equality of variances is significant (P<0.05). A t test not assuming equal variances gives t=2.047 (P<0.05). A nonparametric Mann-Whitney test gives Z=1.469 (P<0.1, one-tailed).

troublesome finding for optimising models of interdependent preferences emerges from Figure 2, which shows the fraction of subjects deciding to burn for each price condition.

(Insert Figure 2 About Here)

The decision whether to burn at all was uncorrelated with price. This result is robust to the choice of test (parametric or nonparametric), to the variable (decision to burn over each price condition, or decision to burn rate in the 0.02-0.1 conditions vs. the 0.25 condition), and to the sample (full or "understanding", 1998 sessions alone or joint with the 1999 sessions). For example, a Mann-Whitney test on the up-to-0.1 data vs. the 0.25 data gives Z=1.0157 (P=0.438, one-tailed) and Z=0.266 (P=0.39, one-tailed) in the "understanding" sample. This result goes against rational choice models of interdependent preferences.

Testing specific rational choice models. Our data obviously reject models that allow only for positive interdependent preferences. These are unable to explain burning[9]. More generally, since 62.5% of the subjects burnt, any type distribution allowing for a majority of altruists is bound to fail. Such is the case of Offerman et al's (1996), with their 7% upper bound, but also of the "best" Oxford 1998 type distribution, Andreoni and Miller's (1998): their upper bound of 34.5% is significantly different from 62.5% in a binomial test (P<0.0005). Instead, as Levine's (1998) upper bound is as high as 86%, it can explain the observed high fraction of burners.

Burning and wealth. How does burning change with the score of the subjects?

On the one hand, there was a lack of correlation between score of the subject and burning. For example, if one considers the 1998 and 1999 sessions together, the Pearson r and the Spearman ρ are just 0.053 (P=0.543, two-tailed) and 0.051 (P=0.554, two-tailed), respectively.

On the other hand, there was a significant correlation between burning and score of who is the object of burning: richer subjects got burnt more (e.g., across all sessions, Spearman ρ=0.336;

[9] They would not even be able to explain self-burning, as self-burning may be an altruistic act only if the altruist believes that there are envious or fairness-sensitive types out there, which will have to be the case in the (perfect Bayesian) equilibrium of the game. Anyway, only very few people self-burnt, and typically it was clear from the instructions that

P<0.0005). We can also look at this correlation as one between the object of burning by a player and wealth rank of other players (see Figure 3). Define ORank with 1 for the top ranked in terms of wealth among the other players, 2 for the second ranked, and 3 for the last ranked. Let OBurn=1 for the subject a player burns most, equal 2 for the second most burnt and 3 for the least burnt[10]. There is a strong positive correlation between ORank and OBurn (in the 1998 sessions, ρ=0.578; in the 1999 sessions, ρ=0.772; in both cases, P<0.0005, two-tailed).

One may perhaps think that the ceiling on the amounts that can be burnt – richer subjects have more, so you can burn more of their cash – drives this effect. According to this line of argument (a variation on H0), subjects are just burning at random, and the correlation between rank and burning is spurious. To check whether this is the case, we ran some Monte Carlo simulations. In these we drew numbers from a uniform distribution, multiplied them by the score of the other players, and considered the outcomes as fictional levels of amounts burnt. One can then compute the Spearman correlation between the corresponding simulated OBurn and ORank. We repeated the procedure thirty times, finding a distribution for the simulated OBurn: the mean simulated ρ is just 0.157 for the 1998 sessions and 0.325 for the 1999 sessions. While these values are significantly above zero, they are significantly below our respective estimated values of 0.578 and 0.772 (e.g., in t tests with 29 d.f., P<0.0005, two-tailed). Therefore, while a ceiling effect might have amplified the effect somewhat, random behaviour probably cannot explain our finding of a correlation between being the object of burning and the wealth rank.

(Insert Figure 3 About Here)

Another way to study this correlation is by checking the number of advantaged and disadvantaged subjects who satisfy what we can label a *rank egalitarian* relationship. A subject satisfies a rank egalitarian relationship if she burns the wealthiest among the other players at least as

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they had not understood the instructions well.

