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Abstract: The literature on dictator [D] and joy-of-destruction [JoD] games demonstrates that people can be nice and nasty. We study, by way of an experiment with between-subjects and within-subjects features, to what extent behaviors are context dependent and consistent. We find that, for one-shot D and JoD games, our participants' niceness and nastiness depend on the choice set. Contradicting the observed altruism and nastiness, participants tend to be selfish but nonetheless make choices that increase social welfare when given the opportunity.

Keywords: Dictator game · Joy-of-Destruction game · Altruism · Nastiness · Efficiency considerations · Mach-IV test
JEL Classification A13 · C79· D03· D64

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1. Introduction

Initial results from Dictator (D), Ultimatum, and Trust games were widely interpreted as people being more altruistic than economic theory had traditionally assumed (see Camerer 2003) . Since then, numerous new studies have demonstrated strikingly the dependence of dictator game experimental outcomes on various design and implementation characteristics (see, for example, Dana, Cain, and Dawes 2006; Dana, Weber, and Kuang 2007) including the choice set (see, for example, List 2007; Bardsley 2008; Andreoni and Bernheim 2009; Lazear, Malmendier, and Weber 2011; Cappelen et al. 2012; Krupka and Weber 2012). Andreoni and Miller (2002) have, in addition, demonstrate that altruism is price sensitive, a finding unlikely to surprise economists but one that contradicts the concept of a "primitive" (Berg et al. 1995; Ortmann et al. 2000) and widely cherished beliefs.

More recently, an emerging literature on Joy-of-Destruction (JoD) games (which, as we will show below, are in interesting ways related to the take-options that have been added to recent D game experiments) demonstrates that some people can be quite nasty in that they are willing to reduce other participants' endowments though they do not benefit directly or might even have to pay for it (Abbink and Sadrieh 2009; Abbink and Herrmann 2011). The results in List (2007) suggest that nastiness (Take $1 and Take $5 options), to the extent that moral scruples tend to be overcome when it is too expensive to stick to them, is also price-sensitive.

To the best of our knowledge, our study is the first to investigate people's pro-social and anti-social behavior by letting participants make decisions in both D and JoD "game" scenarios with different choice sets[1]. We are not aware of any theory that speaks directly to the issues that we are interested in. Our study is empirical and focuses on how people behave in both games (the within-subjects component of our design) and across the treatments (the between-subjects component of our design). Our study also links participants' choices in both D games and JoD games with the well-known Mach-IV test and some basic demographic controls.

The dependence of people's behaviors on the choice set that we demonstrate both for the D game and the JoD game makes it quite difficult to formulate an encompassing model, although the recent work by Krupka and Weber (2012) on the systematic effects of social norms on choice behaviors -- effects seemingly also reflected in our results here -- suggest a way forward.

The remainder of this study is organized as follows. In Section 2 and 3 we provide the experimental design and implementation. In Section 4 we summarize the results and provide a detailed data analysis, followed by a discussion of relevant papers in Section 5. Section 6 contains our conclusion (including plans for further work).

[1] Zizzo and Fleming (2011), which we became aware only after we circulated an initial draft of our manuscript, and in which the authors try to determine to what extent social desirability (aka experimenter demand effects) might influence behaviors in public good contribution games. They ask participants, as part of a complex design, to make decisions as dictators in both D games and costly JoD games (which they call money burning games). There are numerous differences between our studies, from the specifics of the game, to the action spaces (the authors do not allow negative action spaces), and the subject pool selection (the authors try to screen their subjects for English language proficiency). The same is true for a recent working paper by Sadrieh and Schröder (2012) of which we became aware of even later.

2. Experimental Design

We implemented three treatments called "Baseline", "Treatment$1", and "Treatment$5". Within each treatment, our participants made decisions in D and JoD "game" scenarios[2] specific to the treatment; D game and JoD game decisions were denoted as Decision 1 and Decision 2 in the instructions (and the opposite when the order was reversed). For each treatment, the order of the two decisions was counter-balanced in order to control for order effects. The three treatments together constitute a between-subjects design, separately for the D game and the JoD game. In order to have some indication that our randomization (recruiting) was done properly, we controlled for standard demographic characteristics. In order to understand the effects of social distance, we also ran a double-blind version of the third treatment. Following List (2007), we did not use asset legitimacy[3] (see Cherry et al. 2002 and the earnings robustness check in List 2007).

Each treatment had two independent decision situations, one based on a version of the D and the other based on a version of the JoD. In each of the two decision situations, all participants were told that they have, in addition to the show-up fee of $5, an initial $5 endowment. Furthermore, they were told that they would be endowed with yet another $5 as decision makers. This information was read aloud, so can be assumed to have been common knowledge. It was also common knowledge that one of the two decisions made during a treatment would be randomly selected as payoff-relevant and that each participant, in addition, would be at the receiving end of another participant's randomly selected payoff-relevant decision[4]. After all participants, without knowing other participants' decisions, selected their pay-off relevant decision by tossing a coin, we asked them to answer a questionnaire including a Mach-IV test[5] and other demographic questions.

The experiment consisted of three treatments, plus a robustness check of Treatment$5, in which we explored the effect of increasing social distance. Participants had the same information and the same endowments across treatments. The only treatment difference is the different ranges of choice-set across treatments (see Fig. 1 for the pictorial description). For the D decisions, our treatments replicate the key treatments in List (2007). In the following, P

[2] Both games do not really deserve the label, as they are not truly interactive; specifically, the second mover (who is in effect a non-mover, or recipient rather than a responder) is always at the mercy of the first mover.

[3] Asset legitimacy means that endowments and earnings are not "manna from heaven" provided by the experimenter, as is typically the case in economics experiments. For there to be asset legitimacy, at least part of the endowments has to be earned. The term is used in Cherry et al. (2002).

