Cooperation in Public-Goods Experiments: Kindness or Confusion?

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The persistence of cooperation in public-goods experiments has become an important puzzle for economists. This paper presents the first systematic attempt to separate the hypothesis that cooperation is due to kindness, altruism, or warm-glow from the hypothesis that cooperation is simply the result of errors or confusion. The experiment reveals that on average about half of all cooperation comes from subjects who understand free-riding but choose to cooperate out of some form of kindness. This suggests that the focus on errors and "learning" in experimental research should shift to include studies of preferences for cooperation as well. (*JEL* C92, H41)

Theories of free-riding predict that privately provided public goods should have very few contributors, and contributions should be very small. Nonetheless, millions of people give to public goods like the Red Cross and Public Broadcasting, and they generally contribute sizable sums.[1] This observation has caused researchers to reexamine models of giving, and it has become important to understand the role of social and cultural factors like altruism and "warm-glow." These issues extend beyond charitable giving, into public goods within the family and intergenerational altruism (see e.g., B. Douglas Bernheim et al., 1985; Bernheim, 1986; Andreoni, 1989; Joseph Altonji et al., 1992).

As with real-world giving to public goods, experiments on free-riding find that subjects are generally more cooperative than predicted, and often much more cooperative. This suggests that the same social and cultural influences thought to affect real-world giving could be at work in experiments. However, the goal of laboratory experiments is to control the incentives of subjects and to remove the social and cultural influences to the greatest extent possible. If the theory being tested is correct, then the cooperation observed should be due to subjects who misunderstand the instructions or the incentives in the experiment. Hence, the cooperation should be caused only by errors and confusion, and not altruism, warm-glow, or other forms of kindness. If confusion is the principal explanation for cooperation, then this justifies the emphasis on "learning" in the experimental literature.

Unfortunately, it is impossible to tell from existing experiments whether observed cooperation is due to kindness or confusion. For laboratory experiments to be informative on individual motives for giving, it is essential to determine whether there is a significant fraction of giving that is due to kindness. This paper presents an economic laboratory experiment designed to separate kindness and confusion. The experiment strengthens the controls that subtract out the incentives for kindness, leaving confusion as the only explanation for cooperative moves. Comparing subjects in this condition

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[1]See Andreoni (1988a) on how the public-goods model fails to explain privately provided public goods observed in the real world.

to others who can cooperate either out of kindness or confusion, one can determine what fraction of cooperation can be attributed to each motive.

The results of the experiment are, first, that subtracting the incentives for kindness makes subjects far more likely to choose the dominant strategy of free-riding. Overall, contributions to the public good are about one-third the level observed in the usual public-goods experiment. This means that cooperation cannot be attributed to embarrassing amounts of experimenter or subject error. Second, and more importantly, the experiment shows that on average about half of all cooperative moves can be classified as kindness. This implies that social and cultural propensities for kindness and generosity must clearly be very strong, and that such motives cannot easily be removed from experiments simply by providing neutral environments and pledges of anonymity. Third, the results suggest that the decline in cooperation often observed in the multiple trials of public-goods experiments may not be due to learning, but instead may be due to frustrated attempts at kindness. The weight of the evidence now appears to indicate that experiments should focus on detailed studies of charitable behavior Experiments can have a positive role in developing and testing alternative theories of giving.

I. Background

Many experiments have been conducted to test the free-rider hypothesis (see Douglas D. Davis and Charles A. Holt [1993] and John O. Ledyard [1995] for reviews). The most common design requires groups of 4–10 subjects. Each subject is given an endowment which can be "invested" in a public good. Each subject then receives a constant marginal return from each cent invested in the public good, regardless of which subjects invest. The marginal return from the public good is chosen so that each subject has a dominant strategy to invest zero in the public good, that is, to free ride, while the symmetric Pareto-efficient outcome is for all subjects to invest their entire endowment in the public good.