[10] For both ORank and OBurn, 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.

much or more as the second in wealth, and the second in wealth at least as much or more as the third. In the 1998 sessions, 71.05 and 81.05% of the rich (top 2) and poor (bottom 2) subjects satisfied a rank egalitarian relationship in our data. Interestingly, the fraction is higher for the latter category, made almost entirely by undeservedly disadvantaged subjects. In the 1999 sessions, all burners satisfied the rank egalitarian relationship.

These findings on the correlation between Orank and Oburn and on rank egalitarianism constitute evidence against the Bolton and Ockenfels (2000) inequality aversion model. The findings favour rational choice models of envy or inequality-aversion in which subjects care about the relative positions of the other players, and not only about their own relative shares.

Desert and reciprocity. A different type of explanation of rank egalitarianism lies in assuming that many subjects are not actually burning rich people more because they are rich, but rather because and to the extent that they got the money they got undeservedly. In the twin experiment run in Oxford, Zizzo (2000a, 2000b) crossed advantage and desert in a factorial design, and found that desert mattered[11]. More specifically, he found significantly more negative interdependent preferences in sessions where the advantage was induced unfairly than when it was induced according to a relatively fair procedure. Moreover, in one condition of that experiment, stealing was possible. Zizzo then found that there was substantially more stealing by advantaged subjects if they had got the advantage undeservedly. One possible interpretation of this interaction effect was that undeservedly advantaged subjects expected themselves to be stolen or burnt significantly more, and behaved using a reciprocity logic, in defending their own gains significantly more.

The experiment discussed in this paper does not allow a clean isolation of a role for desert, much less one for reciprocity. There is almost perfect collinearity between being rich and having been undeservedly advantaged. Nevertheless, we can go some way towards investigating the potential role of these factors by the means of a regression analysis in which we look at how

[11] Perceptions of desert and non-desert were induced using the same procedure of the experiment described in this paper.

advantaged and disadvantaged subjects treat a doblon earned randomly versus a doblon earned undeservedly by another player. All rational choice models of negative interdependent preferences would predict a significantly positive and identical coefficient on an extra doblon by another player, whether earned deservedly or not.

Define ScoreObj as the score of the subject who is the object of burning. Advantaged subjects receive an extra 300 doblons in the betting stage, and a gift of 500 doblons in the burning stage; so let UndeservedlyEarned=800 if a subject is advantaged, and 0 otherwise. Let RandomEarned = ScoreObj – 800. If desert does not matter (the null hypothesis), then in a multinomial regression we should have the coefficient on UndeservedlyEarned equal to that on RandomEarned: subjects would burn both in the same way. If desert matters (the alternative), the coefficient on UndeservedlyEarned should be higher[12].

We could add up the sum of the burning of which a subject x is the object. Assume first that x is an advantaged subject. The burning comes from the other advantaged subject, and by the two disadvantaged subjects. Thus we could consider two distinct variables, one, BurnAdv*, defined as the burning of the other advantaged subject, and the other, BurnDisadv*, the sum of the burnings of the two disadvantaged subjects. In half of the cases x will be a disadvantaged subject instead, so BurnAdv* will be the sum of the burning of the two advantaged subjects, and BurnDisadv* the burning of the other disadvantaged subject. We want to scale the variables in the same way in the two cases. To do this, we define the variable BurnAdv if x is an advantaged subject. For any x, we compute two data-points: the first is the *sum* of the amounts burnt from disadvantaged subjects; the second is *double* the amount burnt from the other advantaged subject. We can define similarly BurnDisadv if x is a disadvantaged subject. For any x, we compute two data-points: the first is the

Other empirical literature on the role of desert is discussed in the next section.

[12] Obviously, at the start of the burning stage, some advantaged subjects may have actually lost money enough that RandomEarned is negative, and negative earnings cannot be burnt. This might bias the coefficient on RandomEarned downwards. However, only one data-point was negative in our data.

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

Define two other variables. These are used in multinomial Tobit regressions on BurnAdv and BurnDisadv. Price is simply the marginal price. Voice is equal to 1 when the Voice manipulation was administered, whether in 1998 or in 1999, and 0 otherwise; Voice1998 is equal to 1 for the Voice sessions performed in 1998 only, and 0 otherwise. Table 1 reports the results of Tobit regressions on BurnAdv and BurnDisadv, considering the full sample of 1998 and 1999 sessions and using RandomEarned, Gift, Voice and Price as regressors. We ran Tobit rather than OLS regressions because of the censored nature of our data: subjects could not burn negative numbers.

