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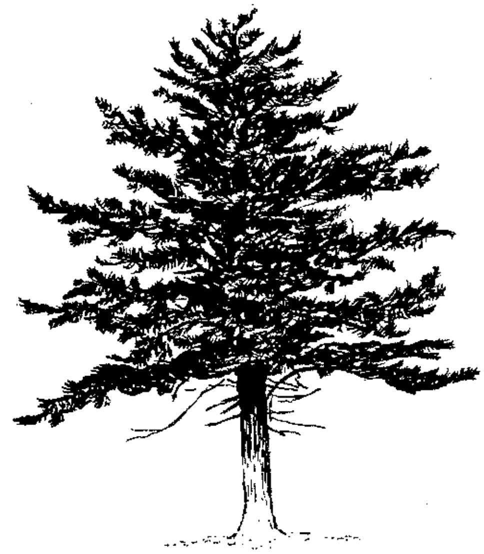

Detecting Other-Regarding Behavior with Virtual Players

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ABSTRACT

Individuals in society and players in laboratory experiments often display levels of cooperative behavior that contradict the predictions of theoretical models of rational self-interested individuals. Leading explanations for these "anomalies" include decision errors and other-regarding behavior. This paper introduces "virtual players" in two public goods experiments as a device to remove the concerns of human subjects for other players. Comparing contributions in all-human and virtual subject treatments, we find support for the hypothesis that other-regarding behavior elevates contributions. The results also suggest that subjects are motivated by fairness considerations and we conclude that fair-share contributions are not made simply because they are cognitively simple to compute. We discuss ways in which the virtual player technique can be applied to help discriminate among competing explanations for the behavior observed in other experiments.

Key words: Altruism; Fairness; Laboratory Experiments; Public Goods; Group Behavior.

JEL Classification: C91, C92, D63, D64, H41

1. Introduction[1]

Game theoretic models of bargaining and voluntary contributions to public goods make strong predictions that rely on the hyper-rationality and self-interest of players. In a Dictator Game, standard theory predicts that a Proposer who is asked to unilaterally divide a sum of money between herself and another player will offer nothing at all. In an Ultimatum game where the Proposer makes an offer that is subject to acceptance or rejection by a Receiver, theory predicts that an amount only marginally greater than the reservation price of the Receiver (typically zero) will be offered by the Proposer and accepted by the Receiver. Similarly, players should not cooperate in repeated prisoner dilemma games, nor contribute to the funding of public goods in typical Voluntary Contribution games.[2]

These predictions, however, are consistently violated in laboratory experiments. Subjects offer significant amounts of money to other players in Dictator and Ultimatum games, achieve non-Nash levels of cooperation in repeated dilemma games and voluntarily contribute to the funding of public goods when these actions are not in their immediate self-interest.[3] Explaining these robust empirical results is challenging. Leading suggestions can be organized into three categories: 1) confusion and decision errors; 2) strategic considerations stemming from individual motivations that extend beyond the absolute magnitude of a player's own payoff (e.g. caring about relative income), or stemming from incomplete information and uncertainty about other player's motivations, payoffs and rationality; and 3) "other-regarding" preferences (altruism, fairness, warm glow, reciprocity, etc.). Discriminating among these competing

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explanations is difficult because non-monetary considerations cannot be directly observed, nor easily manipulated and controlled for in the laboratory (McDaniel et al. 1994).

In this paper, we propose a novel technique to discriminate among other-regarding behavior, self-interested strategic play, and decision errors in laboratory experiments. The approach relies on the introduction of non-human (virtual) players in games normally played by humans only. We posit that opposing a single human subject to virtual players that do not receive payoffs neutralizes the other-regarding components of the human subjects' utility function. By appropriately defining the virtual players' strategy space and behavioral rules, and providing this information to the human subject, it is possible to remove either other-regarding behavior only, or both strategic considerations and other-regarding preferences simultaneously. For instance, if virtual players are programmed to behave exactly as if they were humans, and human players are aware of this, we expect humans to behave no less strategically than humans who play with humans. Thus, one can detect other-regarding behavior by comparing the behavior of humans playing with humans in the control group to the behavior of humans playing with virtual agents in the treatment group. Alternatively, if virtual players use pre-defined decision rules that are not associated with the actions of other players, the experimental environment encourages neither strategic nor other-regarding considerations.

We present the virtual player method and the result of its application in the two methods most widely used to raise funds for public goods: the Voluntary Contribution Mechanism (VCM) and the Provision Point Mechanism (PPM). Our results support the view that players in these games care about the welfare of other players. We are also able to infer that the other-regarding

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[2] The Dictator, Ultimatum, Prisoners Dilemma and Voluntary Contribution games are described and their standard solution presented in most introductory game theory textbooks. See for instance Osborne and Rubinstein (1996) or Bierman and Fernandez (1998).
[3] Excellent surveys of these experimental results appear in Davis and Holt (1993) and Kagel and Roth (1995).

behavior is partially motivated by a desire for fairness in the allocation of costs for the public good.

The remainder of the paper is organized along the following lines. In the next section, we briefly review previous attempts to detect and measure other-regarding behavior and separate it from decision error or strategic play. In section 3, we describe the virtual player approach and the public goods experiments to which we applied it. We present and discuss the experimental results, and then we conclude in section 4 by discussing the wider applicability of the virtual-agent technique.

2. Efforts to Detect, Separate and Measure Other-Regarding Behavior

Economists have used substantially different approaches to separate errors, other-regarding behavior and strategic considerations in public goods and bargaining games. In public goods experiments, they have relied on clever manipulations of payoffs and econometric methods. In bargaining experiments, experimentalists have introduced powerless agents (dummies in the dictator game and three-player ultimatum game; programmed players to direct learning). In this section, we selectively review recent attempts to separate the effect of other-regarding behavior from decision errors and strategic considerations in public goods and bargaining experiments.

