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Motives for Giving: A Reanalysis of Two Classic Public Goods Experiments

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Experimental work in economics prompted the development of theories of other-regarding behavior. In this article we reanalyze two classic public goods experiments and focus on the nature of individuals' responses to others' behavior in order to help distinguish alternative motives for giving, including altruism, warm glow, reciprocity, and inequality aversion. Analysis that allows for asymmetric feedback responses generates support for inequality aversion motives but little for reciprocity (matching), altruism, and warm glow. We conclude that individual-level analysis of existing public goods data can provide more insightful, informative estimates of treatment effects.

JEL Classification: C9, H41, Z13

1. Introduction

One of the primary social problems studied by economists is how to fund the provision of public goods. What makes the public goods problem so interesting is the tension between the theoretical equilibrium prediction that the level of voluntary contributions should be zero and the common result in the lab and in the field that public goods are nevertheless produced. The theorists' response has been to develop and calibrate new theoretical models that are consistent with observed behavior across a variety of games including the public goods game (for example, Andreoni 1989, 1990; Fehr and Schmidt 1999; Bolton and Ockenfels 2000).

Experiments have been used to investigate the factors affecting levels of public good provision and to examine alternative theories of giving. For the most part, experimental research has focused on the Voluntary Contribution Mechanism (VCM), where the quantity of the public good is a linear function of contributions, as a vehicle for examining the public goods problem (see Ledyard 1995 for a survey). Experimental results typically show contributions that are substantially above what theorists expect, but that nevertheless fall below socially optimal levels. Much has been discovered about average behavior in many variations of this

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game, and regularities in behavior from VCM experiments and other types of games have provided fodder for the development of new theories.

In this article we mine the archive of data from two classic studies in search of evidence to support or refute theories about the motives for individual behavior. These two studies were originally designed with specific research goals in mind and focused on the response of aggregate giving to differences in the experimental parameters. The focus of the field is now reoriented toward the study of individual-level behavioral motivations. Our work highlights the extent of subjects' response to information about the behavior of other group members and the implications of the resulting dynamic patterns in the data for proposed theories. As with many studies using field data, we examine new theories using data sets collected for a different purpose. An obvious advantage to this approach is that new analysis of old data also provides a low-cost alternative to new experiments for distinguishing among theories.

The nature of subjects' responses to others' decisions can help distinguish alternative motives for giving. Pure altruism implies that, because subjects care only about the total amount of the public good produced, they will decrease their contributions in response to an increase in giving by others (Andreoni 1990). Pure warm glow predicts that a subject will be unresponsive to the donations of others, because they care only about their own contribution to the public good. Reciprocity (Gintis 2000, ch. 11; Dufwenberg and Kirchsteiger 2004) suggests that subjects may try to coordinate on a level of giving; that is, as Kurzban et al. note, "players are generally willing to contribute to the public good only if others are doing so at similar levels" (2001, p. 1665). Inequality aversion (Fehr and Schmidt 1999; Bolton and Ockenfels 2000) also implies that, with common endowments, subjects will seem to coordinate on a common level of giving.

The difference between the implications of the latter two has to do with the asymmetry of response. Theories of reciprocity predict only that agents will try to do what other members of their group do. In contrast, the Fehr and Schmidt theory of inequality aversion can be distinguished from general reciprocity in that it allows for the possibility that agents will respond differently when their contributions are above rather than below average. To preview, our results from analysis of the dynamics in the data support reciprocity in the form of inequality aversion as a motive for giving but little for reciprocity in the form of matching of others' contributions, less for warm glow, and least for pure altruism.

Experimental studies are often designed in order to distinguish between alternative theories. This type of experiment is designed so that different motivations produce different decisions. An excellent example of such a study is Engelmann and Strobel (2004), where subjects make choices among several possible allocations for themselves and two other persons. The allocations are designed specifically to parse motives for giving. Our research approach is different and has more in common with studies using observational data. We examine these theories using data sets collected for a different purpose. The data sets we choose for this project are from well-cited works examining aspects of public goods giving, enhancing the value of revisiting and reanalyzing their results in light of theoretical and econometric advances in the field. An obvious advantage to this approach is that new analysis of old data also provides a low-cost alternative to new experiments for distinguishing among theories.

