Altruism in Anonymous Dictator Games*

Catherine C. Eckel

Department of Economics, *Virginia Polytechnic Institute and State University*, *Blacksburg*, *Virginia* 24061-0316

and

Philip J. Grossman

Department of Economics, *Wayne State University*, *Detroit*, *Michigan* 48202

We conduct double-anonymous dictator experiments to explore the role of altruism in motivating subjects' behavior. We vary the extent to which an anonymous recipient is deserving of aid and investigate its effect on the allocation of a fixed pie by student subjects. This is accomplished by including as treatments: (1) an anonymous student subject and (2) an established charity. We find that a significant increase in donations occurs when we increase the extent to which a donation goes to a recipient generally agreed to be "deserving." We conclude that subjects are rational in the way they incorporate fairness into their decisions. *Journal of Economic Literature* Classification Numbers: A13, C91, D64.   © 1996 Academic Press, Inc.

> How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortunes of others, and render their happiness necessary to him, though he derives nothing from it except the pleasure of seeing it.
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> —Adam Smith (1969, p. 47)

I. INTRODUCTION

The behavior of subjects in ultimatum and dictator games is well documented and deviates from payoff-maximization.[1] Although the generosity of proposers in the ultimatum game may be motivated in part by strategic considerations, this is not the case in dictator games when the recipient is powerless.[2] The common explanation given for the observed deviations of outcomes from the game-theoretic predictions is that participants are motivated by factors in addition to monetary payoffs.

Two recent papers in this journal—Hoffman *et al*. (1994) and Forsythe *et al*. (1994)—raise questions about whether "other-regarding behavior" (altruism or fairness) explains the observed outcomes. Both papers suggest that it is not the primary motivation for behavioral differences observed across treatments.

We employ the double-anonymous dictator game developed by Hoffman *et al*. (1994) to further test the role of altruism in explaining observed behavior. Our design increases the motivation for altruistic behavior by replacing the usual anonymous recipient with a reputable charity (the American Red Cross). In the standard dictator game, the subjects can infer that the anonymous person is someone like themselves, recruited under similar circumstances to participate in the experiment. In our treatment, the subjects know that the recipient is an established charitable organization. This treatment substantially increases altruistic giving. We infer from our results that: (1) altruism is a motivating factor in human behavior in general and in dictator games in particular, and (2) even under double-anonymous conditions, an increase in the "deservingness" of the recipient increases the quantity of donations by experimental subjects.[3]

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[1] The early research in this area is surveyed by Thaler (1988) and Güth and Tietz (1990), who note evidence that considerations of fairness play a major part in the outcomes of these experiments. A more recent survey by Roth (1995) stresses the importance of the strategic environment in determining the role of fairness in the outcome of bargaining games.

[2] Given the typical pattern of rejection rates in ultimatum games, positive offers by the Proposer may be payoff-maximizing. Roth (1995) notes this pattern. In our ultimatum games (Eckel and Grossman, 1995) we find that the best response to the pattern of rejections is a proposal of a \$3/\$2 split of \$5. After eight rounds, the modal proposal is exactly this best response, and the mean proposal is only slightly higher. We also find differences in the rejection rates of different subject groups, and mean proposals for the two groups reflect these differences.

[3] In our charity treatment, we replace an anonymous *individual* with a charitable *organization*. See Section III and Footnote 9 for a more detailed discussion of the two treatments.

II. OTHER-REGARDING BEHAVIOR IN THE OUTCOME OF
ULTIMATUM AND DICTATOR GAMES

Hoffman, McCabe, Shachat, and Smith (1994). Hoffman et al. argue that the generosity of subjects in ultimatum and dictator games may be the result of the "experimenter effect" (the effect from the experimenter knowing the individual's decision) rather than the result of altruism or fairness.[4] To test this hypothesis, they conducted dictator games under cleverly designed double-anonymous conditions. Under between-subject anonymity alone, 22 of the 48 dictators (46%) donate at least $3 of a $10 pie.[5] When subject–experimenter anonymity is added, only 12 of 77 dictators (16 percent) donate at least $3. They conclude that their double-anonymous condition "may *approach the appropriate indicator of fairness as a pure preference phenomenon*" (italics per the original) and argue that "(altruistic) behavior may be due not to a taste for "fairness" (other-regarding preferences), but rather to a social concern for what others may think, and for being held in high regard by others" (p. 371).