[4] We were aware that this might induce indirect-reciprocity confounds (that, in addition, might differ across treatments and, in any case, might compromise to some extent the independence of decisions.) We chose our design in the hope, also based on pilots, that these confounds would not matter. Our results (namely, order effects about which more below) suggest this hope was somewhat optimistic. A cleaner design would have been to always let half of the participants in a treatment act out the role of dictator and the other half that of recipient; we did not use some such design as it would have made some recipients, especially those whose $5 were destroyed by others, quite unhappy. An even cleaner design would have been to match each of the three D scenarios with each of the JoD scenarios. Obviously, some such design (apart from various other problems) would have been an undertaking manifold more expensive. We consider it unlikely that it would have led to significantly different insights.

[5] The Mach-IV test is a close relative of the agreeableness dimension in the Big Five test, r=0.47 (Paulhus and Williams 2002). We chose the Mach-IV test as it seemed more relevant to our study and previously has been used in economics (see discussions below).

denotes participant. P1 is the decision maker (or dictator) and P2 is the other participant (recipient).

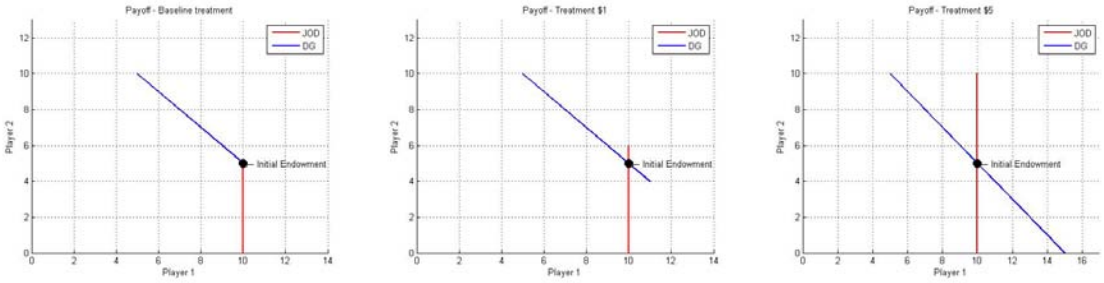

Fig. 1 Pictorial description of the three treatments[6]

Baseline Treatment. As in List (2007), both games start from the same initial ($10, $5) endowment allocation (not counting the participants' show-up fees). The participant who is endowed with the $10 is the decision-maker. In the D game, the decision maker (P1) could give, in integer steps, up to $5 of their additional endowment to the anonymous other participant (P2), hence the final outcome is one of the outcomes that lie on the line connecting (10, 5) to (5, 10). Similarly, in the JoD game, the decision maker (P1) could destroy, in integer steps, up to all of Participant 2's $5 endowment, thus the final outcome is one of the integer outcomes that lie on a vertical line starting from ($10, 5) to ($10, 0).

Treatment$1. This treatment is similar to the Baseline treatment; the only difference is that the action space is larger. In the D game, the decision maker can give up to $5 or take $1 from another participant. In the JoD game, the decision maker can decrease the endowment of another participant or add $1 to that participant's endowment. Hence, the final outcome could be one of the integer outcomes and from (11, 4) to (5, 10) in the D game and from (10, 6) to (10, 0) in the JoD game.

Treatment$5. This treatment is similar to Treatment$1, the only difference is that the action space is even larger. In the D game, the decision maker can now give up to $5 or take up to $5 from another participant. In the JoD game, the decision maker can decrease the endowment of another participant or add up to $5 to that participant's endowment. Hence, the final outcome could be one of the integer outcomes from (15, 0) to (5, 10) in the D game and from (10, 10) to (10, 0) in the JoD game.

Note that, although Fig. 1 might suggest that the decisions in D games and JoD games are symmetric around the initial endowment point, they are not in one important respect. In the D game treatments, the give- and take- options imply a transfer from one participant to the other, and hence have no welfare consequences (the aggregate amount available to participants remains unchanged). In the JoD game, in contrast, the destruction options as well as the options to have money added, have welfare consequences. Specifically, the options of having money added increase the welfare of the participants[7]. In other words, the destruction

[6] Since participants knew that the $5 show-up fee would be independent from decisions taken by themselves or others, hence Fig 1 excludes the show-up fee.

[7] However, it invites another form of destruction – that of the endowment of the experimenter -- although it seems unlikely that subjects think about it this way.

options and options of having money added have efficiency consequences. Correspondingly, the destruction options entail a reduction in social welfare.

We are interested in treatment effects (differences across treatments, the between-subjects component of our design), as well as participants' behaviors within each treatment – i.e., whether subjects would make inconsistent decisions (the within-subjects component of our design).

2.1 Across treatments

In line with List (2007), we expect H_1 (the percentage of givers in D games does not change across treatments), to be rejected. The rationale for the hypothesis is the assumption of stable preferences which predicts that, in the aggregate, the percentage of tangency points of indifference curves on the giving segment ought not to be affected by the addition of a take-option, be it \$1 or \$5. Under the assumption of stable preferences, the take-options are expected to differentiate between those that are constrained not to give money in the Baseline condition.

The rationale is similar for H_2 (the percentage of destroyers in JoD games does not change across treatments).

2.2 Within each treatment

Since our participants make decisions in both the D and the JoD game, we could theoretically categorize participants' types; we provide indeed some such typology in Appendix III. The typology is constructed under the assumption that our subjects have stable preferences across both decision situations and that choices would directly reveal type. Since we do find some order effects (about which more below), this assumption turns out to be not completely innocent and we will hence focus in our results section on those participants that made inconsistent choices.

Since there is considerable evidence on types being a function of demographic characteristics as well as personality traits (see, for example, Wilson, Near, and Miller 1996 ; Athanasakopoulos and Letzler 2006), we used a well-known and validated assessment instrument, the Mach-IV test, to try to shed light on the determinants of participants' Machiavellian instincts. Christie and Geis (1970) design the first Mach test as an assessment instrument for personality traits. The Mach-IV test -- see Appendix II for the battery of questions used -- has been widely applied and cited both in economics and in psychology. It seems well established that high-Mach scorers steal more and are more likely to exploit a trusting supervisor (Harrell and Hartnagel 1976), more likely to behave unethically (Jones and Kavanagh 1996) and more self-interested, more suspicious, and are less concerned for ethics or others (Mudrack and Mason 1995). Wilson et al. (1996) synthesize the evolutionary and psychological literature on Machiavellianism. They show that people with low Mach scores ("low-Machs") are more altruistic, trustworthy, group oriented, empathetic, willing to blame individuals for inappropriate actions and they expect more trust in return.

