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# Individual evolutionary learning, other-regarding preferences, and the voluntary contributions mechanism<sup>☆</sup>

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## ABSTRACT

The data from experiments with the Voluntary Contributions Mechanism suggest five stylized facts, including the restart effect. To date, no theory has explained all of these facts simultaneously. We merge our Individual Evolutionary Learning model with a variation of heterogeneous other-regarding preferences and a distribution of types to provide a new theory that does. In addition, our theory answers some open questions concerning the data on partners–strangers experiments. One interesting feature of the theory is that being a conditional cooperator is not a type but arises endogenously as a behavior. The data generated by our model are quantitatively similar to data from a variety of experiments, and experimenters, and are insensitive to moderate variations in the parameters of the model. That is, we have a robust explanation for most behavior in VCM experiments.

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## 1. Introduction

The voluntary contributions mechanism (VCM) is often used to decide how much of a public good to produce and how to fund it. Beginning with the pioneering work of [Marwell and Ames \(1979\)](#), [Isaac et al. \(1985\)](#) and [Kim and Walker \(1984\)](#), there have been an amazing range of experiments involving the VCM in linear public goods environments. In this paper we focus on the experiments that involve repeated play. Five well-known<sup>1</sup> stylized facts from many experiments are:

1. Average contributions begin at around 50% of the total endowment and then decline with repetition, but not necessarily to zero.
2. There is considerable variation in individual contributions in each repetition. Some give everything. Some give nothing. The individual contributions also show no consistent monotonic pattern over time. Some increase, some decrease, and some have a zig-zag pattern.
3. Increases in the marginal value of the public good relative to the private good lead to an increase in the average rate of contribution. This is particularly true in later repetitions and for small groups.
4. Increases in the size of the group lead to an increase in the average rate of contribution. This is particularly true in later repetitions and for small values of the marginal value of the public good relative to the private good.
5. There is a restart effect; that is, if after 10 periods the subjects are told the game is restarting, then contributions in period 11 increase over those in period 10.

It has not been too hard to come up with a reasonably sensible theory that explains one or two of the qualitative features of these

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<sup>1</sup> See [Ledyard \(1995\)](#), [Holt and Laury \(2008\)](#), and [Chaudhuri \(2011\)](#).

stylized facts, although most of the effort has been spent on explaining just the first. It has been hard to come up with a theory that explains all five facts. It has been even harder to match the quantitative findings of the various experiments and experimenters. Standard game theory provides no help in understanding these facts. In linear experiments, contributing zero is the dominant strategy in a one-shot game. Solving backwards, one finds that zero contribution is also the equilibrium in games with multiple rounds. If one believes that all subjects care only about their own payoff, then one cannot explain positive contributions in VCM experiments, except perhaps as serious mistakes. There have been many suggested modifications to the standard theory in an attempt to explain the experimental data. Holt and Laury (2008) do an excellent job of summarizing much of the earlier literature.

More recently, there is a developing consensus that the levels of contributions seen in VCM experiments are due to conditional cooperation on the part of some players. As Chaudhuri (2011, p.56) summarizes in his excellent survey article: "...many participants in linear public goods games are conditional cooperators whose contributions to the public good are positively correlated either with their *ex ante* beliefs about the contributions to be made by their peer or to the actual contributions made by the same." But this still leaves open the question as to the theoretical basis for this behavior. There have been two basic dimensions in which the literature has carried out the search for a good theory. The first dimension involves the characteristics of the agents, their preferences and attitudes. The second involves the behavior of the agents, how they play a game.

*Characteristics.* While it is a bit of a simplification, two main approaches have been taken in defining the characteristic of an agent: other-regarding preferences and reciprocity. The idea behind the other-regarding preference approach is simple. The experimenter controls the payoff to each subject, but subjects also care about the distribution of the experimental payoffs. Each subject has a utility function that depends on others' payoffs and that is not controlled by the experimenter. Those taking the other-regarding preference approach<sup>2</sup> include Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Charness and Rabin (2002), and Cox et al. (2007, 2008)).