These experiments typically find that subjects are sensitive to free-riding incentives and are generally closer to the free-riding outcome than the Pareto-efficient outcome. Nonetheless, cooperation is still above that which would validate the theory. In a ten-period iterated game, subjects generally begin by contributing about half of their endowments to the public good. As the game is iterated, the contributions "decay" toward the dominant strategy level and stand at about 15–25 percent of the endowment by the tenth iteration (R. Mark Isaac and James M. Walker, 1988). Similar patterns of cooperation are observed in experiments where subjects play in repeated single-shot games created by randomizing the members of groups, rather than in finitely repeated games (Andreoni, 1988b). This suggests that cooperation in these games is not attributable to reputation-building. Prior experience in public-goods experiments does not appear to eliminate cooperation either. Subjects participating in a second public-goods experiment showed significant cooperation as well, regardless of whether the other subjects in their group were the same or different (Isaac and Walker, 1988; Andreoni, 1988b). Subjects are also sensitive to the experimental parameters, even though these do not affect the equilibrium prediction. In particular, people cooperate more the larger the marginal return from the public good (Isaac and Walker, 1988), and counter to the intuition on free-riding, subjects also cooperate more the larger their group (Isaac et al., 1994). They also appear to be more cooperative when the decision is framed as a public good rather than a public bad (Andreoni, 1995). This persistent and sometimes counterintuitive nature of cooperation has created an important puzzle.

II. Kindness or Confusion?

There are two main hypotheses that could explain the lack of free-riding as a dominant strategy in the laboratory. First, one could conjecture that the free-riding hypothesis, in its pure form, is incomplete. Subjects could have tastes for cooperation that they bring from outside of the experiment and which influence their behavior in the experiment.[2] There are many specific alternatives which could be proposed to capture this. Since these would likely appeal to some notions of benevolence or social custom, this paper will refer to these collectively as *kindness*.

A second hypothesis is that the experimenters have somehow failed to convey adequately the incentives to the subjects, perhaps through poorly prepared instructions or inadequate monetary rewards, or simply that many subjects are incapable of deducing the dominant strategy during the course of the experiment. Since this alternative suggests that subjects have somehow not grasped the true incentives, this alternative will be called *confusion*.

Several experiments have examined the kindness hypothesis. These have generally added manipulations that try to influence cooperation in predictable ways. For example, Palfrey and Howard Rosenthal (1991) have added nonbinding "cheap talk," Andreoni (1993) added a "tax" that would be incompletely crowded out in the presence of kindness, and Robyn M. Dawes et al. (1987) added effects for group identification. All of these experiments produced results consistent with various notions of kindness. By contrast, experiments have not attempted to capture the degree of confusion. It is possible, for instance, that the manipulations just mentioned could be influencing confusion in addition to, or perhaps instead of, kindness and real cooperation.

The confusion hypothesis is potentially very important to the experimental literature. Notice that with an equilibrium prediction of zero contribution to the public good there is only one way a confused subject can err, and that is to contribute too much to the public good. Hence, contributions that are really due to confusion may be mistakenly called cooperation.[3] If instead the prediction were some interior choice, so that an error could also lead one to contribute too little, then errors may be more likely to be averaged out of the aggregate data. Indeed, public-goods experiments are strikingly different from other experiments with externalities, such as oligopoly experiments and common-pool resource experiments. These experiments have interior Nash equilibria and get results much closer to the predictions of the theory than do public-goods experiments.[4] It is distinctly possible, therefore, that the dominant-strategy design of the standard public-goods games is biasing experiments toward rejecting the theory.

This paper will directly examine both hypotheses of confusion and kindness. Instead of adding conditions to encourage kindness, the experiments will subtract off the social, cultural, and strategic incentives for subjects to cooperate, leaving confusion as the most reasonable explanation for cooperation. With this methodology I will be able to estimate what fraction of cooperative moves is due to kindness and what fraction is due to confusion.

[2] See Colin Camerer and Keith Weigelt (1988), Richard D. McKelvey and Thomas R. Palfrey (1992), John Neral and Jack Ochs (1992), and Andreoni and John H. Miller (1993) for related discussions regarding finitely repeated games. For a related discussion of the presence of fairness in sequential games see Ochs and Alvin E. Roth (1989), Vesna Prasnikar and Roth (1992), Ernst Fehr et al. (1993), and Robert Forsythe et al. (1994).

[3] This has also been recognized recently by Isaac and Walker (1992), and Palfrey and Jeffrey E. Prisbrey (1992).

[4] Oligopoly experiments find that only four or five subjects are required to generate prices at competitive levels (see Jon Ketcham et al. [1984], Dan Alger [1987], and the summary by Davis and Holt [1993 Ch. 4]). Common-pool resource experiments also find rapid dissipation of the resource, again with relatively small groups (see Walker et al. [1990], Walker and Roy Gardner [1992], and a related study by Charles R. Plott [1983]). I have examined this difference directly (Andreoni, 1995) and found that framing decisions as negative rather than positive externalities greatly reduces cooperation.