(Insert Table 1 About Here)

The coefficient on the marginal price is insignificant, confirming the weakness of the price effect that we discussed earlier on. Marginal price appears to matter twice as much for advantaged than for disadvantaged subjects. This is somewhat paradoxical, for it would imply a *more* binding budget constraints for the *richer* subjects.

The effect of changes in the presentation of the instruction appears ambiguous: there was a reduction of burning in the 1999 sessions and a small net increase in the 1998 voice sessions; however, both effects apply only to disadvantaged subjects, and the first is significant only at the 10% level.

The coefficients on RandomEarned and UndeservedlyEarned yield us the interesting results. Disadvantaged subjects appear to target undeservedly earned money substantially more than they do other money. The fact of receiving the gift was sufficient to increase the burning of which a subject was object on a basically one-to-one basis, on the part of the disadvantaged subjects alone. Disadvantaged subjects cared about the fact that the disadvantage was not deserved, more than about relative position. Auxiliary evidence for this comes from the fact that, in the 1998 sessions, if we modify the rank egalitarian rule to take the form “burn advantaged subjects at least as much or more than disadvantaged subjects; within the same advantage condition, burn the richer at least as much as the poorer", we find that nine disadvantaged burners out of ten (89.47%) satisfy this rule.

While disadvantaged subjects targeted gifts more than they did randomly earned money, exactly the reverse was true for advantaged subjects: in the regression for advantaged subjects, the coefficient on RandomEarned and is *twice* as large as that on UndeservedlyEarned. Also, the latter is statistically insignificant, albeit only marginally so. Since 71.05% of the advantaged burners satisfy the modified rank egalitarian rule in the 1998 sessions[13], the substantial minority of those who behave differently must drive this insignificant result. Indeed, the behaviour of 14.81% of advantaged burners in the 1998 sessions can be described as *reverse* rank egalitarianism: they burn disadvantaged subjects as much or more than advantaged ones (none of the disadvantaged burners did).

Current rational choice models of negative interdependent preferences fail to explain why desert should matter, and why it should matter differently for advantaged and disadvantaged subjects. Together with the finding on the insensitivity of the decision whether to burn to the marginal price, this is our most puzzling result. It seems also an important one. It would not be sufficient for, say, predictive or economic policy purposes to assume that subjects are rank egalitarian because of envy or inequality aversion; one should also take into account how fair the agents *perceive*, say, a redistributional policy.

6. Discussion

Are people willing to pay to burn other people's money? The short answer to this question is: yes. Our subjects gave up large amounts of their cash to hurt others in the laboratory. The extent of burning was a surprise to us. Because of its scale, and its resistance to monetary disincentives, we realised after just a few experimental sessions that the cost of deleting other people's pounds was

[13] It will be recalled that all burners satisfied the rank egalitarian relationship in the five 1999 sessions.

viewed by the subjects as low. Even at a price of 0.25 (meaning that to burn another person's dollar costs me 25 cents), many people wished to destroy other individuals' cash. This appears to be strong evidence for the existence of some kind of envy or concern for fairness.

The details are more complicated. We have to qualify the broad affirmative answer by recognising one limitation of the study. Our measurement of the price elasticity of burning -- and our interpretation of burning activity more generally -- faces an objection. Subjects had to burn *conditional* on the expectations of burning by the other players. These expectations may have been important. Indeed our hypothesis that reciprocity may have played a role hinges on assumptions about those expectations. Due to the role of expectations, we were not able to test for rational choice as narrowly -- for example using a structural model -- as we could have otherwise done. Unsurprisingly, the money-burning game has multiple Perfect Bayesian equilibria. This makes unique quantitative predictions of the amounts burnt impossible to derive (in so far as we have been able to prove)[14]. The broad idea, of course, remains. In particular, while expecting that another subject may have been burnt by other players may have created an incentive to free ride on the other players' burning activities, this potential bias may have operated only in the direction of decreasing, not increasing, the amounts burnt.

While exact quantitative predictions cannot be derived, there are testable predictions from some models. The existence of burning, and the finding of rank egalitarianism, corroborates the idea behind models of negative interdependent preferences in general -- although not that of Bolton and Ockenfels (2000). The price insensitivity of the decision to burn is an anomaly that no current model explains. More fundamentally, desert matters. The way it matters changes according to whether the agent is advantaged or disadvantaged. We hypothesise that this is because of concerns about reciprocity. In this section we shall try to put some flesh to what this might mean.