The reciprocity approach also comes in several flavors. It is sometimes assumed that there are agents who are hard wired as conditional cooperators; that is, these agents will behave as conditional cooperators no matter what. It is in their nature. See, e.g., Ambrus and Pathak (2011). At other times it is assumed that agents have a taste for reciprocity behavior; that is, they get direct utility from cooperating with a cooperator or direct disutility from cooperating with a non-cooperator. In these theories, they cooperate because they like to. See, e.g., Charness and Rabin (2002). Those taking the reciprocity approach include Rabin (1993), Dufwenberg and Kirchsteiger (2004), Wendel and Oppenheimer (2007), and Ambrus and Pathak (2011). Charness and Rabin (2002) mixes both other-regarding preferences and reciprocity.

*Behavior.* The second dimension considers the dynamics of the repeated game problem and how this affects the observed behavior of the agents. Again it is a bit of a simplification but the literature seems also to have split into two approaches here: strategic or learning. In the strategic approach, agents are assumed to follow some game theoretic equilibrium behavior when they play the repeated VCM game. This requires agents to have a serious base of common knowledge, that subjects rarely have, about the rationality and behavior of others as well as about the parameters of the games. As early example of this for prisoner dilemma games is found in Kreps et al. (1982) who introduce the possibility of an altruist, one who always cooperates in prisoner dilemma games.<sup>3</sup> Under an assumption of

common knowledge of Bayesian beliefs, reputation can then induce selfish types to cooperate, i.e. to mimic the altruist, for some number of periods.

Those taking this approach with other-regarding preferences include Anderson et al. (1998), Fehr and Schmidt (1999), and Andreoni and Samulerson (2006). Those taking this approach with reciprocity include Ambrus and Pathak (2011).

In the learning approach, it is usually assumed that subjects are reacting to past choices of others in some kind of best response way. This requires no common knowledge among the agents. Those taking this approach with reciprocity include Wendel and Oppenheimer (2007). Those taking this approach<sup>4</sup> with other-regarding preferences include Anderson et al. (2004), Cooper and Stockman (2002), and Janssen and Ahn (2006).

### 1.1. Our approach

We take a very standard and simple approach to modeling. We merge other-regarding preferences and learning. We provide a common functional form for the utility received by each subject from the outcome of an experiment. Combined with their initial endowments and the rules of a VCM experiment, this will define a game. We also provide a theory about how subjects will play such games.<sup>5</sup> We do not assume they are fully strategic, but instead they learn how to play. Whether they learn to behave selfishly, altruistically or as conditional cooperators arises endogenously as a result of the combination of the parameters of the game and their preferences.

*Characteristic.* In our model, agents have other-regarding preferences (ORP) over outcomes. They neither know nor care about the intentions or preferences of others. Each subject's utility depends on their own payoff, the average payoff to the group, and the amount by which their payoff is less than the average payoff to the group. These three pieces reflect, respectively, a personal preference, a social preference, and a preference for fairness to self. Since the experiments are for relatively small stakes, we further assume as a local approximation that each subject's utility function is linear in its variables. To complete the utility formulation, we assume that an agent's two utility parameters, their marginal utility for altruism and their marginal disutility for envy, are independently and identically drawn from a probability distribution. That is, although there is a common functional form for the utilities, there is heterogeneity among agents in the parameters of that function.

All three pieces of the payoff are important and necessary to explain the contributions in linear public good experiments. Without the fairness component, in the stage game equilibrium contributions will be either to give nothing or to give everything. While both of these behaviors are observed in experiments, this would imply there are no conditional cooperators leaving contributions between all or nothing to be explained by confusion. Without the social component, equilibrium contributions in the stage game would be zero for everyone, which is clearly inconsistent with the evidence. As we will see, with all three pieces we can explain the existence of three types of equilibrium behavior: giving nothing, giving everything and conditional cooperation.<sup>6</sup>

<sup>3</sup> It is possible to compare prisoner dilemmas with voluntary contribution mechanisms by thinking of the strategy in a prisoner dilemma as the probability of cooperating and comparing that to the strategy in a VCM which is the percent of endowment contributed.

<sup>4</sup> We further discuss these papers below in Section 2.3.1.

<sup>5</sup> It is important to emphasize that we are not describing how subjects *should* play the games. Instead we want our theory to tell us how they behaved in the experiments they were in and how they would change their behavior if we changed the parameters of the experiments.

<sup>2</sup> We further discuss these papers below in Section 2.2.2.

