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Recognizing Large Donations to Public Goods: An Experimental Test

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August 5th, 1999

Repeated one-shot public good experiments commonly tell participants only of their group's *total* contribution after each round. In contrast, private charities sometimes publicise large contributions or contributors to encourage others to give or to bring recognition to donors. The effect of supplying such selective information on contribution levels is tested here experimentally. Following a control treatment with standard information, a second treatment also informs subjects of the maximum contribution made in their group after each round. In a third treatment, subjects are further given the opportunity to make costly rewards to the (unidentified) maximum contributor. Revealing generous contributions appears to raise average contributions slightly. Adding the ability to reward large contributors does not generate further increases, but raises the variance of contributions.

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1. Introduction

A robust finding within the experimental public good literature is that a significant proportion of individuals contribute towards a public good when the individually payoff-maximizing choice is to free-ride on others' contributions (Ledyard, 1995). In repetitions of one-shot games, average contributions commonly start roughly half-way between what is individually rational and what is socially optimal, and decline gradually, though not collapsing even in the final round. These results would seem to correspond to the empirical observation that private organizations can successfully raise one-time contributions for charitable causes, albeit below socially optimal levels.

Yet charitable organizations who furnish public goods and the experiments that mimic them often differ in their use of information. Experimenters traditionally have provided subjects with feedback only on the average level of contributions within their group. Charities often publicize additional information, identifying generous donors or the size of large contributions. Such selective information may bolster the "reference contribution level" expected by donors when choosing their own contributions, and bring social recognition to large donors. Thus, experimental studies may provide misleading indications of the degree of free-riding that could be expected in actual one-shot fund-raising drives.

This paper reports the results of an attempt to test for information and recognition effects in a voluntary contributions mechanism (VCM) experiment. To begin with, a control treatment is run, using the standard Isaac, Walker and Thomas (1984) and Andreoni (1995) design, with linear payoffs, 10 decision- rounds and continuous group mixing. The control treatment is then compared to a second one in which subjects are informed of the maximum contribution made in their group after one round before proceeding to the next. In the third treatment, subjects also have the opportunity to make costly rewards to the unidentified maximum contributor.

The paper shall be organized as follows. Section 2 will provide a survey of the literature on information and recognition effects in voluntary public good settings. Section 3 will describe the experimental design of the three treatments. The results are described and analysed in Section 4, and a discussion and conclusion follow in Section 5.

2. Bronze Plaques and Press Releases

The roles of information and recognition in experiments has received increasing attention in the voluntary public good literature. As alluded to, common practice in public experiments has been to inform a subject of the vector of initial incomes, the common marginal per capita return (MPCR) from contributing to the public good, and the *total* group contribution after a given round (Isaac, Walker and Thomas (1984), Andreoni (1988), (1995)). This enables subjects to infer the *average* level of giving in the group they have been in before making their next contribution decision in a new group. Empirical studies of voluntary giving have suggested, however, that charities often provide select information to potential donors. Harbaugh (1998) and Glazer and Konrad (1996) observe that charities often announce contributions in discrete categories, or reveal some or even all *individual* contributions. And, as anyone who has set foot on a North American university campus can observe, the actual identities of large donors are often identified with commemorative plaques. Exposing the generous can increase the private good aspect of charitable giving, presumably to increase donations. This could occur through (at least) two channels.

First, by publicizing large past donations or donors, charities may increase the "reference level" of contributions that people in peer groups expect from others when choosing their own contributions in a current fundraising drive. Individuals expecting others to give more may increase their contributions if they are either "relative" warm glow altruists or wish to reciprocate the maximum generosity of others (Andreoni (1990), Croson (1998)). Romano and Yildirim (1998) propose a helpful general utility function to explore these possibilities:

$$U^i = U^i (x_i, Y, h^i (y_1,...,y_N)) \quad . \qquad (1)$$

Y refers to total public good provision and the y_i's are group member's respective contributions. Absolute warm glow altruism would be represented by $h^i (\cdot) = y_i$. Relative warm glow altruism or reciprocity towards generous contributor j could be represented by $h^i_{i, j\neq i} (\cdot) < 0$. Along a related line, Romano and Yildirim show that charities can use individual reports to transform simultaneous contribution games into sequential ones. They show that this can induce donors with warm-glow altruism to increase their contributions.

As a second channel of influence, publicity creates the opportunity for donors to receive social (external) approval along with the public good. It enables fellow contributors or society at large to supply approval to those who are "doing their part." Conversely, publicity can enable others to express social disapproval towards low- or non-contributors. As Fehr and Schmidt (forthcoming) have pointed out, givers have few other tools at their disposal for punishing free-riders in voluntary public good settings.