We suggest a different interpretation. By imposing double anonymity, Hoffman *et al.* (1994) have removed virtually all motivation for donating money to one's partner. The decision makers cannot identify each other, nor do they have enough information to know if their partner is poor or otherwise deserving of their generosity; thus there is little or no basis for altruism to play a part in the decision. Furthermore, as subjects cannot be identified by either the experimenter or other subjects, there is no role for social esteem to affect the decision. Only self esteem (or warm glow) remains. With little motivation for other-regarding behavior, it is not surprising that the subjects' behavior closely approximates the game-theoretic predictions for noncooperative, nonrepeated games with selfish, payoff-maximizing subjects.

These findings, while important, do not eliminate the *possibility* that behav-

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[4] Hoffman *et al.* (1996) explore the incremental effect of each aspect of their "double-blind" experiment, isolating the experimenter effect from other components of the procedure. They find that every aspect of the design, including experimenter/subject anonymity, affects subjects' decisions.

[5] As shown in Hoffman *et al.* (1994), pp. 365–366, Fig. 4, their results are as follows:

Experiment		Number of subjects choosing a donation of:			
		3	4	5	10
a,b.	single anonymous/ (22/48 subjects)	14	5	3	0
c,d.	double anonymous 1,2 (12/77 subjects)	4	1	6	1

ior can be motivated by altruism. For fairness, even as a pure preference phenomenon, to affect a decision, a donor must obtain some value from his donation. Most donors, even those who wish to remain anonymous, know their recipient, or at least the general characteristics of their recipient, thus opening the door for altruism to play a part in the decision. People most often contribute, anonymously or not, to an appropriate charitable organization which acts as their representative agent and chooses the recipient of the aid in a manner consistent with their wishes. For example, a person finding $10 on the ground is unlikely to share his good fortune with an unknown stranger who just happens by at that moment. The same person may, however, share his new-found wealth with family and friends or contribute a portion to his or her preferred charity.

We assert that fairness and altruism require context: the circumstances of the recipient determine what is the fair or appropriate charitable action to take. In the Hoffman *et al*. (1994) study, the information available to the Proposer is unlikely to motivate charitable behavior.

Forsythe, Horowitz, Savin, and Sefton (1994).   Forsythe *et al*. compare the outcomes of ultimatum and dictator games. Their primary purpose is to test the "fairness hypothesis" that the distributions of offers are the same in the two games.[6] Noting that the subgame perfect Nash equilibrium outcome of an ultimatum and of a dictator game are the same, they argue, "If nontrivial offers are due solely to proposers' concerns with fairness, the distributions of offers will be the same in the two games." However, instead of identical distributions of proposals, they find significant differences in the play of subjects between ultimatum and dictator games. For example, in their $5 dictator game, 36% of the players play as "pure gamesmen" (i.e., play in a manner consistent with game-theoretic predictions), while 22% play as "altruists" (our terminology), giving away an equal share or better; in their $5 ultimatum games, there are no pure gamesmen, while 65% are altruists. Because the outcomes of the two games differ, they conclude that they "... can reject the fairness hypothesis ... implying that a proposer's taste for fairness, by itself, does not explain the distribution of proposals in the ultimatum game" (p. 348).

In rejecting fairness as the motivation for the observed behavior, Forsythe *et al*. overlook the difference in the opportunity cost of altruistic behavior in the two games.[7] The extent to which an individual will engage in altruistic behavior

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[6] Testing the fairness hypothesis is only one component of the contribution of this paper. A secondary hypothesis of their paper is to test whether paying subjects affects the outcome: it does. A third test examines the replicability of the experiments at two different times of year. Finally, an important contribution of their paper is a comparison of the power of several commonly used statistical tests using simulation.

[7] Neither Hoffman *et al*. (1994) nor Forsythe *et al*. (1994) claim that fairness is *not* a determinant of the observed behavior, only that it is not the sole cause of deviations from equilibrium play.

depends on its opportunity cost.[8] The cost of unfair behavior for the Proposer is not the same in the two games because of the veto power granted the Respondent in the ultimatum game. If an offer is less than the Respondent's reservation value, the offer will be rejected. Since the Proposer is uncertain about the Respondent's reservation value, any offer other than one, constituting the whole pie, risks rejection. Increasing the offer will reduce the risk of rejection. The expected cost of increasing the offer by $1 is less than a dollar. In the dictator game, the Respondent has no veto power so there is no risk of rejection. Hence, increasing an offer by a dollar to make it more fair reduces the Proposer's earnings by a full dollar. Since the cost of donation in the ultimatum game is less than in the dictator game, we expect larger offers to the Respondents in the ultimatum game. Consistent with this less extreme hypothesis, Forsythe *et al.* observed fewer gamesmen and more even splits offered in ultimatum games than in dictator games.