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[14] One of the experiments described in Zizzo (2000a) addresses specifically this limitation. In this experiment, only the burning decisions of one player chosen randomly got implemented, after everyone had made their decisions. In this case a rational agent should not bother about what other people are doing in deciding what to do. The results are broadly

When we talk about how agents perceive a decision problem, we are talking about how they *categorise* it. In general, categorisation of X is how an agent represents X (Smith, 1995). In relation to interdependent behaviour, Zizzo (2000b) distinguishes three logical steps in the categorisation process that produces interdependent behaviour (or otherwise). These are a) the *perception* of the features of a decision problem; this requires and may trigger, or be triggered by, b) the *priming* (activation, becoming salient) of a set of categories, natural or social, in relation to which the perceived features of the decision problem are classified; the outcome will be c) the *activation* of interdependent preferences and production of behaviour[15].

In the case of our money burning experiment, advantaged and disadvantaged subjects may, because of the existence of the advantage, perceive the game differently. This different game perception implies that subjects prime differently two social categories, one based on desert and one on reciprocity. For disadvantaged subjects, what matters is the fact that advantaged subjects got the advantage undeservedly, and they did not. Advantaged subjects may think not only in terms of desert, but also in a different light, namely, in the light of the fact that disadvantaged subjects will burn them. They may then want to reciprocate the 'favour'[16].

The data allow us to conclude not only that the desert and reciprocity categories were primed, but also to say something about the relative importance of the two categories, at least in the context of the money burning experiment. Since the majority of the advantaged subjects was still rank egalitarian, we should conclude that desert mattered more.

Economists who have studied sequential games in the laboratory have often argued that there is a role for reciprocity. This may explain why, in sequential labour market experiments, the

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consistent with those presented in this paper.

[15] For reviews of the evidence on the role of categorisation in activating interdependent preferences, see Zizzo (1998a, 2000b).

[16] This was colourfully put in some of the responses to the final questionnaire, such as that of a subject who noted that he "had received a gift and had also won the most doblons. The result being I knew I was going to be fried by the other players…. So I fried them first (1000 each!), heh, heh, heh…."

"workers" exercise more "effort" in response to a higher wage by the "firms"[17] (e.g., Fehr et al., 1993, 1998; Fehr and Falk, 1998). In designs that allow for the possibility of negative reciprocity (retribution) after the other player has made her decision, even when retribution is not subgame perfect credible, it carries significant threat value. It is also often carried out when needed (Fehr et al., 1997).

Much of what goes under the heading of reciprocity *can* be explained with suitable models of negative interdependent preferences (e.g., the inequality aversion model of Fehr and Schmidt, 1999). This is not the case for desert. However, there is a body of evidence that suggests desert can be important -- for example from public good experiments (Van Dijk and Wilke, 1993, 1995) and questionnaire data (Will, 1993; Brockner et al., 1995).

Tyler and Degoey (1993) interviewed Californians during the 1991 water shortage. The object was to determine their willingness to restrain from excessive water consumption and otherwise follow the authority's rules to deal with the shortage. Their degree of compliance was significantly correlated with considerations of procedural fairness followed by the authority. Tyler and Lind (1992) discuss a study of dispute resolution in a federal court. The cases involved amounts between 5000 and 5 million dollars, with decisions being made (for example, whether to accept an arbitration award) that could result in transfers of ten or hundred thousand dollars. A strong role for procedural justice was found: when procedures were perceived as fair, 77% of the awards were accepted, but only 54% if they were perceived as unfair.

Brockner and Wiesenfeld (1996) review a large body of evidence in the literature (45 independently samples). They show that it is consistent not only with an important role for procedural fairness (for other reviews, see Tyler and Lind, 1988; Lynd and Tyler, 1992; Tyler and Smith, 1998), but also with the interaction between procedural fairness and relative favourability of the outcomes. If the agent perceives she has been treated unfairly (e.g., evaluation of wage fairness)

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[17] This strand of experimental work is related to the fair wage effort hypothesis of efficiency wages (Akerlof and Yellen,

or simply unfavourably (e.g., evaluation of wage satisfaction) in terms of outcomes, the fairness of the procedures leading to that outcome becomes particularly salient. This may explain why in our experiment advantaged subjects were less likely to prime desert than disadvantaged subjects.