Of course, we are not the first to study individual effects or dynamics in VCM data.[1] Solow and Kirkwood (2002) look for relationships between gender, group identity, and

[1] Harrison (2007) takes a different approach to reanalyzing experimental panel data, using population-averaged estimation methods known as Generalized Estimating Equations to examine the impact of house money on behavior.

contributions; they account for censoring but not dynamics. Other studies have shown that manipulating information can significantly affect the level and dynamic pattern of contributions. Selective announcement of information about others' contributions (as in Harbaugh 1998; Kurzban et al. 2001; Houser and Kurzban 2005), use of "robot" or predetermined decisions to vary the feedback subjects receive (for example, Ferraro and Vossler 2010) or the announcement of an initial large contribution (Vesterlund 2003; Andreoni 2006) can substantially affect subjects' actions. Gunnthorsdottir, Houser, and McCabe (2007) consider dynamics and censoring but adopt a limited specification of individual effects. Croson, Fatas, and Neugebauer (2005) compare giving in VCM and weakest link mechanism games, and they find strong evidence that subjects attempt to match others' contributions. Chaudhuri, Graziano, and Maitra (2006) study the effect of intergenerational advice in a linear VCM. Bardsley and Moffatt (2007) use data from a unique variation on the VCM to estimate a mixture model to distinguish altruists from reciprocators.

The analysis in the articles by Chaudhuri, Graziano, and Maitra (2006), Croson, Fatas, and Neugebauer (2005), Ferraro and Vossler (2010), and Houser and Kurzban (2005) is most similar to our own, and the paper by Bardsley and Moffatt (2007) has a related structure. These authors econometrically model subjects' reciprocity, that is, their response to information about previous period outcomes. All find that giving is positively related to the giving of others for at least a fraction of subjects. Importantly, however, none of these studies allow for asymmetric response by subjects to situations where they are above or below average giving. Allowing for asymmetric response is necessary to permit testing of the full range of theories we examine.[2]

A secondary contribution of our approach is that it allows us to test for differences in dynamics induced by the common practice of randomly rematching subjects.[3] This methodology was developed in order to induce strategic independence of decisions from period to period. We find strong dependence between rounds for the randomly rematched sample, suggesting that this technique fails at its intended purpose.

2. Motives for Giving

The Voluntary Contribution Game is structured as follows. Subjects typically are divided into groups of 2 to 10 persons. Each subject is given an endowment that must be allocated between a private account and a group account. The private account provides a fixed return per unit to the individual. Contributions to the group account pay to each member of the group the marginal per capita return (MPCR) times the total of the group's contributions. For example, for each dollar invested, the private account might return \$1, while the group account returns \$0.50 to each member of the group. The game is repeated for a number of rounds.

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[2] Keser and van Winden (2000) perform a simple but insightful analysis in the same spirit as this article, treating responses to above- and below-average contributions separately. They count the number of times subjects respond to each situation by increasing or decreasing contributions, and they perform nonparametric tests on the count. Although they find a symmetric response to information that a subject is above or below average in partners conditions, in the strangers condition (with rematching) more subjects respond when finding they are above average than when they are below average. Their approach does not consider the magnitude of responses but rather only the direction.

[3] See Andreoni and Croson (2008) for a survey of studies comparing fixed "partners" with randomly rematched "strangers."

Experimental research on public goods has focused largely on the VCM (see Ledyard 1995). Subjects are divided into groups and given an endowment to allocate between a private and a group account. The private account provides a fixed return; contributions to the group account pay to each member MPCR times the total of the group's contributions, with MPCR $> 1/n$, where n is the number of subjects. This creates a tension between socially optimal and payoff-maximizing strategies.

For a purely self-interested player, the dominant strategy in the VCM is to contribute zero. Because most subjects make positive contributions, it is clear that they analyze the game differently or have different motives. We focus on four such motives—altruism, warm glow, and two types of reciprocal behavior—matching others' contributions and aversion to inequality. Although the models themselves are static, we make inferences about these motives based on subjects' response to giving in previous periods. In other words, we propose decision makers who adapt their behavior in response to the behavior of others.

We estimate the following model of individual contributions in a period, $C_{i,t}$:

$$C^*_{i,t} = \alpha + \lambda_1 \max[C_{i,t-1} - \overline{C}_{i,t-1}, 0] + \lambda_2 \min[C_{i,t-1} - \overline{C}_{i,t-1}, 0] + \sum_{j=1}^{2} \rho_j C_{i,t-j}$$

$$+ \sum_j \beta_j X_{ij} + \sum_j \omega_i z_{ij,t} + \sum_{j=1}^{M} \gamma_j D_{ij} + \varepsilon_{i,t},$$

where i indicates the individual and t the period, α is a constant, and $\varepsilon_{i,t} \sim NIID[0, \sigma^2_\varepsilon]$.