Most experimental work on the provision of public goods involves the Voluntary Contributions Mechanism (VCM). In a typical VCM experiment, subjects are given an endowment to be divided between a private account and a public account (Isaac et al. 1984). Allocations to the public account yield a return to each of the N individuals in the group. If the marginal return to the contributor is less than one, but the sum of marginal returns to the group is greater than one, the Nash equilibrium contribution is zero while the social optimum is for everyone to contribute their entire endowment. In one-shot public good games with a dominant strategy to contribute nothing to the collective cause, subjects clearly contribute at levels far above the theoretically predicted value, typically in the range of 40-60% of endowments (for a survey, see Davis and Holt, and Ledyard 1995). Although the observation of higher than predicted contribution rates is indisputable, the causes of this phenomenon are controversial (Ledyard: 148). Individuals may contribute at high levels if they hold beliefs about the game or other players that differ from those postulated by theory; if they gain utility from increases in the utility of others ("pure altruism"); if they gain utility from the act of giving itself ("impure altruism" or "warm glow"); if they care about fairness or equity; or if they make decision errors.[4] We briefly review three attempts to differentiate among hypothesized motives in public goods games.

Andreoni (1995) attempted to separate decision errors from other-regarding behavior. He created a treatment in which the monetary earnings of players in a VCM game were determined by how the players *rank* in terms of experimental payoffs. Subjects with the highest payoffs received the highest monetary rewards. The treatment preserved the dominant strategy to free ride, but created a null-sum game in which there were no monetary gains for the group from (reciprocal) cooperation.[5] Andreoni also stated that "the potential for kindness or altruism also appears to be largely eliminated" (p. 894).

[4] In a one-shot game, there is no incentive to signal cooperative intentions, but such an incentive may exist in repeated-round games.

[5] As Andreoni recognized, paying on payoff differs from the standard VCM in two ways: 1) information about rank is conveyed to the treatment subjects; and 2) monetary earnings are based on rank in the treatment as opposed to actual contributions and public goods provision levels in the control group. To account for this, Andreoni also conducted experiments with a third design in which information about rank was provided in otherwise standard VCM experiments.

Comparing the results of the different treatments, Andreoni concluded that approximately 50% of positive contributions are attributable to errors and the other half to some form of other-regarding behavior. Paying subjects on the basis of rank, however, may introduce additional confusion and hinder the separation of decision errors and other-regarding behavior. Andreoni acknowledged this shortcoming and warned that the results constitute a lower bound estimate of the proportion of contributions motivated by other-regarding behavior.

In a different approach, Palfrey and Prisbrey (1997) had subjects play a VCM game of four sequences of ten rounds, across which subject returns from the private account were randomly selected from a commonly known random distribution. Contrary to standard implementations of the VCM, the range of private marginal returns chosen created instances in which subjects had a dominant strategy to contribute their entire endowments to the public account. The authors then used econometrics to discriminate among decision errors, repeated game effects, warm glow and altruism. Palfrey and Prisbrey concluded that warm glow (utility from the act of giving) and decision errors played major roles in explaining deviations from the dominant strategy to not contribute. On the other hand, they found that altruism (utility received from providing different levels of public goods to others) played little or no role in the subjects' choice of contributions.

More recently, Goeree et al. (1999) adopted a design in which the main control features were group size[6] and the difference between one's own return from a costly contribution to the public account and the benefits that this contribution provides others. Returns were chosen to maintain a dominant strategy to contribute nothing to the public account. The authors found evidence that subjects positively respond to increases in group size or increases in other players'

[6] The effect of group size in VCM experiments has previously been studied by Isaac, Walker and Williams (1994).

returns. They concluded that VCM contributions are not simply an expression of warm glow, but rather reflect the presence of altruism. The authors also estimated probit models of contributing behavior to account for decision errors and other-regarding behavior and found that both factors play a significant role in predicting contribution patterns.

Together, these studies lend support to the conclusion that other-regarding behavior may account for a large portion of contributions in public goods experiments. However, the reliance on manipulating payoffs in public goods experiments has resulted in incomplete separation of motives and the need to apply econometric methods to discriminate among alternative hypotheses. It requires experimentalists to specify the elements that enter a subject's utility function and to assume a functional form.

Non-monetary motivations in bargaining games are similar to those in public goods. To explain the tendency of Proposers in Ultimatum games to make positive offers, a variety of explanations have been invoked, including fairness concerns, subject error and subject uncertainty about the rationality of the Receiver. In efforts to isolate these effects, experimenters have focused on more directly controlling motivations by adjusting the relative power of individual players.

Forsythe et al. (1994) compared the results of single-shot Ultimatum and Dictator games. They found that offers in the Ultimatum treatment, in which the receiver has the power to reject the proposed allocation, were substantially greater than in the Dictator game, in which such power is not granted to the Receiver. They attribute the reduction in the amount offered to Receiver concerns for equity and the strategic behavior of Proposers who anticipate this concern. The persistence of positive offers in the Dictator game, however, was not addressed by the study; positive offers may have resulted from other-regarding behavior or decision error.

Harrington and McCabe (1996) postulated that strategic behavior in the Ultimatum game stems from incomplete information about other players' preferences and propensities to reject positive offers of different sizes. They suggested that learning would allow subjects to form expectations and act as a surrogate for the common knowledge assumptions of game theory. To demonstrate their point, they employed computer programmed Receivers who supply responses to Proposers generated from pre-determined distributions. Players are not aware of the fact that they are facing an automaton, and thus concerns for others should not be affected by the procedure. Harrington and McCabe demonstrated that Proposers choose their offer level in a way consistent with learning.

In a different approach to the problem, Güth and van Damme (1998) added a third player to the Ultimatum game. With this alteration, the Proposer proposes an allocation between all three players, subject to the approval of the Receiver. The third player is a dummy in the sense that it has no say in the determination of payoffs. The subgame perfect equilibrium of this game is to offer nothing to the Dummy.[7] Sending a positive amount to the Dummy can only be rationalized if the Proposer has unobserved preferences for fairness, if she believes that the Receiver does and could thus refuse offers that do not allocate a sufficient amount of money to the Dummy (a strategic consideration), or confusion. The results of this experiment reinforce the view that strategy plays a greater role in motivating behavior than does fairness. Offers never contained much for the Dummy (although many contain some), indicating little desire for fairness. Furthermore, proposed allocations for the Dummy decline over repetitions of the game, potentially as Proposers realize that Receivers do not reject offers on the basis of the Dummy's share of the pie.