Skarlicki and Folger (1997) investigated the relation between perceived fairness and behavioural measures of retaliation towards the firm (e.g., theft, vandalism and "medical" absences) in a sample of 240 manufacturing workers. Skarlicki and Folger found that a nested model in which each of the three dimensions of fairness (one concerning outcomes, two concerning procedures and "dignity" with which the worker is treated) are entered individually and as two-way and three-way interaction terms explains a full 68% of the variance in "effort". Omitting the three-way interaction leads to a small but significant drop in R^2 (3%). Further omitting the two-way interactions leads to a dramatic drop in R^2 - to only 39%. Desert appears to be primed in a complex way according both to outcome and procedures.

There are bargaining experiments that endorse the view that desert matters, and is not simple a matter of relative payoffs (Hoffman and Spitzer, 1985; Guth and Tietz, 1986; Schotter, 1996; Ruffle, 1998). This becomes apparent if either the desert of the proposer or that of the receiver is manipulated: the more deserving the proposer is relative to the receiver, the more inequitable the accepted splits of the cake are. For example, Hoffman et al. (1994, 1996) manipulated the desert of the proposers. The instructions for the "random entitlements" condition informed them that "a sum of $ 10 has been provisionally allocated to each pair", and the identity of the proposer was chosen randomly. In the "contest entitlements" condition, the instructions informed that the right to be "proposer" is "earned"; this time it was earned by scoring on a general knowledge quiz. The desert category was primed: proposers offered significantly less when their position was deserved than otherwise. This manipulation was stronger than the one we used in our experiment; the wording of our instructions was neutral.

1990; Zizzo, 1998b).

8. Conclusions

In this paper we present a new experimental test of negatively interdependent preferences. We construct a laboratory experiment in which the subjects earn money by betting or by the means of undeservedly assigned gifts. Subjects are then told the experiment is finishing and offered a last decision. They are anonymously allowed to eliminate other subjects' money. To do this, they have to pay a price (that is, give up some of their own cash). We vary the price from one set of subjects to another. The underlying idea of the experiment is to try to test for, and be able to parameterise the nature of, envy or concern-for-fairness.

We find evidence of significant burning. Two thirds of the subjects spend their own money to hurt other people. The price elasticity of burning is different in different experimental conditions: it varies from - 0.07 up to - 0.9. Burning does not decline much with price until we get to a price of 0.25. The decision of whether to burn appears to be insensitive to the price. There is a strong correlation between wealth, or rank, and the amounts by which subjects are burnt: a majority of the subjects, especially undeservedly disadvantaged, relatively poor people, appear to be *rank egalitarian*. Bolton and Ockenfels' (2000) model does not predict rank egalitarianism, and is therefore not consistent with our data.

We demonstrate that rank egalitarianism is driven by concerns about desert rather than simply by relative payoffs. This is particularly true for undeservedly disadvantaged subjects. For undeservedly advantaged subjects, reciprocity may also be salient; they expect to be burnt and want to reciprocate. In conclusion, in categorising the decision problem, agents appear to prime two categories, leading to negatively interdependent preferences: one is desert and the other is reciprocity. In our data, people's concerns about desert appear stronger than their reciprocity concerns.

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APPENDIX

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 convertable 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   00 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 2x50=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 2x0 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 2x100 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.

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 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 2x50=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 2x0 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 2x130 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.

STAGE 3 INSTRUCTIONS

In this stage, you are allowed to eliminate part or all of the winnings of any player - yourself included -.

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.

0.02 Marginal Price Condition:

The price is 0.02 doblons per doblon eliminated, i.e. it takes 2 doblons (or fraction thereof) to eliminate 100 doblons.

0.05 Marginal Price Condition:

The price is 0.05 doblons per doblon eliminated, i.e. it takes 5 doblons (or fraction thereof) to eliminate 100 doblons.

0.1 Marginal Price Condition:

The price is 0.1 doblons per doblon eliminated, i.e. it takes 1 doblon (or fraction thereof) to eliminate 10 doblons.