If player i's contributions are above (below) the average contributions by others in their group, $\bar{C}_{i,t-1}$, then they may adjust their contributions to match the group average: λ_1 (λ_2) captures this adjustment. Contributions in earlier periods, $C_{i,t-1}$ and $C_{i,t-2}$, etc., capture autocorrelation, X_{ij} models the jth treatment in each study, and z_{ijt} represents additional explanatory variables, as explained below. Subject fixed effects control for subject type and are represented by D_{ij}, where M is the number of subjects and $D_{ij} = 1$ for $j = i$ and zero otherwise. In this section we discuss λ_1 and λ_2 only; other variables are discussed below.

Following Andreoni (1989), individuals may be motivated by *altruism*, the extent to which they care about the level of public goods production, and *warm glow*, their own responsibility for production of the public good. Pure altruism means that individuals care only about the total amount of public good produced. This implies that an increase in contributions by others will be offset by a reduction in individual contributions; that is, a higher level of others' contributions in period $t-1$, $\bar{C}_{i,t-1}$, which carries a negative sign in the formulation above, is met by a decrease in own contributions: $\lambda_1 = \lambda_2 > 0$. These parameters are positive because the lagged others' contribution term carries a negative sign in the formulation and equal because the decision maker does not distinguish between situations where his own contribution is above or below average.

On the other hand, pure warm glow implies that individuals care only about their own level of contributions and are unresponsive to changes in others' contributions. This means that the coefficients in the deviation from averages are zero: $\lambda_1 = \lambda_2 = 0$.

Reciprocity suggests that people respond in kind to friendly or unfriendly acts of others (Fehr and Gaechter 2000). In our context, friendly and unfriendly refer to the relationship between own and others' contributions within the group. Positive reciprocity implies that subjects will donate more when the other members of their group also donate, and negative reciprocity implies that subjects will reduce contributions when others' contributions are low. Their simultaneous presence, also called *matching* or conditional cooperation (Croson, Fatas, and Neugebauer 2005), suggests coordination on a level of giving. Coordination on a level of giving implies that $\lambda_1 = \lambda_2 < 0$, where a subject responds by trying to exactly match others' average giving level, increasing (decreasing) his contributions when they are above (below) average.

A weaker form of reciprocity allows both to be negative but relaxes the requirement of equality. Fehr and Schmidt (1999; FS hereafter) argue that subjects are averse to earnings that are higher or lower than others; that is, they are *inequality averse*. Their model distinguishes "unfavorable inequality," when own income is below the income of others, from "favorable inequality," when own income exceeds others and allows these to have asymmetric marginal utilities. Note that, if a subject gives more than others, then her payoff in the game is lower than others in her group, which corresponds to unfavorable inequality. In this case the FS model implies that a subject who is averse to unfavorable inequality would respond by reducing donations in order to move earnings toward equality. Similarly, if a subject gives less than others, his earnings are higher, a situation of "favorable inequality," and a subject who is averse to favorable inequality would give more to equalize earnings in his group. Their theoretical formulation of inequality aversion permits asymmetric responses to differential giving; individuals typically care more, and so respond more, to cases where they find that they are giving more than others (unfavorable inequality) than cases where they are giving less (favorable inequality).[4]

In the VCM environment this translates into predictions about how individuals will respond when they discover that their contributions are above or below the average of the group. A subject whose contributions are above average will earn more and so adjust downward, implying a negative value for λ_1. But a subject whose contributions are below average will earn more and will adjust giving upward more slowly or not at all. Although both parameters are negative, this asymmetric adjustment implies that λ_1 is larger in absolute value. In sum, if $\lambda_1 < \lambda_2$, (keeping in mind that both are negative), then this is consistent only with asymmetric inequality aversion among the motives we consider.

Both matching and inequality aversion are special cases of reciprocity. In the first, the subject responds symmetrically to differences between their own and average contributions; in the latter, the subject responds more when her contributions are above than below average.