Our introduction of virtual players into public goods games is a natural extension of previous approaches and can be used to remove other-regarding concerns in addition to, or instead of, strategic considerations. Harrington and McCabe's subterfuge essentially made use of virtual players to control the stimuli and observe the learning of subjects. Their subjects, however, did not know that other players were automata. A more explicit introduction of virtual players provides a direct method to observe and control other-regarding behavior in public goods experiments and is a logical step toward a better understanding of behavior in public goods games.

3. Design and Applications

We employ an experimental design that focuses on the identification and removal of all forms of other-regarding behavior in two public goods funding mechanisms: 1) a variant of the VCM described above; and 2) a variant of the Provision Point Mechanism (PPM) environment studied by Rondeau et al. (1999).

By removing all other humans from the game, we neutralize the predisposition of some subjects to care about the welfare of others. We hypothesize that humans playing a one-shot public good game with virtual players will contribute less than humans playing an identical game with human players. We attribute any changes in contribution levels to the presence of other-regarding preferences.

To cleanly identify other-regarding behavior, we must ensure that the control experiment (humans playing with humans) and the treatment (humans playing with virtual players) produce identical strategic incentives. We maintain the strategy set across treatments by making the

[7] In their implementation, Güth and van Damme actually constrained the Proposer to offer a small minimum amount to the Dummy. Thus, the subgame perfect Nash involves sending the minimum amount to the Dummy, the smallest

virtual players' actions reflect past human play, and by informing subjects of this fact. We first run control experiments with human subjects and gather the individual data on contributions. The data from the all-human sessions (N players per session) generate a databank of individual choices from which the actions of virtual players in subsequent automata treatments are drawn. In virtual player treatments, a single human subject plays with N-1 virtual players, which are explicitly characterized in the instructions as non-human agents. The human subject also receives information on the way in which the virtual players' actions are determined.

We conducted three sets of experiments. The "Fall 1998" (PPM control with humans) and "Spring 1999" (PPM virtual-player treatment) experiments were a pre-test of the method, but we find the data sufficiently interesting to report them below. In the Fall of 1999, we applied the same method to both the VCM and PPM institutions, using students recruited from a single class.

3.1 Fall 1998/Spring 1999 PPM Experiments

The Fall 1998 experiment drew its subject pool from an introductory engineering economics class (similar to a managerial economics course) composed of engineering students in their senior undergraduate year or in their first year of a terminal masters program. Students were not taught about public goods. A group of thirty-nine students in the class participated in an all-human Provision Point Mechanism (PPM) experiment during a weekly section meeting. Beyond the usual instructions regarding anonymity, subjects were told that the experiment was conducted by researchers from another department and was completely unrelated to the course.

The Fall 1998 PPM institution can be summarized as follows.[8] Each subject receives an initial endowment E=$12 and divides this amount between a private account and a public

possible denomination to the Receiver and keeping the balance.

[8] Instructions are available from the authors.

investment fund (a contribution C_i by player i). Any amount deposited in the subject's private account becomes part of the subject's payoff. If the sum of all subject contributions is below a certain threshold (the provision point, PP), all contributions to the group investment funds are reimbursed (a money-back guarantee). In this case, i's payoff is simply her initial endowment of \$12. If total contributions to the investment fund exactly match the PP, all subjects in the group receive V=\$6 for a net payoff of $E - C_i + V = 18 - C_i$. Finally, if the sum of contributions exceeds the PP, excess contributions return to subject i proportionally to the weight of i's contribution to the investment fund. In this final case, i's net payoff is $E - C_i + V + \frac{C_i}{\sum_{j=1}^{n} C_j}(\sum_{j=1}^{n} C_j - PP) = 18 - \frac{C_i}{\sum_{j=1}^{n} C_j} PP$. Prior to her decision, a subject knows the group size and that all players have the same endowment (E) and value (V) for the investment.

In our design, the public good has an uncertain cost, with the PP chosen randomly from a known uniform distribution. Subjects know that, after all decisions have been made, the PP is revealed by drawing from a bingo cage containing 25 balls numbered 0 to 24 and multiplying the number drawn by \$10. The uncertain cost facilitates the derivation of the selfish Nash equilibrium and reduces the possibility of a focal-point equilibrium at the fair cost-sharing amount PP/N. The symmetric Nash equilibrium is $(\frac{2}{N+1})V$, which is positive but approaches zero as the group size increases (result derived by William D. Schulze; see appendix for derivation).

In the 1998 PPM experiment, the mean contribution to the public investment fund was \$6.09 (median = \$6.00; s.d. = 3.48). The high level of contributions in this one-shot environment is consistent with previous results reported in Rondeau et al. and inconsistent with the symmetric Nash prediction of \$0.30.

In 1999, we replicated the Fall 1998 experiment in the same engineering course and under the same conditions except for the introduction of virtual players. Each 1999 subject played in a group consisting of the subject and thirty-eight virtual players. As explained earlier, the contributions of the thirty-eight virtual students were drawn randomly from the set of contributions generated in the 1998 all-human experiment. Subjects were told that the contributions of the virtual students were drawn randomly from a distribution of contributions generated by Cornell students who participated in the same experiment in all-human groups. With the exception of additional language concerning the virtual players, the instructions and experimental environment were identical across the 1998 and 1999 sessions.

The 1999 data exhibit the expected decrease in contributions. Subjects in the virtual player environment contributed an average of $4.60 (median = $5.00; s.d. = 3.31). Statistical differences between the human and virtual player treatments are significant. For instance, a t-test on means yields a p-value less than .03 and a Mann-Whitney test of equality of the medians yields $p < .06$. As another indicator of other-regarding behavior, the percentage of $0 contributions increased from 3% in the human treatment to 21% in the 1999 virtual player environment, a statistically significant change (a test of equal proportions yields $p < .01$).

While these results are suggestive of the role of other-regarding behavior in this PPM environment, we must question their robustness since the subject pool was not strictly identical between the two years. Our next study draws subjects from the same pool and applies the virtual player technique to the VCM as well as the PPM environment.