Andreoni and Scholz (1998) have tested empirically for interdependent preferences towards charitable giving, using the American Consumer Expenditure Survey. They find "modest support" for interdependent effects. Specifically, increases in the giving of others in a household's "social reference space" (of age, education, occupation and location) by 10% lead it to increase contributions by between 2% and 3% on average.

A growing list of VCM experiments have now been run that test the effect of releasing all individual information or providing sanctions. Sell and Wilson (1991), Weimann (1994) and Croson (forthcoming) each compare sessions in which participants learn of individual contributions or totals only. All allow subjects to remain in the same group each round, creating a finitely repeated public good game. The Sell and Wilson study finds that full information increases mean contributions, whereas the Weimann and Croson studies find no significant effect. It is hard to interpret these contradictory findings. Telling participants what every other group member is doing may facilitate attempts at strategic cooperation (Kreps et al. 1982), but it can also reveal demoralizing strong free-riding by one or two group members.

Others have used VCM experiments to test for evidence of specific preferences, such as reciprocity or warm-glow altruism. Weimann (1994) finds that placing subjects with "phantoms" who are always cooperative has no significant effect on contributions. In contrast, placing subjects with free-riding phantoms significantly reduces contributions. Croson (1998) compares subjects' reported expectations of other's behaviour with their subsequent contributions, and finds strong evidence for reciprocity, but not for warm glow altruism. If Weimann's result is replicable, it would suggest that reciprocal preferences are stronger toward negative than positive behaviour. The two papers together would suggest that charities publicise past donations to reassure potential benefactors that they will not be alone in their generosity.

The effects of external rewards or punishments on VCM contributions have also been explored. Yamagishi (1986) allows subjects to contribute to a "punishment fund" immediately after making their public good contribution, but before learning the results of the round. A scalar times the punishment fund is then subtracted from the round earnings of the lowest contributor. Yamagishi found that adding an *ex ante* punishment decision, itself a prisoner's dilemma, significantly increased contributions. McCusker and Carnevale (1995) ran similar tests for reward and penalty sanctions in VCM and common pool dilemmas. They find that both types of sanctions improve cooperation, rewards more than penalties. As with Weimann, however, McCusker and Carnevale place subjects with computerized team mates.

More recently, Dickinson and Isaac (1998) have run *ex post* reward treatments with full groups, though retaining traditional VCM information conditions. They start subjects with differing endowments, and offer an externally funded prize to the individual making the contribution that is largest absolutely, or relative to size of endowment. Though free-riding remained the dominant strategy, both reward structures elicited significantly higher contributions. Enough in fact to fund the external prizes. Moving from carrot to stick, Fehr and Gachter (1998) have run experiments in which individual contributions are fully identified and participants themselves can initiate *ex post* sanctions against specific individuals. Fehr and Gachter make the cost of punishment increase with size, so that the relative cost of making a small punishment is quite low (in some cases 1 unit given up punishes by 4 units). They find that the (non-credible) threat of punishment is sufficient for the VCM to achieve almost complete efficiency.

While each of these papers has shed light on the effects of information and sanction effects, none has tested for the effects of revealing selective individual data. For actual voluntary agencies, it may not always be feasible or desirable to publicise every individual contribution. In addition, the studies mentioned do not provide a controlled separation of the effects of information (that may alter reference contribution levels) from the effects of social sanctions.[1] To test for these effects, a control treatment is followed by a second that differs only in that subjects are informed of the maximum contribution made in their group after one round before going to the next. A third treatment also allows individuals to make *ex post* rewards to the highest contributor.

3. Experimental Design

All three treatments employ the standard Isaac, Walker and Thomas (1984) and Andreoni

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[1] It is possible, however, to make rough comparisons between Fehr and Gachter's (1998) punishment experiment and the three studies cited that add only full information.

(1995) VCM design. In each of 10 rounds, each person is "placed" in a new group of 5 people, endowed with 80 tokens, and asked to allocate them between an "Individual Exchange" and a "Group Exchange." Each token yields one (New Zealand) cent in the Individual Exchange, and one-half a cent for each of the group members in the Group Exchange (MPCR = .5). With the linear payoff design, a subject who wishes only to maximize own-earnings, and who assumes that all others do too, has a dominant strategy to invest zero tokens into the Group Exchange on every round. Total earnings (and efficiency) are maximized, however, if all subjects were to invest all 80 tokens in the Group Exchange on every round. In the first and second treatments individuals would each earn $20 under the efficient solution, and $8 under the dominant strategy.