Athanasakopoulos and Letzler (2006) argue that "there are striking similarities between economic literature's description of free-riding (cooperative) types and psychology literature's description of high (low) Machs." They review the relevant literature on

ultimatum and trust games and show experimentally – for Voluntary Contribution games – that the Mach-IV test is a useful way to classify subjects and to predict the outcomes of public good provision games. By comparing the behaviors of high- and low- Machs in a public goods experiment, Athanasakopoulos and Letzler (2006) provide evidence that high-Machs free ride and break norms more frequently than low-Machs. In contrast, low-Machs are more likely to conform to norms than high-Machs.

3 Experimental Implementation

All experimental sessions took place in the ASB lab of the University of New South Wales in September of 2012; pilot sessions were conducted in August 2012.

Because directly relevant precedent studies did not exist, we computed the optimal sample size for our experiment from the results of Fisher's exact tests in List (2007): approximately 24 per treatment were required for aspiration levels of 95% confidence and 80% power.

We recruited 48 participants for each treatment (24 participants in each session and two sessions for each treatment, with counter-balanced order in the second session, to control for order effects of D and JoD scenarios that are concerns in designs with within-subjects components)[8]. In total, we had 167 participants (7 sessions including one session for the double-blind treatment). Average earnings were (including $5 show-up fee) over $20 per person. The experiment lasted less than one hour.

In each of the treatments, it was common knowledge that every participant would be paid a standard show-up fee of $5 plus other earnings from the experiment.

Each treatment had two independent decision situations, one based on a version of the D and the other based on a version of JoD. In each of the two decision situations, all participants were told that they have, in addition to the show-up fee of $5, an initial $5 endowment. Furthermore, they were told that, as the decision maker, they would be endowed with yet another $5. They were also told that one of the two decisions made during a treatment would be randomly selected as payoff-relevant and that each participant, in addition, would be at the receiving end of another participant's randomly selected payoff-relevant decision (Whether that payment would come from a D game or a JoD game was not specified in the instructions and was never asked as a question, see Appendix I for details.).

Once all decisions were made, each participant flipped a coin to identify the pay-off relevant game for them, hence they had no reason to doubt that the pay-relevant decision was indeed randomly drawn; the participant at the receiving end was randomly selected (under a random-permutation protocol[9]) from all participants in the same session and anonymously. After all participants selected their pay-off relevant decisions without knowing other participants' decisions, we asked them to answer a post-experiment survey, which was composed of Mach-IV test and several general demographic questions (like gender, age, major etc.). The Mach-IV test (see Appendix II for details) includes 20 statements which participants are asked to rate on a 5 – point Likert scale ranging from strongly disagree to

[8] There were only 47 subjects in the Baseline treatment as some students did not show up in time; overall we had 143 (1x47, 2x48) participants for the three treatments plus 24 more participants for the double-blind version of the third treatment (Treatment$5) to maximize social distance.

[9] They were randomly selected but without replacement in the sense that two people would not be matched twice and a participant could never be a recipient of her- or himself.

strongly agree. Following the standard procedure for the administration of the Mach-IV test, participants were not incentivized to answer it. We added a "catch question"[10] to gauge whether subjects paid attention to the questions. The Mach-IV test was implemented through Qualtrics. Each page featured one of the questions and participants could not go back to previous questions. A time delay (12 seconds), about which participants were informed, was implemented for each question to discourage participants from rushing through the questions.

The experimental sessions were conducted by a non-traditional PhD student, who was not involved in this study. She knew about the study only what she could infer from the instructions. The first author was "quarter-backing" the sessions in that she ran the computerized programs and assisted where necessary (payoff of participants).

4 Results

4.1 Summary statistics and non-parametric tests

4.1.1 Giving Decisions

Table 1 Aggregate giving behaviors

Giving	Obs	Rate of positive offers	Mean	Median	Std. Dev.	Average positive offer
Baseline	47	0.64	1.28	1.00	1.30	0.40
Treatment $1	48	0.35	0.33	0.00	1.48	0.39
Treatment $5	48	0.15	-2.48	-4.00	2.92	0.51

Table 1 summarizes the results of giving decisions across three treatments. The results from the Baseline treatment are similar to results from other Dictator game experiments: 64 percent of the participants (30 out of 47) gave money to others, and average giving was around 20% of the endowment (see Camerer 2003). When the $1 take-option was added to their choice set, only 17 out of 48 participants (35 percent) gave money to others, and average giving was close to zero. When the options of taking up to $5 were added, only 7 out of 48 participants (15 percent) gave money to others, and average giving dramatically decreased to -$2.48. The effect on the median offer is even more dramatic. Hence, giving decisions are clearly context dependent: increasingly fewer people gave money and more people took money from others. Interestingly, when a participant give her or his money to others, the context does not affect how much they gave, as evidenced by the average positive offer[11] which does not change much across treatments: 0.40, 0.39 and 0.51. The shift of the distribution of giving is illustrated by Fig 2. Our results closely replicate the results in List (2007).

[10] The "catch question" is: "A person asks you whether you are studying in the University of Sydney. Please choose 'strongly disagree' as you are studying in the University of New South Wales now".

[11] Following List (2007), we define the average positive offer as the percentage of giving (or destruction) of the total available amount which ignores zero and negative offers (average amount of positive offers divided by $5).