0.25 Marginal Price Condition:

The price is 0.25 doblons per doblon eliminated, i.e. it takes 1 doblon (or fraction thereof) to eliminate 4 doblons.

The total gains are the gains a player had until now, from income we gave her (including gifts) 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 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.

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.

0.02 Marginal Price Condition:

Ex. 2: Joe puts 2000 in the green cell in the player 1 row. Jim does nothing. Then Joe gets 980 doblons (for he pays 0.02x1000=20) 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 Jane's, and both Jim and Jane do nothing, then Jim is left with 1500 doblons, Jane with 100 doblons and Joe with 988 (for he pays 0.02x900=18).

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. Then Jim's balance is 2000 (initial total gains) - 15 (price for activity: 0.02x750) - 1000 (eliminated by Joe) - 1000 (eliminated by Jane) = -15, hence 0 since a negative balance is not allowed. Joe's balance is 1000 - 20 (since 0.02x1000=20) - 500 (eliminated by Jim) = 480 doblons. Jane's balance is 500 - 20 (since 0.02x1000=20) - 250 (eliminated by Jim) = 230 doblons.

0.05 Marginal Price Condition:

Ex. 2: Joe puts 2000 in the green cell in the player 1 row. Jim does nothing. Then Joe gets 950 doblons (for he pays 0.05x1000=50) 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 Jane's, and both Jim and Jane do nothing, then Jim is left with 1500 doblons, Jane with 100 doblons and Joe with 955 (for he pays 0.05x900=45).

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. Then Jim's balance is 2000 (initial total gains) - 37.5 (price for activity: 0.05x750) - 1000 (eliminated by Joe) - 1000 (eliminated by Jane) = -37.5, hence 0 since a negative balance is not allowed. Joe's balance is 1000 - 50 (since 0.05x1000=50) - 500 (eliminated by Jim) = 450 doblons. Jane's balance is 500 - 50 (since 0.05x1000=50) - 250 (eliminated by Jim) = 200 doblons.

0.1 Marginal Price Condition:

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 0.1x1000=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 Jane's, and both Jim and Jane do nothing, then Jim is left with 1500 doblons, Jane with 100 doblons and Joe with 910 (for he pays 0.1x900=90).

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. Then Jim's balance is 2000 (initial total gains) - 75 (price for activity: 0.1x750) - 1000 (eliminated by Joe) - 1000 (eliminated by Jane) = -75, hence 0 since a negative balance is not allowed. Joe's balance is 1000 - 100 (since 0.1x1000=50) - 500 (eliminated by Jim) = 400 doblons. Jane's balance is 500 - 100 (since 0.1x1000=50) - 250 (eliminated by Jim) = 150 doblons.

0.25 Marginal Price Condition:

Ex. 2: Joe puts 2000 in the green cell in the player 1 row. Jim does nothing. Then Joe gets 750 doblons (for he pays 0.25x1000=250) 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 Jane's, and both Jim and Jane do nothing, then Jim is left with 1500 doblons, Jane with 100 doblons and Joe with 775 (for he pays 0.25x900=225).

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. Then Jim's balance is 2000 (initial total gains) - 187.5 (price for activity: 0.25x750) - 1000 (eliminated by Joe) - 1000 (eliminated by Jane) = -187.5, hence 0 since a negative balance is not allowed. Joe's balance is 1000 - 250 (since 0.25x1000=250) - 500 (eliminated by Jim) = 250 doblons. Jane's balance is 500 - 250 (since 0.25x1000=250) - 250 (eliminated by Jim) = 0 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.

STAGE 4 INSTRUCTIONS

Please fill in the provided questionnaire.

Your final gains after the activities of all players are (*number of doblons earned by the player)* doblons, and these have been converted into UK pounds at the rate of 0.6 pence per doblon. (For payment, fractions of pence will be rounded to the nearest penny).

In addition, you are receiving a payment token of 3 pounds for your kind participation to this experiment.

We are very sorry if this amount is less than you expected: if this is the case, the activity of elimination of doblons by other players is to be blamed for this unfortunate outcome.

Whatever the outcome, we acknowledge the time and effort you spent in kindly accepting to participate to this experiment, and this is the spirit in which you are paid the participation token.

IMPORTANT: 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.

In receiving your payment, you will be asked to return the filled questionnaire, and to sign a receipt and a pledge of silence on the content of the experiment.