For reasons unrelated to the experiment reported here, most subjects were recruited with the requirement that "you must bring $8 to the experiment." These subjects were informed that they cold avoid losing the $8 with certainty, and were told that they could also expect to earn, on average, between $10 and $18 (N.Z.) for a one hour session. 16 of 90 students had to be recruited on a same-day basis, and the first 10 of these were not asked to bring $8. The sessions involving these 10 students (2 out of 6) were in the second and third treatments. This difference in recruitment may introduce risk or income bias into the sample of students agreeing to participate in the two sessions, but not to their expectation of earnings within the experiment.

Ninety students participated in total, with thirty per treatment in two sessions of fifteen each. The experiment was conducted manually with paper decision slips in a classroom, with a single computer outside. No communication was allowed, and students were placed so as not to be able to view others' decisions. Two assistants collected the slips within the classroom, and a third entered the results outside on a spreadsheet. A blank "Earnings Report" containing the earnings from the Individual Exchange, Group Exchange, and Total, was collected from subjects after each round along with their decision slip. The Report was then filled in by assistants outside the classroom, and passed back to subjects before each successive round. Group compositions were pre-ordered so that individuals would never be in exactly the same group twice. After making decisions over ten rounds, subjects were called out of the room individually, and paid.

Treatment I, the control treatment, was conducted along these lines. In Treatment II, everything was as in Treatment I, except that the Earnings Report subjects received back after each round included an extra column informing them of "[l]argest single [i]nvestment in Group Exchange in [y]our [g]roup." The following lines were added in the written instructions:

> "Aside from your total payoff from the round, the earnings report will also contain something else. It will inform you of the largest investment in the Group Exchange made by an individual in your group in the round just completed. The identity of this individual (or individuals if they tie) will not be disclosed, only the largest individual amount invested in the Group Exchange."

Then, as with all treatments, subjects were assured that "[a]part from this, your investment decisions and earnings are confidential." Since the groups were scrambled after each round, subjects could not ensure they were rewarding the maximum contributor by increasing their contribution on the subsequent round. Thus, I shall attribute any significant increase in average giving between Treatments I and II to relative warm glow altruism. Higher giving could be attributed to reciprocity to the most generous, but with new groups it could operate only in the generalized sense that "someone has been very nice to me, and so I shall be nicer to others."

Finally, Treatment III makes two alterations from Treatment I. It includes the information of Treatment II, and gives subjects the opportunity to reward the maximum contributor within their group. Since the added step requires a number of design decisions, it shall be described in more detail. Modelled on Fehr and Gachter (1998), each of the 10 rounds is sub- divided into an investment decision, followed by a transfer decision. Following the investment decision, a *Preliminary* Earnings Report is returned. Then, each subject can transfer between 0 and 40 cents to the (unidentified) individual who made the maximum contribution within the group in the investment round just completed. The upper limit is set by the minimum possible amount a subject could have earned in that investment decision. Each cent transferred is doubled by the experimenter before being received by the maximum contributor. Thus, the "transfer exchange rate" is constant at 2.

Subjects were told that if two or more maximum contributors tied, one would be chosen randomly to receive any transfers made by the other four group members.[2] Subjects had to choose their transfers knowing the *size* of the maximum contribution, but not necessarily knowing if they would be the one chosen to receive transfers. For example, if a subject had contributed all 80 tokens to the group exchange, he would then be told that the maximum contribution had been 80, but not whether others had also given 80, nor whether he had been designated to be the recipient of such transfers. Instead, all subjects were told to make a transfer decision, and that the designated receiver's stated transfer would be disregarded. After handing in their transfer decision slips, subjects received an updated *Final* Earnings Report. The Final Report reminded subjects of their preliminary earning from the round and told them their final earnings after transfers had been deducted or added on. Only on the Final Report did the designated receiver learn of his or her status for that round.

What are the (selfish) game-theoretic predictions for the reward treatment? With continuous group mixing, I shall consider a given round as a one-shot, two-stage sequential game. Intuitively, the reward decision piles a prisoner's dilemma on a prisoner's dilemma. Total earnings, or efficiency, would be maximized if subjects contributed 80 tokens to the Group Exchange in the investment decision, and made the maximum transfer of 40 cents in the transfer

[2] Subjects were told (truthfully) that "each participant who has tied has an equal chance of being designated to receive transfers over all rounds of the experiment." The computer spreadsheet was pre-programmed to give all subjects an equal probability of having first, second, third, fourth or fifth priority in case of ties over the ten rounds of the experiment.