III. Experimental Design

The experiment has three conditions. The first, called the *Regular* condition, is the standard public-goods experiment. In a second condition subjects also play a standard public-goods game; however, their monetary payments are not the same as their experimental earnings. Rather, these subjects get paid based on how their experimental earnings rank in comparison to the other subjects in their group. The subject with the highest experimental earnings gets the highest monetary payments, with payments decreasing with rank so that the subject with the lowest experimental earnings gets the lowest monetary payment.[5] If there are ties, those who tie will split the payoffs, keeping the average earnings for each round constant. Note that this payment scheme makes a zero-sum game out of the standard positive-sum public-goods game. This condition will be called the *Rank* condition.

The important feature of the Rank condition is that it preserves the dominant strategy equilibrium of the Regular condition; the way to get the highest rank in the group is to be the biggest free rider, that is, to contribute zero. The Rank condition, however, offers no incentives for cooperation. If three subjects cooperate they can all raise their own experimental earnings, but these three subjects will raise the experimental earnings of the other subjects by even more. Hence, mutual cooperation only assures the cooperators of the lowest possible payoff. Not only are there no monetary gains from cooperation, the potential for kindness or altruism would also appear to be largely eliminated. The incentives for any reciprocal altruism have surely been removed. The zero-sum nature of the Rank payoffs also makes it much less likely that any one subject would consciously wish to make the least amount of money possible: someone has to get the highest Rank payoff—why not me? However, if such selflessness exists it will lead experimenters to overstate confusion. Another possibility is that an interest in equality could lead all subjects to wish to choose identical contributions, so that all subjects get identical earnings. But the focal choice for such an ethic would seem to be zero contributions, since it reaches the goals of equality and is cheat-proof. Hence, this would be unlikely to generate significant amounts of cooperation. Finally, since paying subjects by rank introduces another layer of complexity for subjects it may actually increase the level of confusion. To the extent that this exists, it will increase the estimates of confusion, and as will be seen, it reduces the calculation of kindness.

A potential problem with comparing the Rank condition and the Regular condition is that there are really two differences between them. First, the Rank subjects have information about their rank, while the Regular subjects do not. Second, Rank subjects are paid according to rank, while Regular subjects get paid their experimental earnings. It is possible that the information on rank, apart from the payment by rank, could alter behavior. Giving information about rank, for instance, could sharpen the subjects' focus on the incentives and could help clear up their confusion. Also, the information on rank could distract attention away from natural tendencies for helping one another and direct the attention toward finishing first. Hence, the information alone may squelch some kindness.[6]

For this reason one needs a third condition, called *RegRank*. In this condition subjects get all the same information on their rank (and whether there are ties) that the Rank subjects get, but they get paid according to their experimental earnings, just like the Regular subjects do. Hence, the only difference between Regular and RegRank is the information on rank. This will make it possible to measure the difference in cooperation due only to information on rank.

[5] Paying subjects by their rank was also done by Gary E. Bolton (1991).

[6] It is important to note that the directions were deliberately written to avoid any suggestion of tournament-style behavior. See the Appendix for a copy of the instructions.

Your Cash Earnings Based on Your Rank

	Highest				*Lowest*
YOUR RANK	1	2	3	4	5
YOUR CASH EARNINGS	.95	.87	.80	.73	.65

FIGURE 1. MONETARY EARNINGS FOR SUBJECTS IN THE RANK CONDITION

The RegRank and Rank conditions have all of the same information, but differ on the method of payment. Comparing these two conditions will allow me to focus on the effect of paying by rank alone. As a result, the difference in cooperation between RegRank and Rank will provide a measure of the minimum amount of cooperation that would be attributable to kindness. The cooperation seen in the Rank condition will provide a measure of the minimum amount of cooperation that is attributable to confusion. This leaves the change in cooperation due simply to information on rank (i.e., the difference between Regular and RegRank) which could be attributable to either kindness or confusion.

IV. Results

The public-goods paradigm used here is similar to that used in Andreoni (1988b). Subjects play in groups of five. They are given budgets of 60 tokens in each iteration. A token invested in the private good yields the subject one cent of experimental earnings. A token invested in the public good earns every subject in the group one-half cent of experimental earnings. Hence, investing nothing in the public good is the dominant strategy, while investing everything in the public good is Pareto efficient. The experiments reported below are conducted as follows. On a given day, 40 subjects are recruited from intermediate-level economics classes. The subjects are divided randomly into two rooms of 20 each. In each room a different condition of the experiment is conducted. This is done to maintain the greatest control over random assignments to conditions. In a particular room, the subjects again are assigned randomly to numbered desks. They are given instructions and a packet of ten "investment decision forms," which subjects use to record their decision. One computer and printer is in the back of each room. In each iteration of the game, the experimenter collects the decision forms from each subject and enters the decisions into the computer. The computer is programmed to assign subjects randomly to groups of five and calculate payoffs. It then prints an "earnings report" for each subject. These reports are returned to each subject. The earnings report tells subjects their investment decision, the group's investment in the public good, their experimental earnings, and their monetary earnings. In the RegRank and Rank conditions the earnings report also lists their rank, for example, "Rank 3 tied with 2 others." All of the parameters of the experiment are known to all subjects, but the information on individual payoffs is all private. The subjects are assigned randomly to new groups each iteration. This is important in order to avoid the possibility of reputation-building. Each experiment lasts about 50 minutes, with average earnings of $8.68 per subject. A copy of the subjects' instructions is included in the Appendix of this paper.