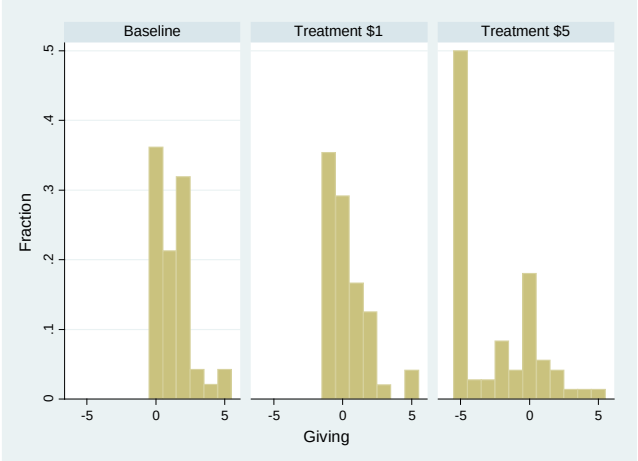

Fig. 2 Histogram of giving decisions across treatments

We use Fisher's exact tests[12] to investigate more formally the proportion of givers across treatments. Our null hypothesis for giving decisions is that the percentage of givers in D games does not change across treatments (H_1). The differences in the percentages of givers across three treatments are strongly statistically significant (three treatments[13]), thus we reject formally the hypothesis that the proportion of people giving positive amounts is the same across treatments[14]. Significantly fewer people gave positive amounts when take-options were allowed.

4.1.2 Destruction Decisions

Table 2 Aggregate destruction behaviors

Destroy	Obs	Rate of positive offers	Mean	Median	Std. Dev.	Average positive offer
Baseline	47	0.28	0.60	0.00	1.26	0.31
Treatment $1	48	0.15	-0.29	-1.00	1.09	0.34
Treatment $5	48	0.17	-2.15	-4.00	3.46	0.83

Table 2 summarizes the results of destruction decisions across the three treatments. Similar to their giving decisions, participants' destruction decisions differ across treatments: 13 out of 47 participants (28 percent) destroyed others' endowments, which is similar to the results in Abbink and Herrman (2011). However, when participants had a chance to add money to others' endowments (and hence increased overall welfare), fewer participants behaved in a destructive manner (7 out of 48 and 8 out 48 participants in Treatment$1 and Treatment$5, respectively). In fact, the majority of participants added money to others in Treatment$5, with

[12] We also run Wilcoxon Mann-Whitney tests (rank-sum tests) to test the distribution of giving decisions across treatments. The results demonstrate strong effects (both economically and statistically significant) across treatments (at the 5% significance level) as shown in Fig 2. All the results from Fisher's exact tests are consistent with those from unconditional Fisher-Boschloo tests which arguably are more appropriate. We report the Fisher's exact tests since they are widely used and understood.

[13] Treatment$5 with double-blind is not included in our discussion of treatment effects.

[14] P-values are 0.005, 0.016 and 0.000 for differences between Baseline and Treatment$1, Treatment$1and Treatment$5, Baseline and Treatment$5 respectively.

the mean destruction amount decreasing to -$2.15 and more than 40 percent having the full amount added. We conclude that destruction decisions are also context dependent: fewer participants destroyed money and increasingly more participants added money to others' endowments when efficiency gains could be captured. However, conditional on being nasty, there is some variation in the proportion of positive destruction (and in fact an increase in the amount of destruction in Treatment$5, which will be discussed in Section 4). The shift of the distribution of destruction is illustrated by Fig 3.

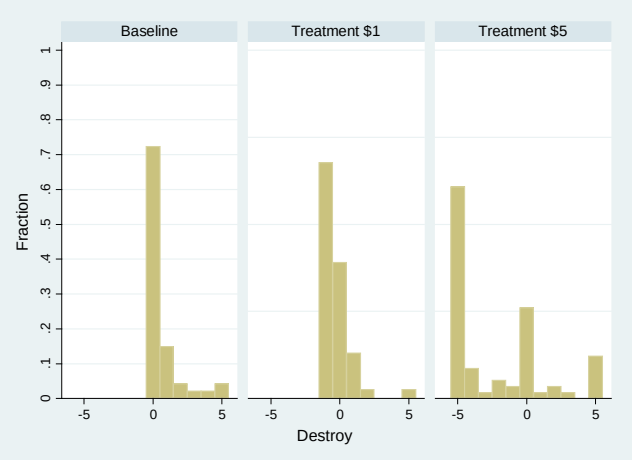

Fig. 3 Histogram of destruction decisions across treatments

We use Fisher's exact tests[15] to investigate more formally the proportion of "destroyers" across JoD treatments. Our null hypothesis for destruction decisions is that the percentage of destroyers in JoD scenarios does not change across treatments (H_2). We find that the differences in destruction decisions are only weakly statistically significant from each other[16]. Fewer participants destroyed other participants' endowments (0.28, 0.15, and 0.17 going from Baseline to Treatment$5, respectively) when adding-options were allowed. The one-dollar adding option dramatically decreases the probability of destruction.

4.2 The Effects of Mach-IV Scores and Demographics on Giving/Destruction Decisions (OLS Regressions[17])

We use a linear model to examine the effects of demographics on personality traits (Mach-IV scores). Departing from much of the literature, we did not find any effect of the standard demographic factors that we collected on participants' Mach-IV scores: no gender, age or culture effect (see Table 2). This may be due to the subject pool being quite homogeneous: all

[15] Wilcoxon Mann-Whitney tests (rank-sum tests) are implemented to test the distribution of destruction decisions across treatments. The results demonstrate strong effects (both economically and statistically significant) across treatments (at 5% significance level) as shown in Fig 3: participants destroyed less from Baseline treatment, to Treatment$1 and Treatment$5.

[16] P-values are 0.095, 0.50 and 0.148 for differences between Baseline and Treatment$1, Treatment$1and Treatment$5, Baseline and Treatment$5 respectively.

[17] Since the choice set varies across treatments (censored at different lower limit), we also use a Tobit model for the corner solution outcomes) separately for different treatments. The results are robust under Tobit models.

participants are students from the University of New South Wales and their ages cluster around 22.