decision. Individually, however, the dominant strategy is for individuals to give zero and transfer zero. Working backward, we can identify two cases in the transfer decision. If the individual is not among the subset T of tied maximum contributors, (where $1 \leq T \leq 5$), he is best off transferring zero to the maximum contributor. If the individual is among the subset T of tied maximum contributors, he has a 1/T chance of receiving transfers, and a (1-T)/T chance of only making them. In the first case, it doesn't matter what he offers to transfer as it will be set to zero. In the second, he should transfer nothing. Thus, in every case, a subject is best off transferring zero. Moving back to the investment decision, the game reverts to a regular linear payoff VCM, and the dominant strategy is to contribute zero to the Group Exchange. Individuals in Treatment III could each expect to earn $22.40 at the efficient solution, and $8.00 at the individually rational one.

4. Results

4.1 Subject Pool

Subjects were recruited among undergraduate students at the University of Canterbury in Christchurch, New Zealand, over a two month period in early 1999. Attracting participants from large lectures was more difficult than anticipated, and recruitment was carried out in lectures for first and second year economics, second year math, and first year political science. Thus there was greater subject heterogeneity than is ideal.[3] Treatment sessions were run serially to mitigate the confounding cohort effects, and as far as possible, a mix of students from various classes was sought for each session. Unfortunately, greater subject heterogeneity may increase the variance of the decisions made within any given session. Similarly, cohort effects that remain may have

[3] 16 students had to be recruited from public gathering spots on a same day basis. Their year and majors were recorded only after the experiment took place, however. All were undergraduates outside economics, with the exception of a Ph.D. student in chemistry. This person's decisions do not appear unusual in his session (1, Treatment IV).

increased the differences across supposedly identical sessions, making it more difficult to discern differences between treatments.

4.2 Pooling Sessions within Treatments

Whether due to subject pool differences or other causes, different sessions of identical treatments yielded surprisingly different mean contribution levels. A Mann-Whitney rank sum test was used to compare the sample distribution of contributions of each subject (averaged over all rounds) between sessions. This test checks whether sample means are significantly different in different sessions of a given treatment. Surprisingly it found no significant difference between the two sessions of each treatment. Moving to a round by round comparison, Figure 1 illustrates the large differences. Mean contributions are systematically higher in the first sessions of Treatments I and III, but not noticeably different in either session of Treatment II.

If we use the Mann-Whitney test round by round, we see that none of the pair of sessions significantly differ in the first round. Better yet, the sessions in Treatments II and III may safely be pooled in every round. In Treatment I (the control), however, the two sessions produced significantly different distributions at the 5% level for Rounds 2, 3 and 4. In the non-parametric analysis that follows, I shall pool the two sessions for Treatment I despite the high intra-treatment variation. Session effects shall be controlled for, however, in the regression analysis that follows.

4.3 Data and Decision Errors

Two errors were detected in the course of running the experiment. In Treatment II, Session 1, a subject's contribution to the Group Exchange in Round 6 was transcribed as 71 rather than 79. In Treatment III, Session 2, a subject in Round 5 transferred 70 cents to the maximum contributor, when the allowable limit was 40 cents. The first error, though minor, would act to