Subjects in the Rank condition were given the schedule for payments shown in Figure 1. They were told that if they tied, they would receive the average of the rank payoffs. For instance, if three tied for first rank, they

TABLE 1—PERCENTAGES OF ENDOWMENT CONTRIBUTED TO THE PUBLIC GOOD PER ROUND

	Round										
Condition	1	2	3	4	5	6	7	8	9	10	All
Regular	56.0	59.8	55.2	49.6	48.1	41.0	36.0	35.1	33.4	26.5	44.07
RegRank	45.8	45.4	32.6	25.0	23.1	17.8	11.3	9.5	8.3	9.0	22.79
Rank	32.7	20.3	17.7	9.9	9.2	6.9	8.1	8.3	7.1	5.4	12.55
RegRank – Rank	13.2	25.1	15.0	15.1	13.9	11.0	3.2	1.3	1.2	3.6	10.24
As percentage of Regular	23.5	42.0	27.1	30.4	28.9	26.7	8.9	3.6	3.6	13.5	20.82

TABLE 2—PERCENTAGE OF SUBJECTS CONTRIBUTING ZERO TO THE PUBLIC GOOD PER ROUND

	Round										
Condition	1	2	3	4	5	6	7	8	9	10	All
Regular	20	12.5	17.5	25	25	30	30	37.5	35	45	27.75
RegRank	10	22.5	27.5	40	35	45	50	67.5	70	65	43.25
Rank	35	52.5	65	72.5	80	85	85	85	92.5	92.5	74.50
Kindness:											
Rank – RegRank	25	30	37.5	32.5	45	40	35	17.5	22.5	27.5	31.25
As percentage of 100 – Regular	31.3	34.3	45.5	43.3	60.0	57.1	50.0	28.0	34.6	50.0	43.41
Confusion:											
100 – Rank	65	47.5	35	27.5	20	15	15	15	7.5	7.5	25.50
As percentage of 100 – Regular	81.3	54.3	42.4	36.7	26.7	21.4	21.4	24.0	11.5	13.6	33.33
Either:											
RegRank – Regular	–10	10	10	15	10	15	20	30	35	20	15.5
As percentage of 100 – Regular	–13.0	11.4	12.1	20.0	13.3	21.4	28.6	48.0	53.8	36.4	23.26

would each get paid $(0.95+0.87+0.80)/3 =$ \$0.873. The median rank payoff of \$0.80 was determined on the basis of a pilot study and was set to be equal to the average earnings per round in a pilot run of the RegRank condition. This was done to minimize differences due to income effects. The actual earnings per round for the RegRank condition reported here were \$0.8007. As can be seen, the range of the rank payoffs is 30 cents. This amount was chosen because this is the maximum difference in earnings in any one round between the highest and lowest earnings from the public good in the Regular and RegRank conditions. This means that the difference in earnings for a subject who goes from contributing all of his endowment to contributing none of his endowment is identical regardless of whether subjects are in the Rank, Regular, or RegRank condition.[7] This basic procedure was conducted three times, using 40 subjects for each condition; hence, a total of 120 subjects were used in this experiment.

With this design, subjects in the Regular condition are expected to be the most cooperative, and subjects in the Rank condition are expected to be the least cooperative. Table 1 lists the average percentage of the endowment contributed to the public good in each round. The first thing to note is that the Regular condition conforms to the pat-

[7]To the extent that there may be ties, the marginal difference between contributing all or nothing to the public good may be smaller in the Rank than in the Regular or RegRank conditions. To the extent that this fails to encourage maximizing behavior it will bias downward the estimate of kindness.

tern of earlier experiments, with cooperation in round 1 of 56 percent, decaying to 26 percent by round 10. This compares to nearly identical experiments reported in Andreoni (1988b) in which cooperation went from 51 percent to 24 percent. Next note that, as predicted, Regular subjects are more cooperative than RegRank subjects, and RegRank subjects are more cooperative than Rank subjects. The significance of these differences can be tested with a Mann-Whitney rank-sum U test, which has a normal distribution. This test organizes the data by subjects.[8] It shows that the differences in mean contributions across all three conditions are significant: comparing Regular and RegRank, $z = 3.772$; and comparing RegRank and Rank, $z = 3.580$.