It is important to note that we do not assume anything about reciprocity. Instead, reciprocal strategies emerge as an endogenous feature of the model. It is not necessary to force conditional cooperation by assuming it as an exogenous fact or as something one gets utility from. Other-regarding preferences can provide a natural economic foundation on which conditional cooperation is based.

*Behavior.* In our theory of behavior, we accept that not only are repeated game experiments with 10 or more periods too complex for subjects to be able to compute and deploy equilibrium strategies of the kind required to sustain the type of behavior seen in experiments, but also those equilibrium strategies would require a common knowledge of beliefs and common knowledge of rationality that is simply not possible to induce and control for in the laboratory.<sup>7</sup> Our maintained hypothesis, therefore, is that there is no strategic component to the behavior observed in repeated VCM experiments and that behavior is built entirely out of reactive learning.<sup>8</sup>

Our behavioral model is Individual Evolutionary Learning (IEL). In IEL, agents retain a finite set of remembered strategies. After each iteration, they update this set through experimentation and replication. Experimentation involves replacing, with low probability, some of the elements of the set with a strategy chosen at random from the entire strategy space. Experimentation introduces strategies that might otherwise never have a chance to be tried. Replication goes through the set of remembered strategies and, using a series of random paired comparisons within the set, replaces the strategies which would have provided a low payoff in the previous period with copies of those that would have yielded a higher payoff. Over time the remembered set becomes homogeneous with copies of the “best reply” strategy. To generate their strategic choices, individuals choose strategies from the set of remembered strategies, at random, proportionately to the payoff they would have received had they been played in the last round. The end result of IEL is a Markov process in strategies where the remembered set of each agent co-evolves with the sets of all other agents.

*Summary of findings.* Our model generates behavior similar to that found in various experiments by various experimenters. We show in Section 2.2.1 that conditional cooperation arises naturally and endogenously as a behavior. The data generated by our model replicate all five of the stylized facts from above. We show in Section 5.2 that, contrary to conventional wisdom, a simple learning model can reproduce the restart phenomenon. A sophisticated strategic model is not necessary. We are also able to provide an explanation in Section 5.1 for the previously confusing data on partners versus strangers.

The data generated by our model are quantitatively similar to that from a range of different experiments and different experimenters. We compare data generated by our model to data from experiments by Isaac and Walker and by Andreoni. In those experiments, both the number of people in a group and the marginal rate of return for the public good are varied systematically. Our data compare very favorably to theirs. We find that the average contributions generated by our model differ from those in the experiments by only 3.4% to 6.6%.

<sup>6</sup> Our approach with respect to other-regarding preferences is very similar to Andreoni and Samulerson (2006) who analyze a two period prisoners' dilemma. In their model, agents have utility functions with a single parameter,  $\alpha$ . If  $\alpha < 0$  then the prisoner is an unconditional cooperator, if  $\alpha > 1$  then the prisoner is an unconditional defector, and if  $0 < \alpha < 1$  the prisoner is a conditional cooperator.

<sup>7</sup> Ambrus and Pathak (2011) run an experiment where they first run subjects through a session which they use to identify conditional cooperators. They then run a session of the repeated VCM in which the identity of the conditional cooperators is common knowledge. But in the standard VCM experiments we are concerned with, subjects have no such information about each other. In fact, in the modern computer based lab, a subject usually only knows the others as numbers with no basis for any prior common knowledge about their characteristics.

<sup>8</sup> We actually believe that subjects in repeated VCM experiments do follow some types of strategic behavior, especially when there are only 2 or 3 periods left. But in this paper we are focusing on just how far one can get with only learning. See Section 7 for more on adding strategic features to our model.

Finally, the precise values of the few parameters of our model do not matter very much. IEL has 3 parameters: the size of the remembered set, and the rate and range of the experimentation. ORP has 3 parameters: those that determine the probability distribution on types. We show that the differences between our model and experimental data change very little as we change the 6 parameters over a fairly wide range. This suggests that if one wants to transfer our model to other VCM experiments or to other experiments like auctions in a private goods world, there will be little need to recalibrate the parameters. In fact, the IEL parameters we end up using here are essentially the same as those in call market experiments in Arifovic and Ledyard (2007) and Groves–Ledyard experiments in Arifovic and Ledyard (2004, 2011).