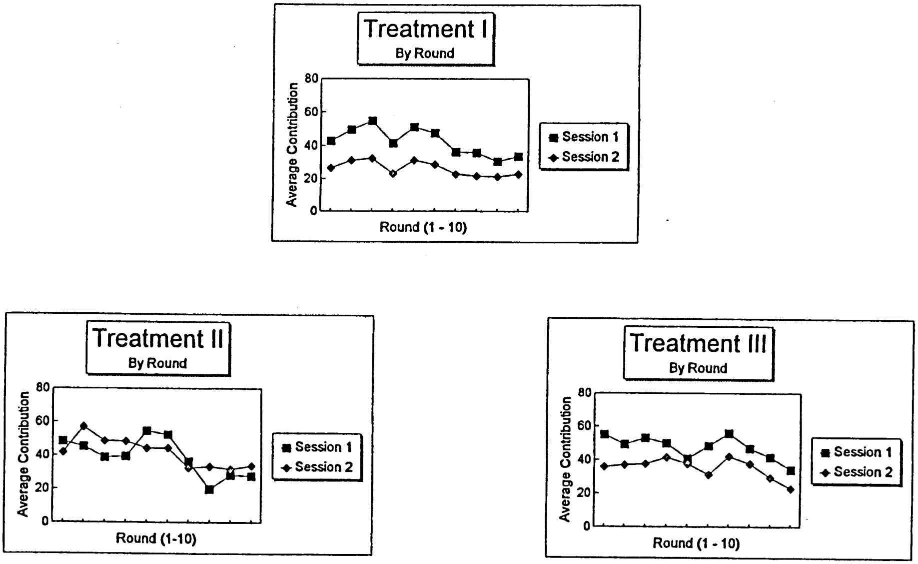

Figure 1: Session Effects

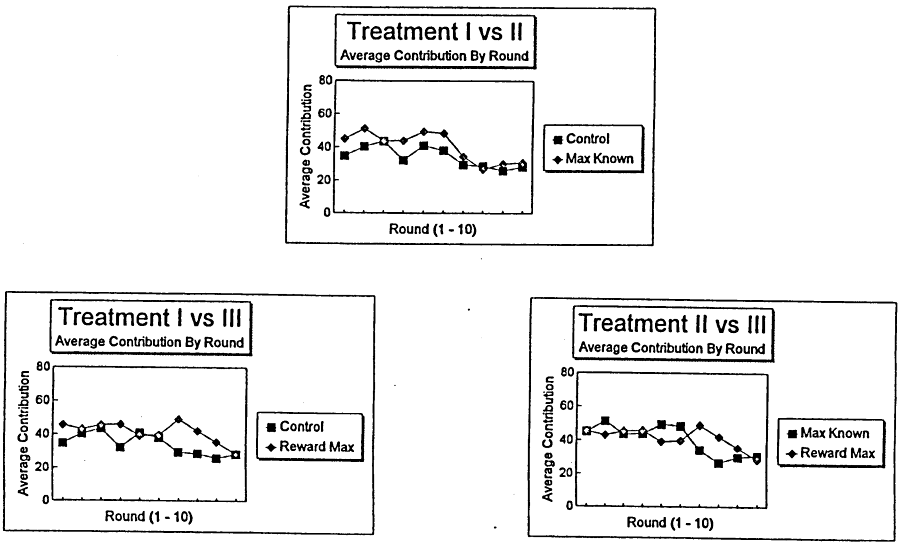

Figure 2: Treatment Effects

lower some subjects' perceptions of contribution rates. The second would have significantly increased the perception of one designated receiver of the average transfers being made by others.

4.4 Findings

Figure 2 presents the mean contribution towards the public good, round by round, for the control (I), information (II) and reward (III) treatments. The overall pattern of contributions is familiar, with mean contributions starting near the mid-way mark and gradually declining over the ten rounds. The average contribution per round appears slightly but consistently higher when subjects are informed of maximum contributions or can reward maximum contributors. Surprisingly, allowing subjects to reward high contributors seemed to have no additional effects on mean contributions beyond those produced by merely identifying high contributions. What appears graphically can be tested statistically, beginning with the simple Mann-Whitney rank sum test for independent samples.

If we take the average contribution of each subject across all ten rounds as our unit of observation, we can compare the distribution of contributions across treatments. Using this approach, the distribution of giving is not significantly higher when subjects are informed of the maximum contribution before going on to the next round ($z = 1.116$, $\alpha = .263$, $N_I = N_{II} = 30$). If we compare the control group with those who had extra information and the ability to reward, we still find that the latter group do not give significantly more ($z = 1.175$, $\alpha = .238$, $N_I = N_{III} = 30$). Not surprisingly, therefore, the effect of adding the ability to reward in isolation is also not significant ($z = .347$, $\alpha = .726$, $N_{II} = N_{III} = 30$).

As a more detailed approach, we can compare the distribution of contributions of subjects round by round. These results are presented in Table 1 below. While the z statistics are of the predicted sign in 24 of 30 cases, they are significant at the 10% level (two-tail) in only 4. Thus

Table 1
A Comparison of Treatments, Round by Round

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
	(z values, two-tail tests)									
I<II?	1.501	1.671*	.118	1.663*	1.131	1.375	.584	*-.370*[1]	.488	.333
I<III?	1.441	.710	.798	1.545	.015	.214	1.959**	1.064	.577	*-.177*
II<III?	.214	*-.562*	.613	.613	*-.976*	*-.850*	1.744*	1.353	.384	*-.517*

* refers to a difference in distributions that is significant at the 10% level (two-tail test)
** refers to a difference in distributions that is significant at the 5% level (two-tail test)
[1] numbers in italics refer to differences of an unexpected sign.

the Mann-Whitney test does not rule out the possibility that the suggestive effects in Figure 2 are merely sampling variation. However, the between-sample Mann-Whitney test is known to have a relatively low power to detect differences, and there are uncontrolled session effects in the control treatment. Thus, we shall re-examine treatment effects using parametric regression analysis.