3. Data Analysis and Specification

We chose data sets from two classic studies, Isaac and Walker (1988; IW hereafter) and Andreoni (1995). IW examine group size (4 or 10) and marginal per capita return (MPCR) (0.3 or 0.75), using a 2×2 within-subjects design (20 periods, 84 subjects). Andreoni tests the effect of positive and negative framing on contributions. His is a between-subjects design (10 periods, 80 subjects). Neither study analyzes variation across individuals or time.

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[4] Bolton and Ockenfels (2000) do not accommodate this asymmetry.

We specify a panel regression model for $C_{i,t}$ expressed as a fraction of the subject's total period t endowment, with double censoring. $C^*_{i,t}$ depends on observable explanatory variables and an unobserved error term $\varepsilon_{i,t}$. For IW, treatments are captured by *MPCRhi*, a dummy variable equal to 1 for the high-MPCR sessions, and *Group size*, which takes on the values 4 or 10. The dummy variable *First 10 periods* was set to one in the first set of 10 periods. The Andreoni data include a variable for the positive frame treatment, *Positive Frame*. For both data sets, we include two periods of lagged own contributions.[5] The variable $C_{i,t-1} - \bar{C}_{i,t-1}$ is denoted *Deviation from Group*(+) when it is positive and is otherwise zero. *Deviation from Group*(−) is similarly defined for negative values. These variables allow for asymmetry in the responses to distinguish inequality aversion from other motives for giving. Finally, because the degree of subject heterogeneity is itself of interest, we include individual fixed-effects dummy variables. Estimation results for both fixed-effects and random-effects regression models are presented and discussed below. However, they do not differ substantially.

Table 1 contains descriptive statistics on these variables for both data sets. Considering how different these two experiments are, the descriptive statistics are remarkably similar. Although average contributions appear to be different—they are 36.0 percent of the endowment in the IW data, and 24.9 percent in the Andreoni data—contributions in his positive frame treatment are 33.6%, very close to the IW data. The lower average contributions in the Andreoni data are thus due to the negative frame treatment and do not threaten the comparability of the data sets. The average change in contributions in each period, $c_{i,t}$, is identical in both data sets at −0.03, indicating that contributions typically fall by three percentage points per round. The deviations from group variables also are similar in the two data sets—0.145 compared with 0.124—indicating a similar range of contributions for both studies. The strong similarity in the descriptive statistics makes us confident that we can make comparisons across the two studies.

Censored panel regression results are contained in Table 2 (fixed-effects regression) and Table 3 (random-effects regressions).[6] Censoring is substantial in both data sets. In addition, 39.4% and 39.2%, respectively, of the fixed-effects coefficients are significantly different from zero, and the joint null hypothesis that all fixed effects are zero is easily rejected, indicating considerable heterogeneity in subject type. For both data sets, the coefficients on own lagged contributions for one and two periods ($C_{i,t-1}$, $C_{i,t-2}$) are positive and significant, indicating that there is stickiness in individual contributions.

Because of the censoring, care must be taken in interpreting the estimated coefficients. The coefficients can be interpreted as the "desired" response of the dependent variable (for example,

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[5] Additional periods did not yield significant coefficients. See Ashley, Ball, and Eckel (2003) for additional specifications.

[6] These models are estimated using Stata routines 'cnreg' and 'xttobit', respectively. Fixed-effects regression with lagged dependent variables is known to yield inconsistent estimators, but the estimates of the fixed effects themselves are informative. Random-effects regression estimators are generally more efficient in large samples, and (in the non-censored case, at least) the inclusion of such lagged variables in a random-effects regression is now known to still yield consistent estimation if the remaining regressors are strictly exogenous, as is reasonable to assume in an experimental setting: See Ashley (2010). On simplicity grounds, we prefer the fixed-effects regression estimates, but the random-effects estimates and estimated standard errors are quite similar and lead to qualitatively identical conclusions; both are included for completeness. The first-differences estimator is not an attractive option here—with a limited number of subjects—because the difference itself uses up an additional observation for each subject and additional data are typically used up in constructing instruments for the change in $C_{i,\,t\text{-}1}$.