3.2 Fall 1999 VCM/PPM Experiments

Subjects for the Fall 1999 experiments were drawn from an introductory undergraduate economics class. These subjects had already participated in four other experiments during the semester including a repeated-round duopoly experiment in which deviations from the payoff-maximizing collusive strategy were common and discussed in follow-up teaching activities. Furthermore, the names of top earners to date were posted in the experimental laboratory. The top moneymakers were to receive prizes at the end of the semester (in addition to performance-based monetary payoffs), although our experiment was not included in the class competition. Thus, our subject pool can be considered an "extreme" environment in which to search for altruistic preferences: subjects were "economists in training," operating in an environment in which self-interest was being reinforced.

Experimental sessions took place over two days in the class's weekly section meetings. Subject participation was quasi-voluntary – a tiny portion of the final grade was attributed to attendance at weekly sections. About half of the subjects participated in a VCM treatment, the other half in a PPM. Roughly half of the subjects made their decisions in all-human groups. The other half made their decisions in groups of "virtual students." Thus the experimental design had four cells: (1) PPM with all-human groups; (2) PPM with virtual-student groups; (3) VCM with all-human groups; and (4) VCM with virtual-student groups. There were two sessions run for each cell, with roughly twenty students in each session. A total of 163 subjects took part in the experiment.

The PPM experiment was conducted with a design that parallels the 1998 pre-test with the exception of three minor changes. The group size was reduced to 20, the individual payoff was increased to $7 and the random provision point was drawn from the uniform distribution with support [0, 140] (see Table I). In the VCM design, the group size was 21 and, as in the PPM design, subjects had common endowments, payoffs, and information (see Table I).

It should be noted that our VCM design differs slightly from the VCM design most commonly found in the literature. In our design, subjects receive an individual return of $0.07 per dollar contributed to the group fund to a maximum of $7. Thus, contrary to previous research on the VCM, the social optimum is for the group to contribute $100 rather than the entire collective endowment. Yet, subjects still have a dominant strategy to contribute nothing. We chose this design to match the design of other public good experiments conducted at Cornell as part of a larger NSF-funded research program. The design matches real world situations in which the maximum quantity of the public good that can be provided is restricted (e.g., roads removed from a national park, Champ et al. 1997).

As predicted, subjects in these experiments behaved differently depending on whether they played with humans or with virtual players (see Table I). The differences were most striking in the VCM environment in which the dominant strategy was to contribute nothing to the public account. In this environment, subjects in the human treatment contributed an average of $2.14 (median of $2.38) while their counterparts in the virtual player treatment contributed $1.16. It is worth noting that the mode and median contribution level in the virtual treatment are exactly zero, consistent with the Nash equilibrium prediction. The percentage of zero contributions increased from 40% to 60% ($p = .03$).[9] We attribute the positive contributions of the remaining subjects to decision errors.

Table I lists four statistical tests of significance, ranging from a nonparametric test (Mood/Westernberg) that makes few assumptions about the underlying distribution of

[9] The number of zero bids in the PPM environment also increased, but not significantly. Since a zero contribution is not necessarily a Nash strategy, this result is not surprising.

contributions to the parametric *t*-test that makes strong assumptions about the underlying distribution. We include the nonparametric tests because of the highly irregular, skewed sample distributions generated by the experiments. Given such poorly-behaved distributions, we believe that the Mood (Westenberg) test, a nonparametric test with few assumptions, is the most powerful of the tests. The assumptions underlying these tests can be found in the appendices. For the VCM data, the tests yield p-values ranging from $p = 0.01$ to $p = 0.03$.

In the PPM experiments, subjects playing with humans contributed an average of $5.30 compared to the $4.27 contributed by subjects playing with virtual agents. In the same order, medians are $5.00 and $4.00. These results are significantly higher than the symmetric Nash equilibrium of $0.67, but the differences between treatments are only weakly significant. The battery of statistical test reported in Table I yields p-values ranging from 0.035 to 0.11, although the more powerful Mood Test indicates significance at the 5% confidence level. The weaker statistical results for the PPM can be explained by the greater variance in contribution levels observed in this mechanism compared to the VCM.[10] This greater variance is likely attributable to the lack of a dominant strategy in the PPM and the existence of other (non-symmetric) Nash equilibria. Thus we observe a flatter distribution and a greater variance, and we find it more difficult to statistically detect other-regarding behavior. It is notable, however, that in both the PPM and VCM experiments, the difference in mean contributions attributed to other-regarding behavior is approximately $1.00 ($0.98 for the VCM and $1.03 for the PPM).

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[10] If the assumption of normality was valid, a *t*-test of the PPM data would require a sample size of over 200 to detect a one dollar difference (power = 0.80), given the underlying variability of the PPM data

TABLE I - Results

Comparison of Contribution Levels: All-Human Groups vs. Virtual-Human Groups

	VCM		PPM	
	Human	Virtual	Human	Virtual
Mean Contribution	$2.14	$1.16	$5.30	$4.27
(Standard Deviation)	(2.06)	(1.89)	(3.89)	(3.38)
Median	$2.38	$0.00	$5.00	$4.00
Induced Value[1]	$7	$7	$7	$7
Endowment	$12	$12	$12	$12
Sample Size[2]	42	43	40	38
Mood (Westenberg) Test	p = .033		p = .035	
Fligner-Policello Test	p = .010		p = .098	
Mann-Whitney Test	p = .011		p = .099	
t-test	p = .012		p = .110	

1 In the VCM session, subjects received $0.07 per share purchased at $1 each by the group, up to a maximum of 100 shares purchased or $7 in individual payoff. Assumptions underlying the statistical tests are presented in the appendix.

2 VCM human experiments were conducted in groups of 21 individuals. All 43 participants in the virtual player treatment were informed that they were playing with 20 virtual agents. In the PPM, the human group size was 20 and all 38 players in the virtual treatment were told that they were playing with 19 virtual players.

To add further support to our contention that other-regarding behavior causes the observed differences in contribution levels, and to demonstrate the opportunities for inquiry made possible by the virtual-agent treatment, we analyzed the data for signs of concerns for fairness.