Similar results hold when looking at the number of subjects choosing zero contributions to the public good. The percentage of free riders in any one round is given in the top panel of Table 2. As predicted, Rank subjects free ride the most, and Regular subjects the least. Differences between these conditions are also statistically significant, with U tests of $z = 2.281$ for Regular versus RegRank, and $z = 4.200$ for RegRank versus Rank.

The outcome for the Rank condition is strikingly different from that for the Regular condition. By round 4 the subjects in the Rank condition are contributing less than 10 percent of their endowments to the public good, and by round 10 they are contributing only 5.4 percent, while the Regular subjects never contribute less than 25 percent in any single round. Likewise, by round 2 over 50 percent of Rank subjects free ride, which is a higher rate than the Regular subjects reach in any one round. By the end of the experiment, all but three of 40 Rank subjects (7.5 percent) are free-riding, but 22 of 40 Regular subjects (55 percent) contribute something to the public good. Although there is not complete free-riding among the Rank subjects, the data are much closer to the predicted values than in any other condition. Using the evidence in these two tables one could reasonably conclude that the behavior of the subjects in the Rank condition, unlike the Regular condition, is broadly consistent with the predicted behavior, especially after round 4 of the experiment.

The data in Table 2 permit a closer look at the motivations of the subjects. Recall that the Rank and RegRank conditions are identical except for the method of payment; hence their difference provides an estimate of the number of subjects who understand the incentives but cooperate out of kindness. Likewise, the amount of cooperation in the Rank condition provides a measure of subjects who are confused. The decline in cooperation from Regular to RegRank could be classified as either kindness or confusion, since it is solely due to RegRank subjects receiving information about the rank. The bottom of Table 2 separates cooperation into each of the other three motives for every round. Confusion is by far the dominant motive in round 1 of the experiment, accounting for 81 percent of all cooperation. However, confusion falls rapidly over rounds 1–5 to only 26.7 percent, and then continues in a more slow decline to a mere 13.6 percent in round 10. Kindness, on the other hand, doubles from its round-1 level to its peak in rounds 5 and 6 of around 60 percent. After round 6, however, kindness sputters to its low of 28 percent in round 8 before returning to 50 percent of all cooperation in round 10.

The measures of kindness and confusion in Table 2 suggest an interesting pattern. Over rounds 1–6 the total amount of cooperation is rather stable. However, over the same period the amount of confusion is declining rapidly, and the amount of kindness is increasing. After round 6, confusion is rather stable, but kindness falls. This points to a possible explanation for the "decay" phenomenon often observed in public-goods experiments. When individuals who

[8] The test is conducted by first calculating the mean contribution for each subject and ranking these means for the joint sample. Under a null hypothesis of no difference between conditions, the sum of the ranks should be equal across conditions (see John E. Freund, 1971 pp. 347–49).

start off confused finally learn the dominant strategy, it appears that they may first try to cooperate but then eventually turn to free-riding.[9] This could suggest that, for some subjects, kindness may depend on reciprocity.

Note that Table 2 also reveals that a number of subjects could not be classified as either cooperating from kindness or confusion. This number is relatively stable at about 10–15 percent, until round 7 when it roughly doubles. Surely some of these subjects belong in the kindness category and some in confusion. A conservative approach would be to classify all of these subjects as confused. This means that the kindness measured in Table 2 is a lower-bound estimate of the amount of kindness present. Hence, combining the confusion and the "either" categories, one could say that, on average, cooperation is about 43 percent kindness and 57 percent confusion. Alternatively, one could get an upper-bound estimate of kindness by combining the "either" category with the kindness category. Doing this, we find that cooperation is no more than 67 percent kindness on average, with 33 percent confusion. A rough characterization of these findings is that cooperation is about half kindness and half confusion.