Table 1. Descriptive Statistics

Variable		N	Mean	SD	Min	Max
A: Isaac-Walker data						
$C_{i,t}$	Fraction of endowment contributed to public good	1680	0.360	0.370	0	1
$c_{i,t}$	Change in contribution to public good: $C_{i,t-1} - C_{i,t}$	1512	−0.030	0.319		
Group size	Size of group: (4 or 10)	1680	8.286	2.711	4	10
MPCRhi	Dummy variable = 1 for high MPCR treatment	1680	0.494	0.500	0	1
Deviation from Group(+)$_{i,t-1}$	$C_{i,t-1}$ less (average contribution by others in group)$_{t-1}$, if >0	1512	0.145	0.216	0	0.907
Deviation from Group(−)$_{i,t-1}$	$C_{i,t-1}$ less (average contribution by others in group)$_{t-1}$, if <0	1512	−0.145	0.199	−0.933	0
$C_{i,1}$	Initial contribution of player i	1680	0.442	0.372	0	1
First	Dummy variable = 1 for first 10 periods	1680	0.500	0.500	0	1
Round Number	Round in experiment (1–20)	1680	10.500	5.768	1	20
B: Andreoni data						
$C_{i,t}$	Fraction of endowment contributed to public good	800	0.249	0.330	0	1
$c_{i,t}$	Change in contribution to public good: $C_{i,t-1} - C_{i,t}$	720	−0.030	0.317		1
Positive Frame	Dummy variable = 1 for positive frame treatment	800	0.500	0.500	0	1
Deviation from Group(+)$_{i,t-1}$	$C_{i,t-1}$ less (average contribution by others in group)$_{t-1}$, if >0	800	0.124	0.235	0	100
Deviation from Group(−)$_{i,t-1}$	$C_{i,t-1}$ less (average contribution by others in group)$_{t-1}$, if <0	800	−0.124	0.169	−0.917	0
$C_{i,1}$	Initial contribution of player i	800	0.377	0.379	0	1
Round Number	Round in experiment (1–10)	800	5.500	2.874	1	10

Table 2. Censored Panel Regression (Fixed-Effects Regression Results)[a]

Variable	Isaac-Walker Data	Andreoni Data
$C_{i,t-1}$	0.848*** (0.110)	0.939*** (0.164)
$C_{i,t-2}$	0.150** (0.051)	0.428*** (0.074)
First 10 rounds	0.046 (0.027)	—
Deviation from Group(+)	−0.649*** (0.132)	−0.607*** (0.189)
Deviation from Group(−)	0.016 (0.132)	0.045 (0.178)
MPCR = HI	0.569*** (0.112)	—
Group size	−0.011* (0.004)	—
MPCRhi × Group size	−0.041*** (0.011)	—
Positive Frame (=1 for this treatment)	—	$0.217^{\#}$ (0.094)
Constant	−0.103 (0.203)	−0.570*** (0.072)
Fixed effects:		
Smallest p value	$p < 0.001$	$p < 0.001$
% p values < 0.05	34.2	29.4
p value for H_o: all 0	$\chi^2_{(79)} = 199.0^{b}$ $p < 0.001$	$\chi^2_{(51)} = 70.0^{b}$ $p = 0.040$
Fraction censored at 0%	0.362	0.512
Fraction censored at 100%	0.134	0.055
N	1312	640
Pseudo R^2	0.339	0.35
LLF	−829.8	−343.5

[a] Dependent variable $C_{i,t}$, contribution levels: fraction of endowment contributed to public good; standard errors in parentheses.
[b] This test is compared to a Tobit model omitting the fixed effect dummy variable.
# $p < 0.05$.
* $p < 0.01$.
** $p < 0.005$.
*** $p < 0.001$.

Table 3. Censored Panel Regression (Random-Effects Regression Results)

Variable	Isaac-Walker Data	Andreoni Data
$C_{i,t-1}$	0.901*** (0.106)	0.899*** (0.154)
$C_{i,t-2}$	0.271** (0.055)	0.292*** (0.079)
First 10 rounds	0.047 (0.028)	—
Deviation from Group(+)	−0.601*** (0.129)	−0.744*** (0.178)
Deviation from Group(−)	0.132 (0.124)	0.018 (0.177)
MPCR = HI	0.459*** (0.111)	—
Group size	−0.029* (0.011)	—
MPCRhi × Group size	−0.033*** (0.011)	—
Positive Frame (=1 for this treatment)	—	$0.185^{\#}$ (0.085)
Constant	−0.474*** (0.092)	−0.328*** (0.070)
Fraction censored at 0%	0.362	0.512
Fraction censored at 100%	0.134	0.055
N	1312	640
LLF	−829.8	−363.8

Dependent variable $C_{i,t}$, contribution levels: fraction of endowment contributed to public good; standard errors in parentheses.
# $p < 0.05$.
* $p < 0.01$.
** $p < 0.005$.
*** $p < 0.001$.

contributions) to a change in the independent variable (for example, others' contributions). It is customary to adjust these coefficients to account for limits on the range of actual contributions to generate more accurate predictions. However, we are interested in the impact on desired response; we report and discuss those coefficients. (Adjusted values are available on request.)