QUESTIONNAIRE

Please answer the following questions. Ask for clarification if you have some query about them. Use the back of this page if the space provided here is not enough, and to write any other eventual comment or feedback on the experiment. Thanks!!!

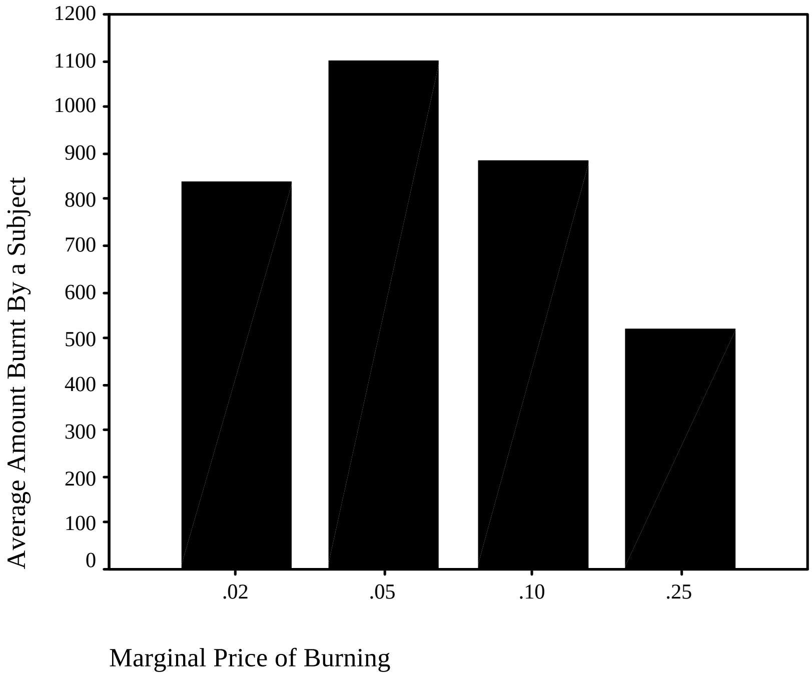
1) What motivated your choices in last stage of the experiment?

2) At the end of the last stage of the experiment, did you expect to earn more, less or as much as you actually did?

3) What do you think the choices of the other players were motivated from?4) If you were to repeat the experiment, do you think that you would make approximately the same choices? If not, why not?

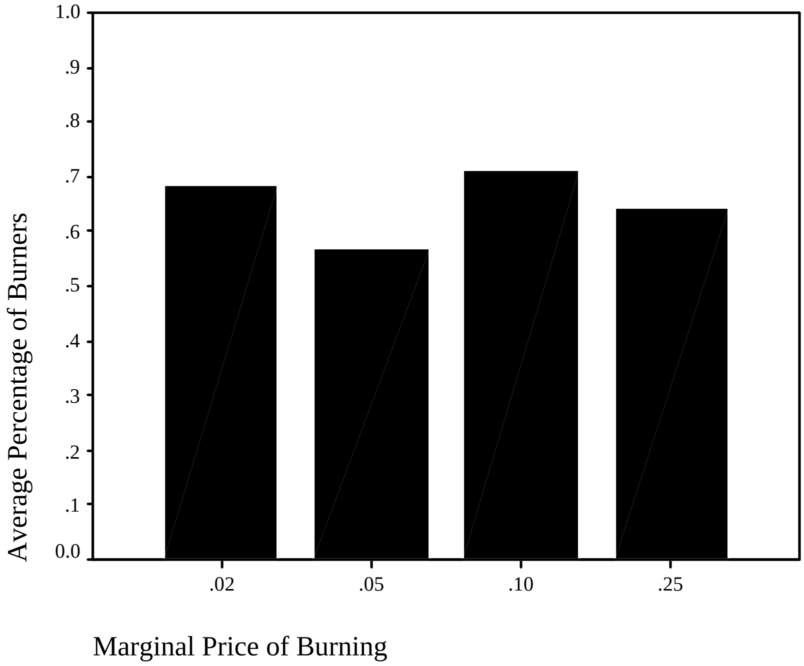
(Space to write was provided between the questions).