We believe we have a robust explanation for a wide range of experiments. We now turn to a fuller description of our approach and the research findings.

## 2. VCM, IEL, and ORP: the theory

We begin with a description of a classic experiment with the voluntary contributions mechanism (VCM) in a linear public goods environment. We then describe the other-regarding preferences (ORP) utility model. Finally, we describe the individual evolutionary learning (IEL) theory of behavior. After we have described our full theory, we discuss its relation to other models and experimental evidence in the literature.

### 2.1. The VCM in linear environments

A linear public good environment consists of  $N$  agents, numbered  $i = 1, \dots, N$ . Each agent has a linear payoff function  $\pi^i = p^i(w^i - c^i) + y$ , where  $1/p^i$  is their marginal willingness to pay in the private good for a unit of the public good,  $w^i$  is their initial endowment of a private good,  $c^i$  is their contribution to the production of the public good, where  $c^i \in [0, w^i]$ , and  $y$  is the amount of the public good produced. The linear production function is  $y = M \sum_{j=1}^N c^j$  where  $M$  is the marginal product of the public good.

The VCM in a linear environment creates a simple game. There are  $N$  players with strategies  $c^i \in [0, w^i]$  and payoffs  $\pi^i(c) = p^i(w^i - c^i) + M \sum c^j$ . In what follows, we will only consider symmetric VCM problems.<sup>9</sup> We will assume that  $p^i = 1$  and  $w^i = w$  for all  $i$ . For symmetric linear VCMs, it is easy to see that if  $M < 1$  then each  $i$  has a dominant strategy of  $c^i = 0$ . It is also true that if  $M > (1/N)$ , then the aggregate payoff is maximized if  $c^i = w, \forall i$ . When  $(1/N) < M < 1$ , there is a tension between private and public interest that is the basis of the standard commons dilemma.

To illustrate the type of experiments seen with the VCM we look at the classic experimental design of Mark Isaac and James Walker that has served as the jumping off point for much of the experimental work that has gone on since. They ran a number of experiments in which subjects use the VCM to decide public good allocations.<sup>10</sup> In an experiment,  $N$  individuals are gathered together. Each individual begins each period with  $w$  tokens. Each individual chooses how many of their tokens to contribute to a group exchange with a total group return of  $NM \sum c^i$ . After summing the  $c^i$ , the per capita individual return,  $M \sum c^i$ , is calculated and each individual receives this amount. Each individual also receives  $w - c^i w - c^i$  from their individual exchange. Thus the payment received by  $i$  for this round is  $\pi^i(c) = w - c^i + M \sum c^i$ .

Isaac and Walker (1988) used a  $2 \times 2$  design with  $N$  equal to 4 and 10 and  $M$  equal to 0.3 and 0.75. They structured the experiment as a repeated game, with no re-matching, played for  $T = 10$  periods. We display, in

<sup>9</sup> We will indicate in the appropriate places how our theory applies more generally to asymmetries in  $p^i$  and in  $w^i$ .

<sup>10</sup> See for example, Isaac et al. (1984), Isaac and Walker (1988), and Isaac et al. (1994).

Table 1, the average rate of contribution,  $(\sum_t \sum_i c_t^i / (10Nw))$  for each of the 10 periods for each of the 4 treatments in that set of experiments.<sup>11</sup> There are six observations per treatment, all with experienced subjects.

These Isaac and Walker (1988) data are consistent with and representative of the first four stylized facts listed in the introduction. But what behavior are the subjects really following? The standard full rationality, game theory prediction is that subjects will use their dominant strategy,  $c^i = 0$ . This is obviously inconsistent with these data. So we need a new theory.

### 2.2. Other-regarding preferences

Some have suggested that subjects bring other-regarding preferences into the lab which the experimenter cannot control and which cause the subjects to behave differently than predicted if one ignores these preferences. Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Charness and Rabin (2002), Cox and Sadiraj (2007) and Cox et al. (2008) are among those who have taken this route. We introduce such an assumption by adding a dash of social preference and envy to some, but not all, agents.