Subjects in the VCM sessions could calculate the fair cost-sharing contribution by dividing the amount of contributions required to fund the public good entirely ($100) by the number of players in the group. The fair cost-share contribution was $4.76 in all VCM sessions. If fairness plays a strong role in determining contribution levels for some subjects, we expect to observe a greater number of fair share contributions in all-human groups than in virtual-player groups.

Indeed, we observe this phenomenon. In human treatments, 14% of subjects contributed *exactly* the expected fair share of $4.76, while none of the subjects who played with automata contributed the fair share.[11] This difference is highly significant ($p = .004$) and fair share contributors accounted for 32% of the total contributions. If we relax the definition of a fair share contribution to any contribution between $4.50 and $5.00, we find that 29% of the all-human subjects contributed the fair share while only 2% in the virtual-player groups contributed in this range ($p < .000$). Using the more flexible definition, the fair share contributors in the all-human sessions accounted for 63% of the total contributions.

The greater frequency of fair share contributions in the all-human groups also suggests that these calculations are *not* made simply because they are cognitively easier to make. Thus, virtual player designs may also allow researchers to differentiate strategies motivated by human interactions from strategies that are adopted because they involve lower cognitive costs.

Finally, to ensure that the detected changes in contribution behavior between the all-human sessions and the virtual-agent sessions were not generated by differences in subject groups, we collected subject information through a post-experiment questionnaire (see appendix). The relevant information is presented in Table II.

The data indicate that subject characteristics across experimental sessions are similar. Although the proportions of females and economics majors appear to differ in the VCM across cells, the differences are not significant ($p \geq .14$). In the PPM cells, however, the difference in the mean number of economics classes taken by subjects does differ significantly ($p < .05$); subjects in the all-human session had taken more economics classes. A simple regression of contributions on the number of economic classes taken suggests that if the number of economics classes taken has an impact on contribution behavior, it likely reduces contributions. The coefficient is negative and weakly significant ($p < .10$). A negative impact on contributions would make detecting increases in contributions due to altruistic preferences more difficult. There is also a difference in subject assessments of the experimental instructions ($p = .01$). Although both groups indicate the instructions were generally easy to follow, subjects in the PPM virtual-agent session, not surprisingly, found the instructions somewhat more difficult than their counterparts in the all-human session. It is not clear what impact such difficulty may have on contributions, but regressing contributions on difficulty scores generates an insignificant ($p = .70$) and negative coefficient (i.e.. the easier a subject thought the instructions were, the less the subject contributed). In conclusion, we believe that the differences in contribution patterns across cells cannot be attributed to differences in subject pools.

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[11] We considered contributions at \$4.76 or \$4.77 as exact fair share contribution. In the first VCM session, there was one contribution at \$4.80 and another at \$4.86. We did not classify these contributions as exact fair share contributions, but clearly they are very close.

Table II - Subject Pool Characteristics

Characterization of Subject Pools By Session								
Session	Avg Age	%Female	% Go to Church	% Econ Major	Avg. # Econ Classes	Avg Anonym Score[1]	Avg Difficult Score[2]	Avg Altruism Score[3]
VCM All-Human	18.9	41%	26%	71%	1.1	6.0	6.2	3.2
VCM Virtual	18.8	26%	26%	84%	1.1	6.1	6.0	3.1
PPM All-Human	18.5	38%	23%	78%	1.4	6.3	6.4	3.2
PPM Virtual	18.9	37%	32%	76%	.84	6.2	5.9	3.1

1 "The instructions for the experiment were clear and easy to follow." Scale 1 (strongly disagree) - 7 (strongly agree).
2 "The procedures followed in this experiment preserved my anonymity." Scale 1 (strongly disagree) - 7 (strongly agree).
3 The index was generated from a series of ten questions (see questionnaire for more details).

4. Concluding Remarks

The indisputable evidence that individuals derive non-trivial amounts of satisfaction from contributing to the welfare of others or achieving "fair" or "cooperative" outcomes poses fundamental challenges to economic theory. Despite recent creative experimental designs by Andreoni (1995), Palfrey and Prisbrey (1997) and Goeree *et al.* (1999), our understanding of behavior in simple public goods environments remains incomplete.

By introducing virtual players, we generated data that are consistent with the presence of other-regarding behavior of identical magnitude in two public goods games: the Voluntary Contribution Mechanism and the Provision Point Mechanism. Our results do not support the hypothesis of Ledyard (1995: 171) that high average rates of contributions in VCM environments are "the unintended result of a corner noncooperative solution and not altruism." Although further work is needed to clarify the precise components of other-regarding preferences that are neutralized through the virtual-agent treatment, we reported evidence demonstrating that a non-trivial number of subjects are concerned with fairness and that opposing humans to virtual players could eliminate such concerns.

In addition to corroborating past work on the presence of non-monetary components of the utility function in a simple and direct fashion, the virtual-agent technique opens up myriad possibilities for exploring individual decision-making in group settings. For example, it is often believed that in the context of repeated public goods games, the decline in contributions over multiple periods represents a learning effect (e.g., Palfrey and Prisbey). However, the decline may also result from a decline in warm glow or altruism over time. Using the virtual-agent technique, a researcher can isolate the effects of learning from diminishing altruistic behavior. To further discriminate between "warm glow" motivations and "pure altruism" motivations

(Palfrey and Prisbey; Goeree et al.), one could run an experiment that varies the human-virtual agent mix across cells. A "warm glow-only" hypothesis predicts no change in contribution levels as the number of human agents is varied above two.

The virtual-agent technique can also be fruitfully applied to games outside of the public goods context. In the Centipede game, for example, introducing virtual players endowed with different strategies could help discriminate between errors, beliefs, signaling, and altruistic preferences. As Harrison and McCabe have demonstrated, virtual players can be "programmed" with strategy spaces or specific rules of behavior designed to verify whether or not human subjects respond strategically to their opponents' motives. Thus, the virtual-agent method is not limited exclusively to the removal of other-regarding behavior. It holds the potential to help us understand learning and optimization skills and strategy formulations in a broad range of games and economic situations.