Pair wise comparisons analogous to those reported in Table 1 are made using a series of ordinary least squares regressions. In each case the contribution of each subject in each round is regressed on a constant and dummy variables for treatment, session, session-treatment interaction and round. Unfortunately, demographic variables were not collected, and so models that control for subject-specific effects, such as random or fixed-effects regressions, cannot be run while simultaneously testing for treatment effects in a between-subject design.

Tests for heteroscedasticity (Breusch-Pagan-Godfrey, Harvey and Glejser) indicated a problem for all regressions involving Treatment III. Thus the White heteroscedasticity-consistent estimator is used for all regressions (Greene, 1997). In practice, this has virtually no effect on any of the results to follow. With only 10 decision rounds, test for autoregressive conditional heteroscedasticity (ARCH) are not pursued.

Finally, the choice of treatment - session interaction dummy terms merits some discussion. In, for example, a comparison of Treatment I and Treatment II, a natural way to proceed would be to regress contributions on a constant, dummy if Treatment II, dummy if Session 2, and a TreatmentII - Session 2 interaction term (along with 9 round dummies). The problem is that the interaction term would be positively correlated with both treatment and session, especially treatment ($\rho = .65$). Ideally, to separately estimate the effect of treatment alone, we would need an interaction term that is orthogonal to both session and treatment. An interaction dummy such as "Treatment I, Session 1 *or* Treatment II, Session 2" would fulfill this requirement. Thus analogous interaction dummies have been used in all regressions. The regression results are reported in Table 2. To conserve space the coefficients on the nine round dummies have been suppressed. Generally, they were positive and insignificant in early rounds, and negative and significant by rounds 9 and 10.

The baseline constant for each regression is the average contribution in the first round of the first session of the first treatment being compared. The magnitudes of the estimated treatment effects correspond closely to non-parametric tests reported earlier. From regression (1) the positive coefficient on Treatment II indicates that is caused a moderate increase in mean contributions over the control treatment. And from (2) the positive coefficient on Treatment III suggests it caused a slightly larger increase over the control. Consistent with this, regression (3) indicates a very small increase in mean contributions from Treatment II to Treatment III. Finally, all three treatments are compared simultaneously in regression (4), with the same results.[4]

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[4] I could not construct two treatment-session interaction terms that were orthogonal to both Treatments II, III and Session 2. The two interaction dummies in (4) are orthogonal to both dummies for treatment, but positively correlated to Session 2 (both with $\rho = .333$).

Table 2
A Test of Information and Reward Effects Using OLS Regression

	(1) I vs. II	(2) I vs. III	(3) II vs. III	(4) I vs. II vs. III
	$Contribution_{i,t}$	$Contribution_{i,t}$	$Contribution_{i,t}$	$Contribution_{i,t}$
Constant	36.593*** (3.750)	43.638*** (4.144)	50.805*** (4.121)	46.317*** (3.363)
Treatment II	5.967*** (2.189)			5.967*** (2.199)
Treatment III		7.050*** (2.465)	1.083 (2.535)	7.050*** (2.470)
Session 2	-7.060*** (2.189)	-14.430*** (2.465)	-4.890* (2.535)	-4.890* (2.537)
TreatI,Sess1 or TreatII,Sess2	9.540*** (2.189)			
Treat1,Sess1 or TreatIII,Sess2		2.170 (2.465)		
TreatII,Sess1 or TreatIII,Sess2			-7.370*** (2.535)	
TreatI,Sess 2 or TreatII,Sess 2 or TreatIII Sess 1				-2.170 (2.470)
TreatI,Sess 2 or TreatII,Sess 1 or Treat III,Sess2				-9.540*** (2.199)
Round 2 . . *Round 10*	*results suppressed*	*results suppressed*	*results suppressed*	*results suppressed*
R^2	.116	.089	.054	.082
N observations	600	600	600	900

*, **, *** refer to significance at the 10%, 5% and 1% levels, respectively in two-tailed tests
Run on Shazam 8.0

What differs in the regression analysis is the level of significance of the treatment effects. When errors are assumed normally distributed and session effects are controlled for, both the information and (information + reward) treatments significantly raised mean contributions over the control treatment. Yet consistent with the non-parametric findings, adding the ability to reward large contributors did not produced any significant further increase in mean contributions.

4.5 Auxiliary Findings About Rewards

While the initial objectives of this paper have been met, several interesting questions emerge in relation to the reward treatment. These shall be briefly stated and answered. First, did the transfers in Treatment III increase efficiency enough to cover the 1:1 matching expenses incurred by the experimenter? In other words, did the 1:1 reward subsidy increase efficiency enough to pay for itself? Second, is there any systematic relationship between those who contribute to the Public Exchange and those who reward contributors? And third, did the information or reward treatments affect the variance of contributions, as opposed to means?