While the authors of both papers downplay the role of altruism in simple bargaining games, neither convinces us. Hoffman *et al.* (1994) remove any reason for subjects to donate money; Forsythe *et al.* (1994) overlook the difference between the dictator game and the ultimatum game in the opportunity cost of giving. As a result, they discount altruism as an explanation for their subjects' behavior.

III. EXPERIMENT DESIGN AND PROCEDURE

Our subjects participate in a dictator game in which the dictator is asked to determine the division of $10 between himself/herself and a respondent. We report the results of two treatments. In the first, we replicate the Double Blind 1 experiment of Hoffman *et al.* (1994). In the second, the anonymous partner in the next room is replaced by a charity, the local branch of the American Red Cross.[9] All other aspects of the experimental design are identical to the

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[8] As a referee notes, altruistic behavior is context-dependent. A comparison of the ultimatum and dictator contexts reveals that one difference between the two is the opportunity cost of "fair" or altruistic behavior. We investigate the effect of relative price on fair behavior in Eckel and Grossman (1996).

[9] As a referee accurately pointed out to us, we are replacing an anonymous *individual* with an established *organization*. Thus, between our two treatments we are changing two characteristics of the recipient: the "deservingness" and individual/organization. The American Red Cross has a strong reputation for providing disaster relief in addition to its well-known blood collection and distribution activities. However, subjects were not informed about the activities of the organization or the distribution of Red Cross funds, and we did not collect data on subjects' beliefs. The individual/organization characteristic of the recipient might affect the results. For example, if subjects believed that only a small fraction of their donation was likely to reach a "deserving" individual (because of administrative costs or wastage, for example), this would decrease donations and strengthen the interpretation of our results. Alternatively, subjects might be more likely to contribute to an organization than, say, a "deserving" person chosen at random from the local community, making our results stronger than they otherwise would be. This might be due to paternalistic concerns about how the money would be

experiments reported in Hoffman *et al.* (1994). A copy of the instructions used for our variation is included in the Appendix.

Subjects were recruited from introductory courses in economics, accounting, finance, and psychology at Virginia Polytechnic Institute and State University during the summer and fall terms of 1993. For each session in Treatment 1, 29 subjects were recruited, 15 to one room (the dictator room) and 14 to a separate room (the recipient room), where each received a $5 show-up fee. After signing a permission release, subjects in both rooms were given the instructions for the experiment. A monitor was chosen in the dictator room, and she conducted the experiment. The experimenter was in the room for the duration of the experiment. For Treatment 2, 15 subjects were recruited to a single room. For Treatment 1, one replication of the Hoffman *et al.* (1994) design was conducted. In Treatment 2, four replications were conducted.

All sessions are conducted using the double-anonymous procedure proposed by Hoffman *et al.* (1994a). Fourteen envelopes—twelve containing 10 one-dollar bills and 10 blank slips of paper—and two containing 20 blank slips of paper—are randomly distributed to the subjects. Subjects, in private, remove 10 units—any combination of dollar bills and paper—from the envelope, seal the envelope, drop it in a box, and leave. After all subjects have deposited their envelopes and departed, the envelopes are randomly distributed to subjects from the Recipient room. Envelopes are opened and the money recorded and given to the subject. A monitor verifies the procedures followed.

In our Charity design, Treatment 2, we modify the procedure so that subjects are asked to divide $10 with a charity, the American Red Cross. After all subjects deposit their envelopes and leave, the monitor is responsible for opening each envelope and recording the contents. The monitor must verify, by a signed statement, that a check for the total amount is written and sealed in an envelope addressed to the charity. The monitor and experimenter then walk together to the nearest mailbox and deposit the envelope.