To obtain a different measure of confusion, all subjects were also given a postexperiment questionnaire which was designed to determine whether subjects understood the incentives. Subjects were presented with two hypothetical situations similar to those that they could encounter in the experiment and were asked what choice would yield the highest experimental earnings. They were also asked for verbal descriptions of their strategies. In each condition, exactly two subjects failed to answer these questions correctly. Hence, there were no systematic differences across conditions in the ability to discern incentives by round 10. All of the errant subjects in the Regular and RegRank conditions were also cooperators in round 10. Of the two Rank subjects who erred on the questionnaire, one was a cooperator in round 10, and one was not. Two other Rank subjects who did cooperate in round 10 were able to answer the questionnaire correctly. For one of these subjects the questionnaire itself may have cleared up some confusion. In the other's verbal explanation of his strategy, however, the subject indicated that he chose the dominant strategy for the first half of the experiment, but then switched to giving all his endowment to the public good in order to "give others a chance." For this subject, a clear motive of kindness is classified as confusion in Table 2. On balance, however, the amount of confusion shown in the table for round 10 generally corresponds to the results of the direct questionnaire.

There is a final surprising contrast that can be found between Tables 1 and 2 concerning the RegRank condition. As seen in Table 1, over rounds 7–10 the fraction of the endowment contributed to the public good by RegRank subjects is very close to that contributed by the Rank subjects, 9.5 percent versus 7.3 percent, while it is far from the fraction contributed by Regular subjects, who contribute 30.2 percent. In Table 2, by contrast, the fraction of subjects who contribute *something* to the public good over rounds 7–10 is 36.9 percent in the RegRank group, which is almost exactly halfway between the 63.1 percent in the Regular group and the 11.3 percent in the Rank group. Hence, conditional on giving at all, the average contribution of the RegRank contributors is actually lower than that of the Rank contributors. This means that information about rank decreases the amount given much more than it decreases the number of givers. It is unclear what this implies about the way information affects kindness and confusion, but it remains a striking puzzle that future work may address.

[9]A stricter view of confusion would assume that a person is confused if that person cooperates at any time in the future, even if he or she does not cooperate in the current period. An earlier version of this paper (available from the author upon request) also considers this definition, and the results are very similar to those reported here.

V. Discussion

The significant presence of both kindness and confusion in public-goods experiments suggests that both merit greater consideration. Kindness in experiments corresponds to a large body of evidence from privately provided public goods, like charitable giving, which indicates that people contribute more than the theory predicts. Several alternative models have been suggested to explain this, and these models could be adapted to experimental environments to help inform the theory. For instance, one hypotheses is that subjects may be purely altruistic, that is, they care directly about the payoffs of the other subjects. A more general hypothesis is that subjects also care about the act of being nice to each other, that is, they are "warm-glow" givers (Andreoni, 1989, 1990). Other alternative models are based on moral arguments, such as reciprocity (Robert Sugden, 1984), group ethics (Howard Margolis, 1982), and fairness (Matthew Rabin, 1993). These models could be examined experimentally.

The significant presence of confusion presents a much different challenge to experimenters. Confusion is especially apparent in this experiment because errors can only be in one direction and, hence, will not be averaged out of the aggregate data. This suggests that experiments with interior equilibria could potentially overstate the extent to which subjects understand incentives. Since games with interior equilibria generally do not have dominant strategies, it is much more difficult to classify exactly when a subject is making an error. An assumption of no error may mistakenly lead experimenters to be overly confident of their theories.

One example of this is illustrated in a recent public-goods experiment published in this *Review* (Andreoni, 1993).[10] This experiment offered subjects a payoff matrix for a public good for which there was an interior Nash equilibrium. One matrix, however, reflected "taxation," and the tax revenue was added to the public good. If subjects either have altruism toward other subjects or get warm-glows from giving, then theory suggests that the subjects with the tax should provide more public goods than the subjects without the tax. The alternative of no altruism or no warm-glow predicts the same equilibrium contribution in both conditions. The experiment revealed that there was indeed a significant difference between these conditions, indicating a presence of altruism or warm-glow. However, neither condition by itself was significantly different from the no-warm-glow equilibrium prediction, even though the two conditions were significantly different from each other. Hence, if either condition were conducted in isolation, the experimenter might mistakenly conclude that altruism or warm-glow is not present.

This fact may also reconcile the standard public-goods experiments with the broader literature on externalities mentioned in Section II. Experiments on externalities, which have interior equilibria, generally cannot reject the theory. The results of the current paper raise the possibility that the confusion in these experiments may create enough variance in the data to mask any influence of kindness. If controls are added in an effort to manipulate or measure kindness, then perhaps it will be identified.