In IW, each session includes two 10-period blocks, with a change in treatment for the second block. In Table 2, the variable First 10 Rounds is equal to 1 for the first 10 periods of a session and so captures any learning effect that takes place between the two treatments in a session. This coefficient is marginally significant and positive, indicating a small deterioration in contributions for the second set of rounds. The treatment effects are qualitatively similar to IW's findings. They report data graphically by period, aggregated over all individuals in a treatment, and observe that a lower MPCR leads to more free riding, lower average contributions, and a faster decay in contributions toward zero. In their analysis, group size appears to make a difference in contributions only in the low MPCR sessions. Proportions tests on the fraction of zero contributors are reported for end-period data only and confirm the visual results. Our analysis allows much more precise estimation of the treatment effects and interactions. In Table 2, estimates of the main effects of the treatments show that higher MPCR increases contributions by 57 percentage points and that moving from a group size of 4 to 10 decreases contributions by about 7 percentage points.[7] The estimates in Table 3 are of similar magnitude (46 percentage points for MPCR = HI, and -17 percentage points for Group Size). In both tables, the interaction term shows that the effect of the higher MPCR is significantly smaller for large groups, a result that the authors were unable to tease out using their aggregate approach to analyzing the data. Thus although we confirm their results, our approach allows a more detailed understanding of the treatment effects and one new result, the interaction effect between MPCR and group size.

Tables 2 and 3 also report similar estimates for the Andreoni data. Treatment effects mirror those discussed in the original paper. Andreoni reports data by period for each session, averaged over all individuals, and finds a significant treatment effect, with contributions that are 17 percentage points higher. In his paper, statistical tests (Mann-Whitney rank sum) are conducted by averaging all contributions for each individual, then testing for differences in distributions across treatments. Table 2 shows that a positive frame results in a somewhat larger treatment effect: Contributions are 21.7 percentage points higher than a negative frame. The estimate in Table 3 is 18.5 percentage points.

Recall that positive (negative) coefficients on *Deviation from Group*(+) and *Deviation from Group*($-$) indicate adjustment away from (toward) the group average. In Tables 2 and 3, both data sets show the coefficient on positive deviations, λ_1, is negative and significant: If an individual's contributions are above the group average, a subject will reduce his contribution by about 60 percent of the excess. However, the coefficient on negative deviations, λ_2, is insignificantly different from zero, indicating no corresponding increase for contributions below average. The null hypothesis that $\lambda_1 = \lambda_2$ is easily rejected for the IW data (Table 2: $\chi^2_{(1)} = 17.0$, $p < 0.001$) and for the Andreoni data ($\chi^2_{(1)} = 6.8$, $p = 0.009$, and similarly for Table 3).

These results have implications for the theories of motivation for giving discussed above. Pure altruism required $\lambda_1 = \lambda_2 > 0$; this is clearly not the case. Pure warm glow required $\lambda_1 = \lambda_2 = 0$; this too is rejected. Impure altruism is thus also rejected because the coefficients are unequal. Reciprocity in form of matching required $\lambda_1 = \lambda_2 < 0$. Although λ_1 is negative for

[7] Recall that the variable Group Size is either 4 or 10. The effect of the difference in group size is thus six times the coefficient.

both data sets, λ_2 is positive and insignificantly different from zero. Thus we can conclude that subjects are not attempting to match others' contributions, and this form of reciprocity can be ruled out. Inequality aversion seems a closer description of behavior because the values of the coefficients on these two variables are clearly not equal. Subjects' behavior is consistent only with asymmetric aversion to inequality: Subjects react strongly when their contributions are above average and react very little or not at all when they are below. Disadvantageous inequality motivates subjects' decisions.

4. Random Rematching and Independence

In theory the random rematching of the subjects should render the outcomes independent in each round, because subjects should ignore information about one group in making a decision with another. Experimenters often rematch subjects in order to remove one type of dependence between rounds: dependence caused by subjects' strategic reactions to the behavior of others in their groups. Another type of dependence between rounds, that caused by subjects learning about the game or the behavior of other subjects in general, is possible in any repeated game. If random rematching induced independence, then the coefficient on the deviations from group variable should differ across the two data sets.