Regarding the first question, the 30 subjects in Treatment III actually paid out a total of \$10.97 of their own earnings to others who had made maximum contributions.[5] This translates to an average of 4.57 cents out of a maximum 40 per eligible person, per round. Total preliminary earnings (which are equivalent to total final earnings net of subsidy) in Treatment III were \$32.63 higher than final earnings in Treatment I. Thus the 1:1 reward scheme payed for itself, and increased efficiency, consistent with Dickinson and Isaac (1998). However, preliminary earnings in Treatment III were \$4.84 *less* than earnings in Treatment II. That is, Treatment III raised efficiency over the control group by \$32.63, or 8.3%, but Treatment II raised it even higher, by

[5] If the subjects who transferred 70 tokens is constrained *ex post* to give 40, total transfers fall to \$10.67.

$37.47, or 9.5%. Thus externally-funded or matched rewards increased efficiency, but publicising selective information alone did just as well.

Secondly, who is doing the rewarding in Treatment III? Are the same people who bravely ignore the prisoner's dilemma in contributions also ignoring it when making transfers? Are the generous rewarding the generous? Or are guilty non-contributors salving their consciences? When answering this we may include the proposed transfers of maximum contributors whose transfers were subsequently disregarded. The correlation between the average amount a subject contributed (over ten rounds) and the average amount he or she transferred was surprisingly low. It is .125 if the errant subject who made the 70 cent transfer is included, and .275 if he or she is excluded. Figure 3 provides a scatterplot of subjects' contributions and proposed transfers. It similarly reveals a high degree of variance, with only a hint of a positive correlation.

Finally, while the reward treatment does not increase the mean contribution per subject beyond the information treatment, it seems to alter the distribution of contributions. Specifically, the option to reward maximum contributors induces subjects to abandon moderate contribution levels, and opt for extremes of zero or maximum contribution. Figure 4 illustrates this effect by comparing the density functions of contribution levels across the three treatments for Round 8. The spikes of individuals opting to give 20 or 40 in the first two treatments are missing in the third. Rather, almost all contributions were at the tails. Table 3 reports the results of tests of differences in the proportion of subjects contributing zero and contributing 80, across the three treatments.[6] Interestingly, the proportion giving zero is almost never statistically distinguishable between treatments. The proportion giving 80, however, is significantly higher in the reward treatment than in the control treatment in 7 of ten rounds. This finding is also consistent with

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[6] The test of differences in proportions between treatment A and treatment B is given by $z = (p_A - p_B)/(p(1-p)(1/n_A + 1/n_B))^{.5}$, where 'p' is the pooled proportion across A and B.