IV. RESULTS

Table I contains data on the number of subjects making each possible choice about the allocation of the money between the dictators and the recipients. Data are reported for 48 subjects in each treatment.[10]

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spent. In either case, it is an empirical question how our results might differ if a "deserving" *individual* replaced the anonymous recipient in our baseline treatment. (This is an issue we plan to explore in our ongoing research.) However, most importantly, we can be confident that the increase in donations that we observe is not due to the experimenter effect, but to information about the characteristics of the recipient.

[10] We pool our data with the three Hoffman *et al.* (1994) sessions, giving us a balanced data set with four replications in each cell. We exclude two observations of zero in each session; recall that two subjects in each session receive envelopes with no money in them, and so have no opportunity to

TABLE I

| | Number of subjects keeping various amounts | | | |
| | Treatment 1: Anonymous recipient | | | Treatment 2: Charity recipient |
Amount kept	Hoffman et al.	Replication	Pooled	
10	23	7	**30**	**13**
9	7	0	**7**	**5**
8	2	1	**3**	**11**
7	1	1	**2**	**0**
6	0	2	**2**	**4**
5	2	1	**3**	**8**
4	0	0	**0**	**0**
3	0	0	**0**	**0**
2	0	0	**0**	**2**
1	1	0	**1**	**0**
0	0	0	**0**	**5**
Total amount kept	$327	$102	**$429**	**$331**
Total amount donated	$33	$18	**$51**	**$149**
Amount donated as a percentage of the maximum	9.2	15.0	**10.6**	**30.1**

In Treatment 1, with an anonymous recipient, 30 of the 48 subjects chose to keep $10, the entire amount in the envelope; seven kept $9, leaving $1 in the envelope, etc. Four allocated half or more of the $10 to their anonymous partner. The 48 subjects donated a total of $51.

The behavior of the subjects in Treatment 2 was quite different. While Treatment 1 subjects donated on average 10.6% of their payoffs ($51), subjects in the charity treatment donated 31.0% ($149). Subjects in the charity experiments were much less likely to take all $10 for themselves; 27.1% of the subjects kept $10 in the charity sessions, compared to 62.5% in the anonymous-treatment data. Five subjects in the charity treatment donated the full $10; none in the anonymous treatment did so.

In Table II, we report the results of statistical tests on the treatments. These distributions are tested for significant differences using the Epps–Singleton test.[11]

donate.

[11] The Epps–Singleton test is a nonparametric test designed to distinguish between different sample distributions. See Epps and Singleton (1986). Forsythe *et al*. (1994), evaluate the power of this and four other such tests (Cramer–von Mises, Anderson Darling, Kolmogorov–Smirnov, and Wilcoxon rank-sum) and find that the Anderson Darling and Epps–Singleton tests have the most statistical power with ultimatum game data. The Epps–Singleton test is also appealing because it does not require that

TABLE II

Test description:	Epps–Singleton test[a]	
	Statistic	p value
Replication = Hoffman et al.[a]	4.66	0.34
Anonymous vs. Charity (Treatment 1 vs. Treatment 2)[a]	13.09	0.01

Note. Forsythe *et al.* (1994) suggest that for small samples with frequent ties, the Epps–Singleton test performance is poor. They use a tie-breaking technique of adding a random number from the uniform distribution $(0, 0.001)$ to each proposal. We compared our results using unadjusted proposals to proposals adjusted by this tie-breaking technique. No difference in the test statistics was found.

[a] Distributed chi-squared, with four degrees of freedom—critical value for significance of $0.95 = 9.49$.

We first test our replication (column 2 in Table II) against the data from Hoffman *et al.*'s (1994a) Double Blind 1 (column 3). The test statistic, which is distributed $\chi^2(4)$, is 4.66 with a corresponding *p*-value of 0.34; we cannot reject that they are the same distribution. The test statistic for differences between the distributions of the pooled double-blind data (Treatment 1) against the four charity sessions (Treatment 2) is 13.09, statistically significant with a *p*-value of 0.01.

V. DISCUSSION

In double-anonymous dictator games, we observe a change in donations when subjects are given information about the characteristics of the recipient. Our experiments provide evidence that altruism is a motivating factor in human behavior. When an anonymous individual is replaced by an established charity, donations triple; altruistic behavior increases in dictator games when the recipient is a legitimate charity as opposed to an anonymous person who the subjects can infer is drawn from the same population as themselves. Since the experiment is conducted so that subjects' decisions are anonymous to the experimenter, the results cannot be attributed to experimenter effect. The spectator theory of moral motivation argued by Hoffman *et al.* (1994) is not dead, but must be modified to incorporate genuine altruism.