We also examine participants' Mach-IV scores, treatment dummies and other demographic characteristics on giving decisions by a linear model (see results in Table 3):

$$Giving\ (Destruction)amount_i = \alpha + \beta X_i + \gamma_1 Treatment\$1 + \gamma_2 Treatment\$5 + \gamma_3 JoD\ first + \gamma_4 Treatment\$1 * JoD\ first + \gamma_5 Treatment\$5 * JoD\ first$$

Table 3 Giving/destruction decisions

	OLS regressions			Probit		Truncated	
	Mach-IV Score	Giving	Destruction	Whether to Give	Whether to Destroy	Giving	Destruction
Mach-IV Score		-0.07**	0.00	-0.01+	0.00	-0.03	0.11*
		(0.02)	(0.03)	(0.01)	(0.00)	(0.03)	(0.05)
Male	1.50	0.18	-0.54	0.08	-0.05	-0.28	-2.08**
	(1.12)	(0.36)	(0.39)	(0.09)	(0.07)	(0.36)	(0.80)
Age	0.05	-0.03	-0.05	-0.00	-0.01	0.01	0.19
	(0.21)	(0.08)	(0.06)	(0.02)	(0.01)	(0.09)	(0.16)
Australia	-0.14	0.36	-0.41	-0.03	-0.09	-0.04	-1.91
	(1.96)	(0.54)	(0.50)	(0.14)	(0.12)	(0.85)	(1.68)
Years	-0.07	0.02	-0.01	0.01	0.01	0.00	0.02
	(0.12)	(0.05)	(0.03)	(0.01)	(0.01)	(0.05)	(0.11)
Allowance	0.01	0.00	0.00	0.00	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Careless	-1.34	0.80+	0.25	0.27*	0.08	0.11	0.86
	(1.43)	(0.42)	(0.50)	(0.11)	(0.10)	(0.42)	(0.72)
Double-blind	-0.81	-1.37	-1.94+	-0.17	-0.14+	-0.69	-0.11
	(1.81)	(0.83)	(1.05)	(0.13)	(0.08)	(0.68)	(0.67)
Developed	-0.38	-0.32	-0.10	-0.15	-0.12	0.88	1.88*
	(1.26)	(0.40)	(0.41)	(0.10)	(0.09)	(0.63)	(0.76)
Treatment $1	-1.50	-1.53***	-0.91+	-0.38***	-0.20	-1.23+	3.84**
	(1.80)	(0.42)	(0.47)	(0.09)	(0.12)	(0.67)	(1.25)
Treatment $5	-0.01	-3.30***	-0.96	-0.51***	0.10	-0.19	-0.08
	(1.88)	(0.70)	(0.82)	(0.11)	(0.12)	(0.66)	(1.13)
JoD first	-0.23	-1.11**	0.39	-0.32*	0.20	-1.26*	-1.44+
	(1.87)	(0.41)	(0.42)	(0.13)	(0.13)	(0.51)	(0.84)
Treatment $1*JoD first	1.62	1.23*	-0.28	0.41*	0.08	2.19*	-6.08***
	(2.67)	(0.58)	(0.57)	(0.20)	(0.22)	(1.06)	(1.60)
Treatment $5*JoD first	0.93	-0.57	-3.84***	-0.01		3.52***	
	(2.65)	(0.93)	(0.92)	(0.24)		(1.05)	
_cons	56.59***	5.95*	1.93				
	(4.71)	(2.30)	(1.90)				
N	166	166	166	166	142	57	31
(Pseudo) R-sq	0.065	0.493	0.372	0.254	0.129		

Notes: Marginal effects are reported for Probit model; *** p< 0.001; ** p< 0.01; * p< 0.05.

Participants who have higher Mach-IV scores gave less, even though the effect size is small[18]. Our result is consistent with the result of Jones and Kavanagh (1996), which shows that high-Machs are more apt than low-Machs to behave unethically. High-Machs are also

[18] However, the coefficient only means the difference in giving amount when the Mach-IV score is increased by one unit. Hence, the giving decisions might be very different if participants' Mach-IV scores are very different.

less likely to be affected by social pressure than low-Machs. There is no gender effect or culture effect on giving. Participants who incorrectly answered our catch-question also gave more, though not at a statistically significant level. However, except for treatment effects, none of these factors have a statistically significant effect on destruction decisions.

The treatment effects for giving amount and destruction amount remain economically and statistically significant after controlling for the demographic factors. When also controlling for the order in which decisions were made, giving amount and destruction amount become statistically and economically even more significant when D and JoD decisions are elicited first, respectively. The treatment effects for giving amount remain statistically significant (albeit at a lower level of significance) when D decisions are elicited after JoD decisions. The treatment effects for destruction amounts, in contrast, turn insignificant when JoD decisions are elicited after D decisions. These results indicate that our data are to some extent afflicted by order effects.

An interesting result is that, in Treatment$5 when D decisions are elicited first, participants continued to destroy at a comparatively high rate even after they took money from others. We can think of two possible explanations: firstly, randomization was not perfect and those participants were very nasty. We can rule out that explanation as we do not find a difference in Mach-IV scores. Secondly, participants enjoyed the power of destroying other participants' endowment even though they could not benefit from it. Why did not they destroy as much when JoD was elicited first? Even though it was made clearly in the instruction that two decisions are independent, we conjecture that this finding is due to the uncertain nature of the second decision. Specifically, we conjecture that (some of) our participants might have thought that the second decision could entail some form of retaliation (e.g., Balafoutas and Nikiforakis 2012). As a matter of fact, none of the 24 participants engages in destruction activity when JoD decisions are elicited first, indicating that that fear must have been considerable.

4.3 The Treatment effects on Giving/Destruction Decisions (hurdle model)

Above we use non-parametric (Fisher's exact) tests for the proportion of participants who give or destroy a positive amount, now we separate the decisions of whether to give/destroy from the decisions of how much to give/destroy[19] (see Table 3).

The results from Probit regressions[20] confirm the findings from Fisher's exact test: less participants gave money and destroyed others' endowment in Treatment$5. In line with the order effects found in the OLS regressions, participants made nastier choices in the second game: less participants gave money when JoD was played first; more participants destroyed money when D was played first.