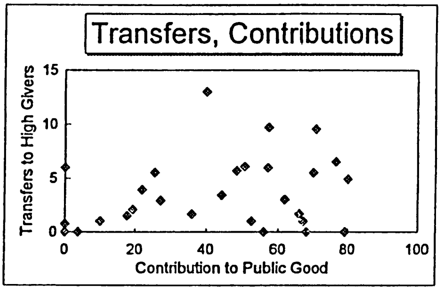

Figure 3: Average Transfer vs. Average Contribution, by Subject

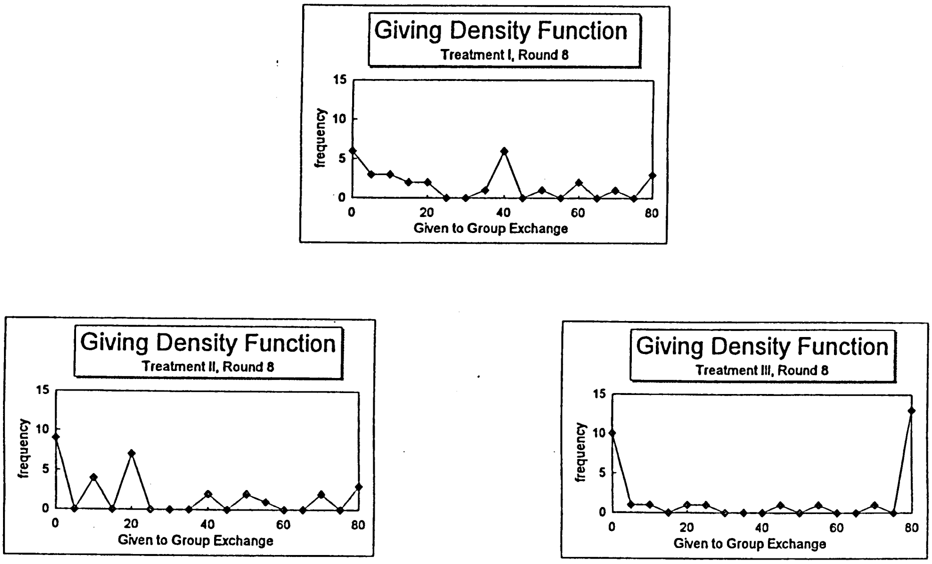

Figure 4: Density Functions of Contributions, Round 8

Table 3
A Comparison of Corner Decisions, Round by Round

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
% who gave zero tokens to public good (N = 30 each cell)										
Tr. I	.233	.167	.167	.233	.233	.2	.267	.2	.267	.367
Tr. II	.067	.067	.133	.2	.167	.2	.267	.3	.3	.3
Tr. III	.167	.3	.233	.3	.267	.333	.267	.333	.367	.467
% who gave 80 tokens to public good (N = 30 each cell)										
Tr. I	.1	.067	.1	.067	.167	.133	.133	.1	.1	.167
Tr. II	.2	.233	.133	.2	.3	.267	.133	.1	.1	.167
Tr. III	.267	.333	.333	.433	.267	.233	.467	.4	.267	.167
Tests of differences in proportions giving zero (significant z statistics only are reported)										
I > II?	1.81*	-	-	-	-	-	-	-	-	-
I > III?	-	-	-	-	-	-	-	-	-	-
II > III?	-	*-2.33***	-	-	-	-	-	-	-	-
Tests of differences in proportions giving 80 (significant z statistics only are reported)										
I<II?	-	-	-	-	-	-	-	-	-	-
I<III?	1.67*	2.58***	2.19**	3.28***	-	-	2.82***	2.68***	1.67*	-
II<III?	-	-	1.83*	1.94*	-	-	2.82***	2.68***	1.67*	-

*, **, *** refer to differences in proportions that are significant at the 10%, 5% and 1% level, respectively, in two-tail tests

[1] numbers in italics refer to differences of an unexpected sign.

Dickinson and Isaac (1998), who find an increase in variance with a purely external reward. In contrast the proportion giving 80 is never significantly higher in the information treatment than in the control. Thus one may conclude that it is the transfer/reward system that is drawing some would-be intermediate contributors to opt for maximum contributions, and not the information feedback on large contributions. This result may be compared with Croson (forthcoming), who finds that *full* individual contribution feedback does increase the variance of contributions.

5. Discussion and Conclusions

This paper reports on the attempts to test for selective information and reward effects upon contribution levels in a voluntary public good environment.

It is found that providing subjects with additional selective information concerning the maximum individual contribution in their group had a small but significant positive influence upon average contributions. Descriptively, mean contributions were higher in 9 of 10 rounds when the high contributions were identified. The increase was significant in only 2 of these rounds using non-parametric tests, but significant overall at the 1% level in regression analysis. These results seem to provide evidence that subjects' contributions are influenced positively by relative warm-glow altruism. This evidence for relative warm glow altruism does contrast, however, with Weimann's (1994) finding that generous phantom subjects do not invoke greater contributions among the living in conventional information treatments. For if all team mates give more, this also should cause relative warm glow altruists to give more.

Surprisingly, providing subjects with the opportunity to make *ex post* rewards to the largest contributor seemed to create no additional increase in mean contributions. This result seems in contrast to the limited findings to date that some types of sanctions can dramatically increase cooperation in VCM environments (Yamagishi (1986), McCusker and Carnevale (1995), Dickinson and Isaac (1998), Fehr and Gachter (1998)). The differences across studies make it difficult to know whether the additional information provided in sanction systems is what really drives the increase in contributions, or if there are simply features of the reward system used here that rendered it ineffective. For example, perhaps voluntary rewards would prove more effective if they could be subsidized at a higher rate than 1:1.

The reward treatment brought several other points to light. First, it raised efficiency sufficiently to pay for a 1:1 external subsidy of transfers. Second, large contributors were not the only ones to make transfers to others. That is, some subjects played the dominant strategy regarding contributions, yet violated it to reward others who did not. Thirdly, the reward structure in particular drew subjects away from contributing intermediate amounts towards the extremes of zero or maximum contribution. Thus the reward treatment affected the dispersion of contributions, even though its effect on mean contributions was slight.

In short, the experiments reported here could help to explain why charities commemorate large donations from past fund-raising drives. The social approval afforded past donors may not spur on current ones, but the size of past donations may increase the amount current donors want to give.

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