Finally, the presence of kindness in public-goods experiments is consistent with evidence for fairness found in bargaining experiments. In particular, Forsythe et al. (1994) compare ultimatum and dictator games and find a significant tendency for people in dictator games to give away money, even when there is not the threat of retribution found in ultimatum games. Since the dictator game is not very confusing, this generosity is thought to be due to kindness. In a related study, Bolton (1991) followed up on a study by Ochs and Roth (1989) in which bargainers often made counteroffers that were worse for themselves than offers they had already rejected. Bolton found that a rank-order treatment substantially reduced these "disadvantageous counterpro-

[10] See Kenneth Chan et al. (1993) for a replication of this result.

posals," again indicating that fairness, as well as some confusion, may be at play in bargaining experiments.

VI. Conclusion

The persistent and sometimes counterintuitive nature of cooperation in public-goods experiments has presented an important puzzle for economists. In general, laboratory experiments are designed to control the incentives of subjects and to restrict social and cultural influences. Hence, many experimenters have focused on learning hypotheses as potential explanations for cooperation. In contrast, studies of giving and cooperation that are based on real-world data have increasingly focused on social influences, such as fairness and warm-glow, to understand giving behavior. In order to use experiments to learn about giving in real situations it is important to understand whether the experiments are indeed identifying only confusion by subjects, or whether kindness is also fundamental to the strategies.

The experiment presented in this paper is the first systematic attempt to separate the hypotheses of kindness and confusion. It reveals that on average about 75 percent of the subjects are cooperative, and about half of these are confused about incentives, while about half understand free-riding but choose to cooperate out of some form of kindness. This demonstrates that kindness and confusion are equally important in generating cooperative moves in public-goods experiments and suggests that the focus on "learning" in experimental research should shift to include studies of preferences for cooperation.

It is important to note that laboratory experiments are designed to be neutral and to minimize social effects like kindness. Hence, regular public-goods experiments may already be eliminating a large amount of subjects' natural tendency to be cooperative. In the real world a much larger fraction of people may naturally be cooperative than this experiment indicates. Admittedly, the stakes for kindness are often higher in the real world, so comparisons cannot be direct. Nonetheless, the striking importance of these effects in the laboratory and the parallel of these findings with real-world evidence on giving point to a promising area of research. Is it possible to test alternative models of kindness in the laboratory as well as with real world data?

One should also note the importance of confusion. Most of the learning in this experiment was accomplished in the first five rounds. However, this reduction in confusion was replaced by a growth in kindness, leaving total cooperation fairly stable. The movement toward the equilibrium in the last half of the experiment appeared to be due to frustrated attempts at kindness, rather than learning the free-riding incentives. This, rather than learning per se, could explain the decay of cooperation often observed in public-goods experiments.

In summary, this paper goes beyond showing that subjects tend to cooperate too much in free-finding experiments; it identifies the part of this cooperation that needs explanation with behavioral models, and the part that may be due to methodological issues in experiments. The findings of this experiment indicate that future research, both theoretical and experimental, should focus on developing reliable predictive models of charitable and altruistic behavior.

Appendix: Subjects' Instructions [Exact Transcript]

WELCOME

This experiment is a study of group and individual investment behavior. The instructions are simple. If you follow them carefully and make good investment decisions you may earn a considerable amount of money.

The money you earn will be paid to you, in cash, at the end of the experiment. A research foundation has provided the funds for this study.

THE INVESTMENT OPPORTUNITIES

You have been assigned to a group of 5 people. Each of you will be given an investment account with a specific number of tokens in it. These are then invested to turn them into cash. *All tokens must be invested to earn cash from them.*

You will be choosing how to divide your tokens between two investment opportunities:

1. The Individual Exchange

Every token you invest in the Individual Exchange will earn you a return of one cent.

Example. Suppose you invested 55 tokens in the Individual Exchange. Then you would earn $0.55 from this exchange.

Example. Suppose you invested 148 tokens in the Individual Exchange. Then you would earn $1.48 from this exchange.

Example. Suppose you invested 0 tokens in the Individual Exchange. Then you would earn nothing from this exchange.

2. The Group Exchange

The return you earn from the Group Exchange is a little more difficult to determine.

What you earn from the Group Exchange will depend on the *total number of tokens* that you and the other four members of your group invest in the Group Exchange. The more the *group* invests in the Group Exchange, the more *each member of the group* earns. The process is best explained by a number of examples:

Example. Suppose that you decided to invest no tokens in the Group Exchange, but that the four other members invested a total of 100 tokens. Then your earnings from the Group Exchange would be $0.50. Everyone else in your group would also earn $0.50.