Conditional on participants' willingness to destroy, higher Mach-IVs will statistically destroy more. Participants destroyed more money in Treatment $1, but destroyed dramatically less when JoD decisions were elicited first.

[19] The negative observations are ignored.

[20] 24 Observations are missed because the Treatment$5*JoD first perfectly predict no destruction behavior (nobody destroyed others' money when JoD is played first). Hence the treatment effect cannot be reflected in the regression.

4.4 Inconsistent choices within each treatment

In our experiment, dictators made decisions in both D and JoD games. We find considerable heterogeneity in types of participants' choices (see Appendix III). We find that relatively few of our subjects make choices that are clearly inconsistent (both altruistic and nasty) when we enlarge participants' choice set (both giving/destroying and taking/adding options are allowed in Treatment$1 and Treatment$5).

In the Baseline treatment, more than 20 percent of participants made "positive choices" in both situations (they gave money to another participant and also destroyed another participant's endowments). The proportion of people belonging to this type (T3 in Appendix III) decreases when "negative decisions" are allowed (take-options as negative giving and options of having money added as negative destruction). Only 10 percent and 4 percent are of T3-type in Treatment$1 and Treatment$5, respectively.

5 Discussion

The literature on asset legitimacy (see, for example, Cherry et al. 2002 and also the Earnings treatment in List 2007) suggests that our results would be affected by it; essentially the effects would be reduced and we would see less giving (and destruction). Asset legitimacy could, for example, induce a feeling of entitlement towards the endowments that could be given or taken (the relevant reference point, to be discussed below, when we talk about the consequences of normalization). Interestingly, how exactly entitlements are implemented is important. Cherry et al. (2002) find a powerful effect on giving when dictators earn their endowments[21]. Cappelen et al (2012)[22] essentially replicate List (2007), but add asset legitimacy for the initial endowments through a production phase and find that two-sided asset-legitimacy does not make a difference.

Contradicting the assumption of stable preferences but confirming earlier results reported in List (2007) on giving, we demonstrate that the distributions of giving and destruction amount dramatically shift to the left, with the proportion of participants that give positive amounts decreasing significantly when negative choices are allowed; the proportion of participants who destroy money likewise decreases although the shift is statistically insignificant.

The two simple vehicles used in our study could also be used to explain the level of altruism and spitefulness (Kimbrough and Reiss 2012). The advantage of our study over their auction set-up and other games (such as ultimatum games and public goods games) is that the two games in our study are quite simple; hence participants can express their decision clearly and easily without having to think about others' strategies.

Since a considerable fraction of our participants is swayed by efficiency considerations (a result well established in the literature, see Engelmann and Strobel 2004), it is natural to ask to what extent inequality aversion might have affected our results. We believe this concern to be moot. We have two sessions with decisions in different orders for each treatment. Participants might add money because of inequality aversion, but the inequality aversion

[21] But both dictators and recipients get $5 show-up fee. List (2007) uses the same implementation.

[22] High giving in the work treatment in this paper is not surprising as half of the extra endowments given to dictators have in fact been earned by recipients.

could not explain the fact that they took money from others in the later D games which increased the inequality (participant 1 received higher amount). Also, without knowing the other decision scenario, when D decisions were elicited first, participants apparently took money from others and thus increased the inequality[23]. Thus inequality aversion is not a likely rationale for the addition of money that we observe, while social welfare (efficiency) is.

We also find order effects of two decisions. We see less taking when D decisions are elicited first and less destruction when JoD game is played first. It seems that being prompted to answer the JoD decision first makes subjects aware of the fact that (if they are at the receiving end) they might have some of their endowments taken or destroyed by other participants.

Since we only have one session for treatments with decisions in each order, the order effects could also be confounded with session effects (Fréchette 2012). If there are indeed session effects (which we think are unlikely), they seem more likely for destruction decisions (Treatment$5 when JoD is played first). The order effects of giving decisions could be due to a social norm: participants usually ask for (or expect) return when they are nice to others; the order effects of destruction decision may be that participants enjoy the power andwant to earn relatively more money than their peers. Additional sessions could be run to check for the session effects (for example, we could run both orders in one session but that would cause other problems); we doubt that this is what drives our results.

Participants' adding behavior weakly indicates that they care more about the welfare of their peers (other participants in the same session) than the experimenters', this result being in line with the results reported in Frank (1998).

We do not find any gender or age effects on Mach-IV scores, but the Mach-IV score has a statistically strong negative relationship with giving decisions, albeit not for destruction decisions. Therefore, participants who have high Mach-IV scores are more selfish: more likely to take money from others if it is beneficial for them to do so. For the destruction decisions in our experiment, since they could not benefit from destroying, high Mach-IV scorers add money to others and maximize social welfare. High Mach-IV scorers are more cunning, but they are not nasty if it is not beneficial to do so.

6 Conclusion

The literature on Dictator game experiments shows that people are quite altruistic, while the literature on Joy-of-Destruction game experiments implies that sometimes people are very nasty. We also know, especially for D game experiments, that altruistic behavior is likely to be affected by institutional changes that -- in a well-defined sense – should not make a difference (List 2007). We test such institutional effects for both D games and JoD games.

By asking participants to make decisions (in D games and JoD games), we explore the consistency of participants' giving and destruction decisions and their robustness across different contexts; we find considerable context dependence (see the dramatic shifts of

[23] In the Baseline treatment, 15 out 47 chose to give $2 (allocation was 8, 7), however, only 6 out 48 and 2 out 48 gave money in Treatment$1 and Treatment$5.

distributions for both decisions). For the decision of whether to give money to others, the statistically strongly significant effect in Treatment$5 reflects that moral scruples get thrown overboard when they become too costly. Our study replicates the results of List (2007) which demonstrate that giving decisions (whether to give) are context dependent[24]. We also demonstrate that destruction decisions (whether to destroy) are context dependent when destruction decisions (JoD game) are elicited first, even though the destruction decisions are not as sensitive as giving decisions[25].