Recall that the IW subjects are in stable groups, while the Andreoni subjects are randomly rematched. If random rematching succeeds in its objective of making games independent across rounds, then the coefficient on the *Deviation from Group* variables in the Andreoni data should be zero. We find a negative and economically meaningful coefficient on *Deviation from Group*(+).[8] If random rematching eliminates only the first type of dependence, one would expect the coefficients on the variables to be closer to zero in data from rematched groups. Interestingly, the coefficients we find in both studies are very similar for both *Deviation from Group* variables using fixed effects (Table 2), and with random effects (Table 3) the coefficient is somewhat larger in magnitude for the Andreoni data. We interpret this to mean that, controlling for other factors in the analysis, subjects in IW respond in a very similar way (on average) compared with subjects in Andreoni to a given deviation from the group average.

Of course, this is not meant to indicate that there are, *ceteris paribus*, no differences between behavior in groups of partners and of strangers. Considerable evidence has accumulated across a wide range of experiments that there are different "types" in the population. For example, in our analysis, we find that roughly 40% of fixed-effects coefficients are significantly different from zero in both datasets, a clear indication that subjects have different underlying propensities to donate to the public good. Besides differences in initial giving, subjects also are likely to be predisposed to respond differently to average group behavior.[9] A given "partners" group consists of a draw from this population and may include

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[8] This result is maintained in a model with only experimental design variables and the *Deviation from Group* variables.

[9] For example, Fischbacher, Gächter, and Fehr (2001) elicit preference profiles by asking subjects to specify their contribution level for various levels of giving by other group members; see also Fischbacher and Gächter (2009). They find evidence of altruists, reciprocators, and other types in their subject population. De Oliveira, Croson, and Eckel (2009) elicit types using the Fischbacher and Gächter method, then recruit and match subjects according to type. They find that groups of all-reciprocators give more than groups of all-free-riders, and this difference is larger when types are known.

all cooperators, all free riders, or anything in between. A given ''strangers'' participant will interact with a larger subsample and so is likely to encounter a fairly representative set of counterparts. This suggests that it is likely that one would observe more variation between the average contributions of groups of partners than those of strangers, a result found in Keser and van Winden (2000). This makes it all the more surprising that the coefficients on *Deviation from Group*(+) are so similar across studies.

5. Discussion and Conclusion

In reanalyzing existing data sets, we accomplish several objectives. First, by disaggregating and reanalyzing the data reported in two classic studies, we are able to confirm and extend their reported results. We are able to provide more precise estimates of the treatment effects and to disentangle interaction effects among the treatments and the dynamics in the data. We find (as did IW) that the MPCR is an important determinant of behavior in the VCM. Group size is also a significant determinant of the allocation level when MPCR is low. IW's conclusion that the effect of group size is ''weak and ambiguous'' is clarified somewhat: Our results show that larger groups are less sensitive to higher MPCR. Similarly, our results support Andreoni's conclusion that subjects contribute more when the public good is positively framed, but we also are able to show that the negative frame reduces contributions in part because it affects the speed with which subjects adjust their contributions to match those of the group.

Our second and primary objective is to achieve a better understanding of the motivations for giving. It is clear from the analysis that subjects' decisions are affected by information about others' choices. In addition, the response to others' contributions is asymmetric. When a subject's allocation is above average, he reduces his contribution; when he discovers his contribution is below average, his adjustment toward the others is weak. This pattern of behavior is consistent with inequality aversion (Fehr and Schmidt 1999), which asserts that people care about inequality, but that they care more when their income is below than when it is above others' income. It also implies that analysis that restricts subjects' responses to be symmetric is likely to be misspecified and can produce the misleading impression that subjects are matching, when in fact the responses are asymmetric and depend on whether an individual is above or below average contributions.

Third, we are able to conclude that random rematching does not achieve its intended purpose. Comparing the two data sets, we find remarkably similar responses to prior round behavior, and so we find no evidence that random rematching enhances independence of observations across rounds.

In our search for new wine from an old bottle we show that, overall, subjects' decisions depend critically on what others are doing, and we provide support for the theory of asymmetric inequality aversion. These findings underscore the importance of public data sets in experimental economics. Collecting experimental data has significant time and money costs. Existing data can be explored as a low-cost first step to answering new research questions.

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