Example. Suppose that you invested 40 tokens in the Group Exchange and that the other four members of your group invested a total of 80 tokens. This makes a total of 120 tokens. Your return from the Group Exchange would be $0.60. The other four members of the group would also get a return of $0.60.

Example. Suppose that you invested 60 tokens in the Group Exchange, but that the other four members of the group invest nothing. Then you, and everyone else in the group, would get a return from the Group Exchange of $0.30.

As you can see, every token invested in the Group Exchange will earn one half of a cent for every member of the group, not just the person who invested it. *It does not matter who invests tokens in the Group Exchange. Everyone will get a return from every token invested—whether they invest in the Group Exchange or not.*

The table on the following page [Table A1 in this Appendix] can be used to help you calculate your earnings from the Group Exchange.

THE INVESTMENT DECISION

Your task is to decide how many of your tokens to invest in the Individual Exchange and how many to invest in the Group Exchange. You are free to put some tokens into the Individual Exchange and some into the Group Exchange. Alternatively, you can put all of them into the Group Exchange or all of them into the Individual Exchange.

STAGES OF INVESTMENT

There will be 10 decision rounds in which you will be asked to make investment decisions. At the end of each round your payoff will be recorded by the experimenter. After the last round you will be paid the total of your payoffs from all 10 rounds.

TABLE A1—RETURNS FROM THE GROUP EXCHANGE

Total investment by your group	Return to each member of your group
0	0
10	5
20	10
30	15
40	20
50	25
60	30
80	40
100	50
120	60
140	70
160	80
180	90
200	100
220	110
250	120
280	140
310	155

At the beginning of each round you will be given a fresh investment account. You will also be given an INVESTMENT DECISION FORM. You are to record your decision using this form. Be sure that your investment in the Individual Exchange plus your investment in the Group Exchange equals the number of tokens in your account. You must make your investment decisions *without* knowing what the others in your group are deciding.

Do not discuss your decision with any other participant!

The experimenter will collect the form when you have filled it out. The experimenter will then calculate your earnings from the Individual and Group Exchanges, and calculate your total payoff. This information will be conveyed to you on an EARNINGS REPORT.

IMPORTANT NOTICE: The Earnings Report tells you the total investment in the Group Exchange and your personal earnings. It will also tell you where your investment earnings ranked in comparison to the other 4 members of your group. 1 is the highest rank, and 5 is the lowest rank. In case of ties for rank, the highest number will be reported. Your earnings report does not tell you the investment decisions or earnings of the other members of your group. YOUR INVESTMENT DECISIONS AND EARNINGS ARE CONFIDENTIAL.

YOUR INVESTMENT ACCOUNT

The number of tokens in your Investment Account is indicated on your Investment Decision Form. You and every other member of your group will have 60 tokens in your investment account each decision round. The total number of tokens in each group in every decision round is 300.

YOUR GROUP

The composition of your group will be changing *every* decision round. After each decision round you will be **reassigned** to a **new group** of 5 participants. The 5 group members will never have been members of the same group in the past. The chance that any other participant will ever be in a group with you more than one time is very small.

At no point in the experiment will the identities of the other members of the group be made known to you, nor will your identity be made known to them.

YOUR PAYOFF

Your monetary payoff from your investment *will not* be the same as your investment earnings. Instead, your payoff from each investment decision will depend on how your investment earnings compare to the investment earnings of the other subjects in your group. If your investment earnings are the highest among the 5 subjects in your group, then your payoff will be \$0.95. If your earnings are second highest, your payoff will be \$0.87. If your earnings are third highest, your payoff will be \$0.80. If your earnings are fourth highest, your payoff will be \$0.73. If your earnings are fifth highest, your payoff will be \$0.65. For example, suppose five subjects in your group had investment earnings of 100, 80, 60, 40, and 20. Then they would receive payoffs of \$0.95, \$0.87, \$0.80, \$0.73, and \$0.65, respectively. If two people have the same investment earnings—so they have the same rank—then they will earn the average payoff from the tie. For example, suppose the second and third highest investors both earned 70 from their investments. Then each of them would receive a payoff of $(0.87+0.80)/2=\$0.835$. Suppose instead that the first, second and third highest investment earnings were all equal to 75. Then all three players would receive a payoff of $(0.95+0.87+0.80)/3=2.62/3=\$0.873$.

The following table can help you determine your payoff:

Your Cash Earnings Based on Your Rank

	Highest				*Lowest*
YOUR RANK	1	2	3	4	5
YOUR CASH EARNINGS	0.95	0.87	0.80	0.73	0.65

GOOD LUCK!

You may begin by completing the first Investment Decision Form.

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