The context effects might reflect that, when it is easy to figure out cues (such as giving and destruction decisions in the Baseline treatment), participants are more likely to choose the "expected" choice - give or destroy (20 percent are inconsistent). However, when cues are more difficult to identify (for example, the action space is symmetric to the initial endowment in Treatment$5, participants could either choose to give (destroy) or take (add money)), experimenter demand effects decrease (Zizzo 2010). Participants' choices then may reflect their true preferences (50 percent of participants took money and had money added in Treatment$5).

It is well known that the way in which an experiment is conducted is eminently important (Smith 2002; Camerer 2003; Ortmann 2010). In addition to controlling for social distance (as we did) or order effects, a number of other robustness tests suggest themselves: increasing the stakes and studying the effect of asset legitimacy (especially the entitlement for the money that could be taken) come to mind immediately. We anticipate that such robustness tests would go in predictable directions.

[24] It also seems to suggest that indirect reciprocity, whether negative or positive, that our design makes possible in principle, seems not something we have to worry about much.

[25] The two decisions are also highly correlated. Most participants are social-maximizer: they are selfish (taking money from others) and nice (adding money to others if they do not need to pay for it).

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Appendix I: Instructions

Welcome to this experiment on individual decision-making.
Please do not talk during the experiment. If you have a question, please raise your hand and an experimenter will come to your carrel and answer your question. Please also turn off your mobile phone, or other electronic gadgets, now.

Anonymity

Your decisions in this experiment will not be revealed to anyone.

Total payment:

In this experiment, each of you will be paid $5 for having shown up on time. **This show-up fee is independent of any decision that you make in the experiment that follows.** You will be able to earn additional dollars through the decisions we ask you to make in Part I.

PART I:

You will be asked to make **two independent decisions**. In each of the two decision situations, you are the decision maker (DM). After you have made your decisions, we will ask you to flip a coin. **The result of the toss will determine which decision is payoff-relevant (if it is head, your payoff-relevant decision is decision 1; otherwise your payoff-relevant decision is decision 2)**. Hence you can affect your earnings directly through your decisions.

Your payoff-relevant decision might also affect another person (from participants in this experiment) that you will be matched with, and you will also receive a payment which is determined through another participant's decision.

So your earnings for this part is your payment as a decision maker resulting from your payoff-relevant decision, plus the payment as a recipient resulting from another participant's payoff-relevant decision.
All matches are anonymous and will be done by random permutation. Hence, two participants will not be matched twice. If participant A is the recipient of participant B, then participant B will not be the recipient of participant A and each participant would not be the participant of her-or himself.

[Any questions?]

[We now discuss in more detail the two decisions that you have to make.]

In **Decision 1**, you will be randomly and anonymously matched with another participant. You are the decision maker (DM) and the other participant is the recipient.

Now imagine that each of you will be given $5. As DM, you will be given an additional $5, for a total of $10. The other participant, as a recipient, does not have a decision to make.

You can either leave payments unchanged, or give part or all of the additional $5 to the other participant. All possible final payments are given in the table below:

Options	How much you give	Your payment	The other participant's Payment
Option 1	0	10	5
Option 2	1	9	6
Option 3	2	8	7
Option 4	3	7	8
Option 5	4	6	9
Option 6	5	5	10

[Please study the table now. We will ask you in a couple of minutes to choose your preferred option online.]

[Does anyone need more time? Any questions?]

[Now, it is time to make your decision.]

In **Decision 2**, you will be randomly and anonymously matched with another participant. You are the decision maker (DM) and the other participant is the recipient.
Now imagine that each of you will be given $5. As DM, you will be given an additional $5, for a total of $10. The other participant, as a recipient, does not have a decision to make.
Your payment remains unchanged; however, you need to decide whether to leave the other participant's payment unchanged, or decrease part or all of her/his $5. All possible final payments are given in the table below:

Options	How much you decrease	Your payment	The other participant's Payment
Option 1	0	10	5
Option 2	1	10	4
Option 3	2	10	3
Option 4	3	10	2
Option 5	4	10	1
Option 6	5	10	0

[Please study the table now. We will ask you in a couple of minutes to choose your preferred option online.]

[Does anyone need more time? Any questions?]

[Now, it is time to make your decision.]

[We are now going around and determine which of your decisions is payoff-relevant.]

[Part II: Online questionnaire
Experimenter B: while experimenter A is preparing your payment, now we ask that you answer the brief questionnaire that you find online. There are no right or wrong answers. We appreciate you reading the questions carefully – please note that we have put in a time delay for each of the questions, so rushing through is not possible. We appreciate you answering the questions honestly and completely. Thank you for your cooperation.]