Control is the essence of experimental methodology. We have demonstrated that the introduction of virtual players can be used to detect, and remove, other-regarding behavior in laboratory experiments. It can also be used to manipulate and control the strategy sets of laboratory subjects. Individuals are motivated by more complex factors than the *Homo economicus* we typically represent with models. We are hopeful that the virtual player technique can help elucidate these motivations and provide an empirical basis to refine our theories of economic behavior.

Appendix

Derivation of Symmetric Nash Equilibrium in PPM Environment with Uncertain Cost

The derivation of the symmetric NE in the PPM environment with uncertain cost is attributed to Professor William D. Schulze, Cornell University. There are N individuals in a group that has an opportunity to fund a public good. The cost of the public good, or Provision Point, is C. C is chosen randomly from the uniform distribution $[0, C_{max}]$ after each group member has made a contribution $b_i \geq 0$. The i^{th} member of the group receives a payoff of V_i if $(\sum_{j=1}^{N} b_j \geq C)$. If the sum of all contributions is greater than or equal to the Provision Point $(\sum_{j=1}^{N} b_j \geq C)$. If the sum of all contributions is greater than C, each agent receives a share of the excess contributions through a rebate that is proportional to their contribution: $\frac{b_i}{\sum_{j=1}^{N} b_j} * (\sum_{j=1}^{N} b_j - C)$. Thus the total cost to agent i is:

$$b_i - \frac{b_i}{\sum_{j=1}^{N} b_j} * (\sum_{j=1}^{N} b_j - C) = \frac{b_i}{\sum_{j=1}^{N} b_j} C. \tag{1}$$

Taking as given the contributions of all N-1 group members ($B_{\neg i} = \sum_{j=1}^{N} b_j - b_i$), agent i's problem is to choose a contribution, $b_i \geq 0$, to maximize her expected utility:

$$\max_{b_i} \int_0^{b_i + B_{\neg i}} \frac{1}{C_{\max}} (V_i - \frac{b_i}{\sum_{j=1}^{N} b_j} C)\, dC. \tag{2}$$

First consider an interior solution. Taking the derivative of expression (2) and setting it equal to zero, we obtain

$$\frac{1}{C_{\max}} (V_i - b_i - \frac{1}{2} B_{\neg i}) = 0. \tag{3}$$

Solving for b_i yields

$$b_i = V_i - \frac{1}{2} B_{\neg i} \leq V_i \quad for\ B_{\neg i} \geq 0. \tag{4}$$

If $V = V_j\ \forall j$, then $B_{\neg i} = (N-1)\, b$, *where* $b = b_j\ \forall j$. Thus the symmetric Nash equilibrium contribution is

$$b = V - \frac{1}{2}(N-1)b \;\Rightarrow\; b = \left(\frac{2}{N+1}\right)V . \tag{5}$$

One can also show that $b = b_j = 0\ \forall j$ is *not* a symmetric Nash equilibrium. If it were, an agent could do no better than a payoff of zero by deviating. If an agent were to deviate, however, and offer a positive contribution, $b_i > 0$, she could achieve a positive payoff

$$\left(\frac{b_i V}{C_{max}} - \frac{b_i^2}{2\, C_{max}} \right).$$

Assumptions for Statistical Tests

In general, nonparametric procedures are only slightly less efficient than normal theory tests when the underlying populations are normal, but they can be "mildly or wildly more efficient" when the underlying populations are not normal (Hollander and Wolfe 1999: 1).

Mood (Westenberg) Test: The test assumes data are from two independent random samples, the measurement scale is at least ordinal, the variable of interest is continuous, and if the two populations have the same median, then for each population the probability p is the same that an observed value will exceed the grand median of the two samples combined (Daniel 1978).

Original References: Mood, Alexander M. 1950. Introduction to the Theory of Statistics. New York: McGraw; Westenberg, J. 1948. "Significance Test for Median and Interquartile Range in Samples from Continuous Populations of Any Form." Akad. Wetensch. Afdeeling Voor de Wis, 51: 252-261.

Fligner-Policello Test: The test assumes we have two independent random samples from continuous distributions that are symmetric about the population medians. It does *not* assume that the distributions have the same form or the same variance (Hollander and Wolfe: 135-139). The Fligner-Policello test has attractive properties when underlying assumptions are met: consistency, asymptotic normality of the test statistic, and an asymptotic relative efficiency equal to the Wilcoxon rank sum statistic when compared with a variety of possible population distributions.

Original Reference: Fligner, M.A., and G.E. Policello II. 1981. "Robust Rank Procedures for the Behrens-Fisher Problem." Journal of the American Statistical Association. 79: 208-211.

Mann-Whitney (Wilcoxon) Rank Sum Test: The test assumes that each observation comes from independent random samples from continuous populations that are symmetric about the population median. i.e., the average median (mean) is symmetric about the true median (mean). It also assumes that the distributions have the same general shape and dispersion.

***t*-test:** The test assumes that each observation comes independent random samples from normal populations.

Experiment Instructions

We include the instructions (without formatting) for the virtual player treatments only. Instructions for the all-human sessions were the same but with virtual-player language omitted. All-human instructions and decision sheets for all sessions can be obtained from the authors. All sessions included a five-minute oral summary and a question-and-answer period after all subjects had read the instructions. In the oral summary, the non-human nature of the virtual students and the way in which the virtual players made their decisions were emphasized.

<u>VCM Instructions</u>

This is an experiment in the economics of decision making. If you follow these instructions closely and make a careful decision, you can earn money. Please do not communicate with any other student during the experiment.

In today's experiment, you are a member of a group that consists of you and ________ "virtual" students (so if there are fifteen students in the lab today, there are fifteen groups – each human student is paired with a group of virtual students). The decisions of these virtual students are made by a computer. To start the experiment, we give you and each virtual student an "initial balance" of $12.00. Once you have read and understood these instructions, you will be asked to enter a "bid" indicating how much of your $12 you want to invest into a "group investment fund." You can bid any value between $0 to $12.

The money that you bid to the group investment fund will be combined with the bids received from the virtual student members of your group. The fund can purchase "shares" at a price of $1/share. The fund can purchase up to 100 shares, but no more than that. Hence, all available shares will be purchased if the sum of bids made to the investment fund equals or exceeds $100.