This work also has broader implications for the conduct of economics experiments. It is received wisdom in experimental economics that abstraction is important. Experimental procedures should be as context-free as possible, and the interaction among subjects should be carefully limited by the rules of the ex-

the distributions be continuous. The statistics reported in Table II include the Epps–Singleton small sample correction.

periment to ensure that they are playing the game we intend them to play. For tests of economic theory, these procedural restrictions are critical. As experimenters, we aspire to instructions that most closely mimic the environment implicit in the theory, which is inevitably a mathematical abstraction of an economic situation. We are careful not to contaminate our tests by unnecessary context.

But it is also possible to use experimental methodology to explore the importance and consequence of context. Economists are becoming increasingly aware that social and psychological factors affect economic decision making, and the importance of social factors can only be introduced by abandoning, at least to some extent, abstraction. This may be particularly true for the investigation of other-regarding behavior in the economic arena.

APPENDIX 1: INSTRUCTIONS FOR CHARITY TREATMENT
(Treatment 2)

Instructions.   You have been asked to participate in an economics experiment. For your participation today we have paid you $5 in cash. You may earn an additional amount of money, which will also be paid to you in cash at the end of the experiment.

In this experiment each of you will be paired with the charity: **American Red Cross, Montgomery County Chapter**.

One of the persons in this room is chosen to be the monitor for today's experiment. The monitor will be paid $10 in addition to the $5 already paid. The monitor will be in charge of the envelopes as explained below. In addition the monitor will verify that the instructions have been followed as they appear here.

The experiment is conducted as follows: Fourteen unmarked envelopes have been placed in a box. Twelve of these envelopes contain 10 one dollar bills and 10 blank slips of paper. The remaining envelopes contain 20 blank slips of paper. The monitor will be given a list of names of people in the room. He or she will call one person at a time to the back of the room, and hand each person an envelope from the box. The person who was called will then go to one of the seats, with a large box on top, in the back of the room. The envelope will then be opened privately inside the box.

Each person in this room must decide how many dollar bills (if any) and how many slips of paper to leave in the envelope. The number of dollar bills plus the number of slips of paper must add up to 10. The person then pockets the remaining dollar bills and slips of paper. Example: (1) Leave $2 and 8 slips in the envelope, pocket $8 and 2 slips; (2) Leave $9 and 1 slip in the envelope, pocket $1 and 9 slips. These are examples only, the actual decision is up to each person. Also note that no one else, including the experimenter, will know the personal decisions of people in this room.

Once you have made your decision you will seal your envelope inside the

large box, and then place it in the box at the front marked return envelopes. The monitor will check that your envelope is sealed.

After signing the receipt form which states that you received the $5 appearance fee and giving it to the experimenter, you may then leave the room.

After all fourteen envelopes have been returned the monitor will open the envelopes, record their contents, and count the remaining money. The experimenter will make out a check for this amount to the **American Red Cross, Montgomery County Chapter**, and the monitor will place it in the addressed and stamped envelope. The monitor and the experimenter will go together to the nearest mailbox (in front of Burruss Hall) and drop the envelope in the mailbox. After signing the form which verifies that the experiment was conducted according to the instructions, the monitor is free to leave. The experiment is then over.

MONITOR RECEIPT FORM

NAME _____

ADDRESS _____

_____

TELEPHONE NUMBER _____

I have received the sum of $15 in payment for my participation in an economic experiment conducted by Professors C. Eckel and P. Grossman.

The envelopes distributed in the course of the experiment contained 10 $1 bills and 10 pieces of paper (12 envelopes) or 20 pieces of paper (2 envelopes), and the experiment was conducted according to the stated instructions.

The sum of $_____ was forwarded to the charity: **American Red Cross, Montgomery County Chapter, 185 W. Main Street, Christiansburg, VA 24073.**

Signature _____   Date _____

EXPERIMENTER STATEMENT

The envelopes distributed in the course of the experiment conducted on (DATE) _____ at (LOCATION) _____ each contained 10 $1 bills and the experiment was conducted according to the procedures outlined in the grant proposal.

The sum of $_____ was forwarded to the charity: **American Red Cross, Montgomery County Chapter, 185 W. Main Street, Christiansburg, VA 24073.**

Signature _____   Date _____

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