FIGURE 1. Amounts Burnt for Each Marginal Price



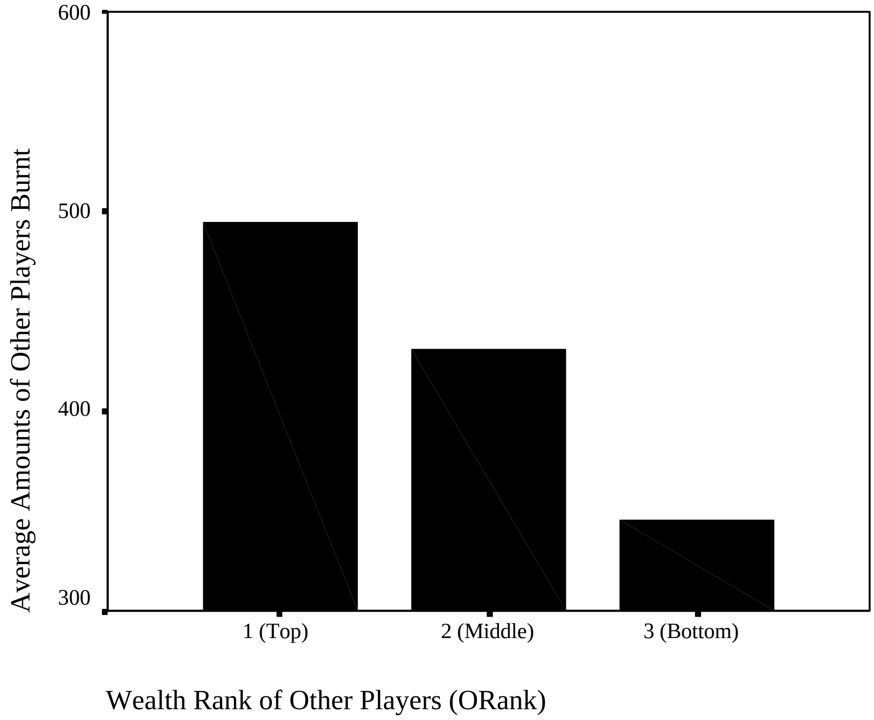
The figure plots how much subjects burn on average in each experimental condition, i.e. for different marginal prices of burning. A marginal price of burning of, say, 0.25, means that a subject had to spend one experimental currency unit (the "doblon") for every currency unit burnt.

FIGURE 2. Percentage of Burners for Each Marginal Price



The figure represents the fraction of burners for every experimental condition, i.e. for each marginal price of burning.

FIGURE 3. Burning According to the Wealth of the Other Players



The figure represents the average amounts that a subject burns of the wealthiest among the other players (1), the second wealthiest (2), and the poorest (3).

TABLE 1. Multinomial Tobit Regression on BurnAdv and Burn Disadv

	Disadvantaged Subjects Dependent Variable: BurnDisadv			Advantaged Subjects Dependent Variable: Burn Adv		
Explanatory Variables	**Coef.**	**S.E.**	**Prob.**	**Coef.**	**S.E.**	**Prob.**
Voice	-466.648	261.027	0.076	251.03	366.167	0.494
Voice1998	525.177	254.632	0.041	219.377	352.913	0.535
Price	-945.689	824.077	0.253	-1849.234	1216.289	0.131
RandomlyEarned	0.649	0.255	0.012	0.822	0.357	0.023
UndeservedlyEarned	1.024	0.187	0	0.419	0.254	0.101
Constant	-780.664	319.379	0.016	-611.604	430.078	0.157

These regressions investigate the determinants of the burning by advantaged subjects (BurnAdv) and by disadvantaged subjects (BurnDisadv). For any advantaged subject x, we define two observations for the variable BurnAdv: the first is the *sum* of the amounts burnt from disadvantaged subjects; the second is *double* the amount burnt from the other advantaged subject. The need to scale differently the two burning activities arises from the fact that, for every x, there is only one other advantaged subject that she can burn, while there are two disadvantaged subjects We can define similarly BurnDisadv if x is a disadvantaged subject. In this case, we compute two datapoints: the first is the *sum* of the amounts burnt this time from advantaged subjects; the second is *double* the amount burnt from the other disadvantaged subject. Since for each subject there are two observations and there are 68 advantaged and 68 disadvantaged subjects (in each case, 58 in the 1998 sessions, 10 in the 1999 sessions), the sample size n=136 for each regression.