For every share purchased by the group investment fund, <u>you</u> will each receive $0.07/share (7 cents/share), up to a maximum of $7 (100 shares times $0.07). Note that the virtual students will not receive a payoff because they are not real people.

The virtual students have already submitted their bids, which are in an envelope at the front of the room. Their bids come from a distribution of actual bids made by Cornell students in this exact experiment. In the previous experiments, however, all group members were human; no virtual students were used. Remember, your group is you and the virtual students. None of the other human students in your class are in your group; they are working in different groups.

Your final earnings for the experiment will depend on your bid and the bids of your virtual group members.

There are two possible outcomes:

FIRST POSSIBLE OUTCOME: the sum of bids is LESS than $100. In this case, all bids will go toward the purchase of shares at $1/share. You will receive a personal payoff of $0.07/share from the group investment fund. Thus your earnings for the experiment would be your initial balance of $12, minus your bid to the group investment fund, plus your payoff of $0.07/share for every share purchased by the group fund (so if 80 shares are purchased, for example, each member of the group receives $0.07 x 80 = $5.60). Note that the virtual students are also bidding as if they were to receive a payoff per share of $0.07. In other words, their bids come from a distribution of bids submitted by human students who, like you, were to receive $0.07/share for every share purchased by the investment fund.

SECOND POSSIBLE OUTCOME: the sum of bids is EQUAL to or GREATER THAN $100. If the sum of bids is equal to or greater than $100, the investment fund will purchase all 100 available shares. Thus you would receive the maximum payoff of $7. Your earnings for the experiment would be your initial balance of $12, minus your bid to the group investment fund, plus your payoff of $7. Note that no matter how much money is contributed to the group fund, no more than 100 shares can be purchased. Note also that the virtual students, like you, are bidding as if they were to receive a payoff per share of $0.07. In other words, their bids come from a distribution of bids submitted by human students who, like you, were to receive $7 if the sum of bids was equal to or greater than $100.

SUMMARY:

- You are a member of a group that consists of you and ________ virtual students. These virtual students are played by a computer that chooses bids randomly from a set of bids submitted by real human students. These human students participated in an all-human version of this exact experiment in the past.
- You, and each of the virtual students, have an initial balance of $12.
- You must decide how much of your $12 to bid into a group investment fund.
- The group investment fund will buy "shares" that cost $1/share and pay $0.07/share to every member in the group.
- <u>If the sum of bids to the investment fund for your group is smaller than $100</u>, the group investment fund will use the money to purchase as many shares as possible at $1/share. In this case, your earnings will be your initial balance of $12, minus your bid, plus $0.07 times the number of shares purchased.
- <u>If the sum of bids for your group is equal to or greater than $100</u>, the group investment fund will use the money to purchase all 100 available shares at $1/share. In this case, your earnings will be your initial balance of $12, minus your bid, plus $7 ($0.07 times 100 shares)

To submit your bid you must fill out the bottom portion of the attached decision sheet, including your name and social security number (these are necessary for you to be paid for the experiment). Once everyone has completed the form, the instructions and the form will be collected. This will end the experiment. Your bids will be entered into a computer to determine the outcome for your group and calculate your personal earnings. You can collect your earnings next week in section. Remember to bring a form of ID to section in order to collect your earnings.

<u>All information regarding your personal bid, rebate and earnings are strictly confidential</u>
<u>and will not be revealed to anyone.</u>
<u>This experiment is conducted for research purposes only</u>
<u>and is in no way related to your class standing.</u>
It is very important that you understand these instructions.
Raise your hand if you have any questions.

<u>PPM Instructions</u>

This is an experiment in the economics of decision making. If you follow these instructions closely and make a careful decision, you can earn money. Please do not communicate with any other student during the experiment.

In today's experiment, you are a member of a group that consists of you and _______ "virtual" students (so if there are fifteen students in the lab today, there are fifteen groups – each human student is paired with a group of virtual students). The decisions of these virtual students are made by a computer. To start the experiment, we give you and each virtual student an "initial balance" of $12.00. Once you have read and understood these instructions, you will be asked to enter a "bid" indicating how much of your $12 you want to invest into a "group investment fund." You can bid any value between $0 to $12.

The money that you bid to the group investment fund will be combined with the bids received from the virtual student members of your group. The group fund has an opportunity to make a single investment. The cost of this investment will be randomly determined after everyone in the section has completed the experiment. A volunteer will draw a ball from a bingo cage containing balls numbered $0, $1, $2,..., up to $14. The randomly drawn number between $0 and $14 will be multiplied by ten (10) and the result, between $0 and $140, will be the investment cost for your group.

If the sum of all bids made by you and members of your group is less than the investment cost, no investment will be made. On the other hand, if the sum of bids equals or exceeds the investment cost, the investment will be made and <u>you</u> will receive a payoff of $7.00 from the investment (the virtual students will not receive a payoff because they are not real people).

The virtual students have already submitted their bids, which are in an envelope at the front of the room. Their bids come from a distribution of actual bids made by Cornell students in this exact experiment. In the previous experiments, however, all group members were human; no virtual students were used. Remember, your group is you and the virtual students. None of the other human students in your class are in your group; they are working in different groups.

Your earnings for the experiment depend on your bid, the bids of your virtual group members, and the randomly drawn investment cost. There are three possible outcomes:

FIRST POSSIBLE OUTCOME: the sum of bids is LESS than the investment cost. In this case, your bid will be refunded. This is a Money-Back-Guarantee: if the investment cost is not reached by the group, the full amount of your bid will be refunded to you. Therefore, if the sum of bids for the group is less than the investment cost, your earnings for the experiment will be equal to your initial balance of $12, regardless of the amount of your bid.

SECOND POSSIBLE OUTCOME: the sum of bids is EQUAL to the investment cost. If the sum of bids equals the investment cost, the investment will be made and you will receive a payoff of $7 from the investment fund. Therefore, your final earnings for the experiment would be your initial balance of $12, minus your bid, plus your personal payoff of $7. The virtual students also "receive" $7; in other words, their bids come from a distribution of bids submitted by human students who, like you, were to receive $7 if the sum of group bids reached the investment cost.