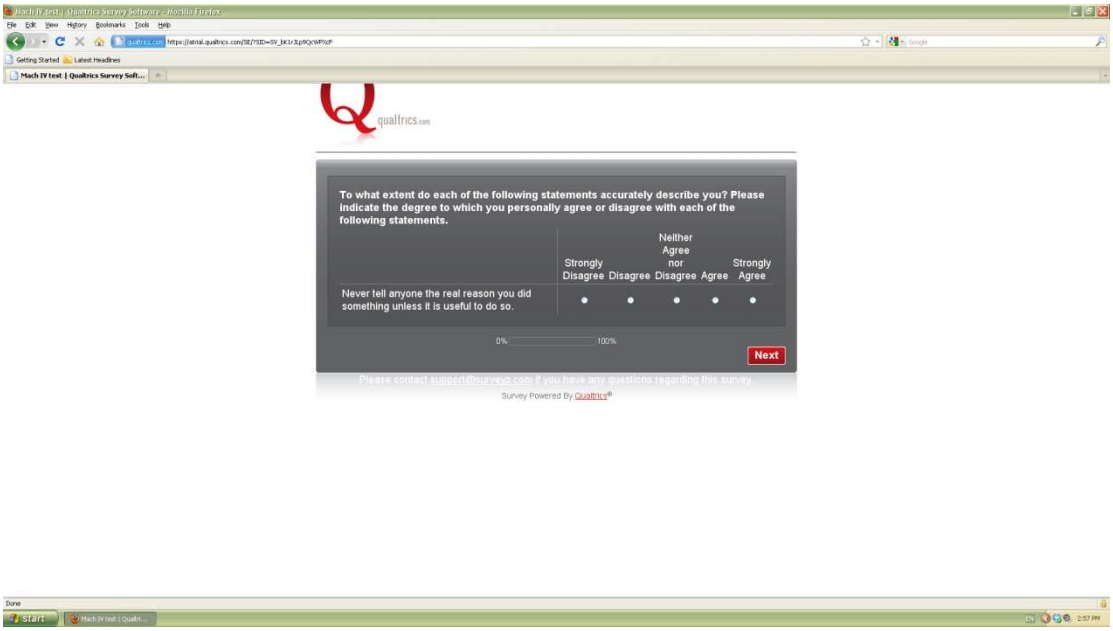
Appendix II: The Mach-IV test

The Mach-IV test includes 20 statements which participants were asked to rate on a 5 – point Likert scale ranging from strongly disagree to strongly agree. The answers are aggregated and mapped into a range that allows classification of types as high- and low- Machs[26]. We added a "catch question"[27] to gauge whether subjects paid attention to the questions.

Q1. Never tell anyone the real reason you did something unless it is useful to do so.
Q2. The best way to handle people is to tell them what they want to hear.
Q3. One should take action only when sure it is morally right.
Q4. Most people are basically good and kind.
Q5. It is safest to assume that all people have a vicious streak and it will come out when they are given a chance.
Q6. Honesty is the best policy in all cases.
Q7. There is no excuse for lying to someone else.
Q8. Generally speaking, people won't work hard unless they're forced to do so.
Q9. All in all, it is better to be humble and honest than to be important and dishonest.
Q10. When you ask someone to do something for you, it is best to give the real reasons for wanting it rather than giving reasons which carry more weight.
Q11. Most people who get ahead in the world lead clean, moral lives.
Q12. Anyone who completely trusts anyone else is asking for trouble.
Q13. The biggest difference between most criminals and other people is that the criminals are stupid enough to get caught.
Q14. Most people are brave.
Q15. It is wise to flatter important people.
Q16. It is possible to be good in all respects.
Q17. P.T. Barnum was wrong when he said that there's a sucker born every minute.
Q18. It is hard to get ahead without cutting corners here and there.
Q19. People suffering from incurable diseases should have the choice of being put painlessly to death.
Q20. Most people forget more easily the death of their parents than the loss of their property.

[26] Here is the scoring rule: Score = $_POST['Q1']+ $_POST['Q2'] + (6-$_POST['Q3']) + (6-$_POST['Q4']) + $_POST['Q5'] + (6-$_POST['Q6']) + (6-$_POST['Q7']) + $_POST['Q8'] + (6-$_POST['Q9']) + (6-$_POST['Q10']) + (6-$_POST['Q11']) + $_POST['Q12'] + $_POST['Q13'] + (6-$_POST['Q14']) + $_POST['Q15'] + (6-$_POST['Q16']) + $_POST['Q17'] + $_POST['Q18'] + $_POST['Q19'] + $_POST['Q20'];
[27] The "catch question" is "A person asks you whether you are studying in the University of Sydney. Please choose "strongly disagree" as you are studying in the University of New South Wales now".

Screenshot:



Appendix III: Typology

Within each of the three treatments, participants are asked to make decisions in both D and JoD games. We expected to find four types[28] in the Baseline treatment: participants who do not give in D and do not destroy JoD are T0 (status quo); participants who give in D and do not destroy in JoD are T1 (pro-social); participants who do not give in D and destroy in JoD are T2 (anti-social); participants who give in D and destroy in JoD are T3 (inconsistent).

In the Treatment$1 and Treatment$5, in addition to those four types, the pro-social type also includes T1-2 (giving in D and having money added in JoD) and T1-3 (not giving in D and having money added in JoD); the anti-social type now also includes T2-2 (taking in D and destroying in JoD) and T2-3 (taking in D and not destroying in JoD). We also

[28] We separate Baseline treatment from Treatment$1 and Treatment$5 (which List (2007) uses to understand the price of moral scruples), as not giving or not destroying in the Baseline treatment might mask the lack of moral scruples (or the cost of deception in Konow (2000)). Murphy and Ackermann (2012) categorize participants in to eight types: altruistic, pro-social, individualistic, competitive, sadistic, sadomasochistic, masochistic and Martyr. These map into our categories (roughly) in the Treatment$1 and Treatment$5 as follows: T0-competitive; T1-altruistic; T2-sadistic; T3-sadomasochistic and T4-prosocial.

hypothesized that a new type will emerge-T4 (selfish welfare maximizer). Those participants take money in D and have money added in JoD.

Table 4 Categorization for types of participants' choices

Treatment	Types	Give	Destroy	Description
Baseline	T0	0	0	Status quo
	T1	+	0	Pro-social
	T2	0	+	Anti-social
	T3	+	+	Inconsistent
Treatment$1 & $5	T0	0	0	Status quo
	T1	+	0	Pro-social
	T1-2	+	-	
	T1-3	0	-	
	T2	0	+	Anti-social
	T2-2	-	+	
	T2-3	-	0	
	T3	+	+	Inconsistent
	T4	-	-	Welfare maximize

In contrast, more people of T4 type (welfare maximizer: took money and also had the experimenter added money to others) populate those treatments: 25 percent in Treatment$1 and 50 percent in Treatment$5. We also find that the distributions of different types across treatments are statistically significantly different (pair-wise comparisons with Fisher's exact tests).

Table 5 Participants' types across treatments

Types										
Treatment	T0	T1	T1-2	T1-3	T2	T2-2	T2-3	T3	T4	Total
Baseline	14	20	0	0	3	0	0	10	0	47
Treatment$1	6	5	7	7	1	1	4	5	12	48
Treatment$5	3	1	4	4	1	5	4	2	24	48
Total	26	26	12	13	5	7	12	19	47	167