THIRD POSSIBLE OUTCOME: the sum of bids is GREATER THAN the investment cost. If the sum of bids exceeds the investment cost, the investment will be made and you will receive a personal payoff of $7. Given that the sum of bids exceeds the investment cost, the fund will return the difference between the sum of bids and the investment cost to the group (of course, only you will receive money; the virtual students will not). This "rebate" of excess contributions, which is described below, reduces your investment fund payment to an amount less than your bid. Thus, if the investment cost is exceeded, your earnings for the round would be your initial balance of $12, plus your payoff of $7, minus your bid, plus your personal rebate of excess contributions.

CALCULATION OF PERSONAL REBATES (SUM OF MEMBER BIDS IS GREATER THAN INVESTMENT COST): Your rebate is directly proportional to the amount of your bid relative to the total amount of your group's bids. Thus, if your own bid were equal to 20% of the sum of bids for the group, your rebate would be 20% of the bids in excess of the investment cost. To illustrate how the rebates are calculated, let's consider an example. The example is provided only to illustrate how the personal rebates are calculated. All numbers presented in the example are fictitious and unrelated to the actual experiment you are in today.

Rebate example: Consider a group of one human and fourteen student bids a total of $1,000. Chris, the human member of this group, made a bid of $100. Her bid thus represents 10% of the sum of bids to the group fund ($100 is 10% of $1,000). Now, suppose that the investment cost turns out to be $700. The amount of excess bids is therefore $300 ($1,000 - $700 = $300). By multiplying the share of Chris's contribution (10%) by the amount of excess contributions ($300), we see that Chris's rebate is equal to $30.

Regardless of the numbers chosen to illustrate the rebate rule, the rule guarantees that when the investment cost is exceeded, the group does not pay more than the investment cost. All excess bids are rebated to the group. Furthermore, individuals cannot pay more than their initial bid to the investment fund.

SUMMARY:

- You are a member of a group that consists of you and ________ virtual students. These virtual students are played by a computer that chooses bids randomly from a set of bids submitted by real human students. These human students participated in an all-human version of this exact experiment in the past.
- You, and each of the virtual students, have an initial balance of $12.
- You must decide how much of your $12 to bid into a group investment fund.

- Once everyone has completed the experiment, an investment cost between $0 and $140 will be randomly chosen by drawing a bingo ball from a set of balls numbered $0, $1, $2,…, up to $14 and multiplying the number on the drawn ball by 10.
- If the sum of bids to the investment fund for your group is smaller than the investment cost, the full money-back-guarantee ensures that your bid will be returned to you. In this case, your earnings for the experiment will be your initial balance of $12.
- If the sum of bids for your group is equal to the investment cost, you will receive a payoff from the investment fund of $7 and your earnings for the experiment will be your initial balance of $12, minus your bid, plus your payoff of $7.
- If the sum of bids for your group is greater than the investment cost, you will receive a payoff from the investment fund of $7. All bids in excess of the investment cost will be rebated back to group members so that the group does not pay more than the investment cost. The exact amount of your personal rebate would be calculated according to the proportional rebate rule described above. Your earnings for the experiment will be your initial balance of $12, minus your bid, plus the payoff of $7, plus your personal rebate.

To submit your bid you must fill out the bottom portion of the attached decision sheet, including your name and social security number (these are necessary for you to be paid for the experiment). Once everyone has completed the form, the instructions and the form will be collected. This will end the experiment. Your bids will be entered into a computer to determine the outcome for your group and calculate your personal earnings. You can collect your earnings next week in section. Remember to bring a form of ID to section in order to collect your earnings.

All information regarding your personal bid, rebate and earnings are strictly confidential and will not be revealed to anyone.

This experiment is conducted for research purposes only and is in no way related to your class standing.

It is very important that you understand these instructions.

Raise your hand if you have any questions.

Post-experiment Questionnaire (unformatted)

1. **Age** ___________
2. **What is your sex? (Circle one number)** 01 Male 02 Female
3. **Do you regularly attend religious services?** 01 Yes 02 No
4. **Class (Circle one number)**
   01 First Year 02 Sophomore 03 Junior 04 Senior
5. **Major (Circle one number)**
   01 Ag & Applied Econ 02 Agbusiness Mgmt 03 Business Mgmt & Mrkting
   04 Env & Res Econ 05 Farm Bus Mgmt & Finance 06 Food Industry Mgmt
   07 Economics 08 Other (Please Write)_____________
6. **How many economics classes have you taken at the university level? (Circle One)**
   None One Two Three Four Five More than Five

For questions 7 and 8, circle the number on the rating scale that best represents your opinion about that item.

7. **The procedures followed in this experiment preserved my anonymity (CIRCLE ONE NUMBER)**

1	2	3	4	5	6	7
Strongly Disagree						Strongly Agree

8. **The instructions for the experiment were clear and easy to follow (CIRCLE ONE NUMBER)**

1	2	3	4	5	6	7
Strongly Disagree						Strongly Agree

9. **What is your best estimate of the total money contributed by your group to the investment fund? (FILL IN THE BLANK)**

10. **Prior to today, I had heard about this experiment directly or indirectly from other students who have participated in the past.**

YES **NO**

Please fill out the table below. For each statement, place an "X" in the box that most accurately reflects the frequency with which you have engaged in the activity.

	Never	Once	More Than Once	Often (3 or 4 times)	Very Often (5 or more times)
I have done volunteer work for a charity.					
I have donated blood.					
I have helped a classmate who I did not know well with a homework assignment when my knowledge was greater than his or hers.					
I have offered my seat on a bus or a train to a stranger who was standing.					
I have written a letter to my congressperson or senator.					
I have purchased an item that was being sold as part of a fundraiser for a charity.					
I have given money to a stranger who needed it or who asked for it.					
I have allowed someone to go ahead of me in a line (at copy machine, at supermarket)					
I have donated clothes or goods to a charity.					
I have returned a lost item to some authority (a Lost and Found office, the police, an administrator)					

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