In the experiments, each agent receives a payoff  $\pi^i(c) = w - c^i + M \sum c^j$ . We take social preference to be a preference for higher values of the average payoff to all agents,<sup>12</sup>  $\bar{\pi} = \sum \pi^i / N = w - \bar{c} + M \bar{c}$  where  $\bar{c} = \sum c^i / N$ . We take (one-sided) envy to be a disutility for being taken advantage of<sup>13</sup> which happens when  $\bar{\pi}(c) > \pi^i(c)$ . That is,  $i$  loses utility when  $i$ 's payoff is below the average payoff in this group.<sup>14</sup> Finally because payoffs are small, we assume utility is linear. We model subjects as having a utility function

$$u^i(c) = \pi^i(c) + \beta^i \bar{\pi}(c) - \gamma^i \max\{0, \bar{\pi}(c) - \pi^i(c)\} \quad (1)$$

with  $\beta^i \geq 0$  and  $\gamma^i \geq 0$ .

It is clear from experimental data that there is considerable heterogeneity in the population with respect to levels of other-regarding behavior. Also the experimenter can neither observe nor control for each agent's values of  $(\beta, \gamma)$ . We model the heterogeneity by assuming that subject  $i$  comes to the lab endowed with a particular value  $(\beta^i, \gamma^i)$ . We model the lack of control by assuming each  $(\beta^i, \gamma^i)$  is distributed independently and identically in the population according to a distribution  $F(\beta, \gamma)$ . We will be more precise about the functional form of  $F(\cdot)$  below.

#### 2.2.1. Equilibrium behavior with ORP

It will be useful later to know what the one-shot Nash Equilibrium levels of contributions are for any particular draw of subject parameters  $(\beta, \gamma)$ . With linear other-regarding preferences (Eq. 1), given  $(N, M)$  and heterogeneity across  $(\beta, \gamma)$ , there are only three types of Nash Equilibrium strategies: free riding ( $c^i = 0$ ), fully contributing ( $c^i = w^i$ ), and

**Table 1**  
Average rate of contribution (%) per subject by period.

	t=1	2	3	4	5	6	7	8	9	10
M=.3 N=4	34.3	29.5	17.6	10.1	7.7	9.0	7.1	4.2	2.3	5.8
M=.3 N=10	46.3	39.5	32.2	26.9	33.0	30.3	20.8	21.9	8.6	
M=.75 N=4	54.7	56.3	62.3	57.3	62.3	49.5	45	47.7	33.3	29.2
M=.75 N=10	47.5	56.2	57.8	53	51.7	40.0	51.0	43.7	33.8	29.8

Source: Isaac and Walker (1988).

conditionally cooperating ( $c^i = \bar{c} = (\sum_i c^i) / N$ ). In equilibrium,

$$c^i = \begin{cases} 0 \\ \bar{c} \\ w \end{cases} \text{ if } \begin{cases} 0 \geq \left[ \left( M - \frac{1}{N} \right) \beta^i + M - 1 \right] \\ \gamma^i \left( \frac{N-1}{N} \right) \geq \left[ \left( M - \frac{1}{N} \right) \beta^i + M - 1 \right] \geq 0 \\ \gamma^i \left( \frac{N-1}{N} \right) \leq \left[ \left( M - \frac{1}{N} \right) \beta^i + M - 1 \right] \end{cases} \quad (2)$$

The ratio  $\frac{1-M}{M-\frac{1}{N}}$ , acts as a differentiator between 0 contributions and positive contributions in equilibrium for VCM. This is also true in non-linear utility approaches to other-regarding preferences as found, for example, in Cox et al. (2007, 2008) and Cox and Sadiraj (2007).

A key point to remember as one reads this paper is that knowing an agent's parameters  $(\beta^i, \gamma^i)$  alone is not enough to determine whether they will behave selfishly (i.e., give nothing), altruistically (i.e., give everything), or as a conditional cooperator (giving something which depends on the contributions of others). Which strategy a particular agent uses depends both on that agent's parameters  $(\beta^i, \gamma^i)$  and on the parameters of the environment  $(N, M)$ . That is, altruism and conditional cooperation are behaviors that arise from other-regarding preferences when the environment provides the setting for it. A subject may act altruistically when  $M = 0.7$  but act selfishly when  $M = 0.3$ . For example, consider a subject in an experiment with  $N = 4$  and with ORP parameters  $\gamma = 4$  and  $5 < \beta < 14$ . In equilibrium, this subject will contribute everything if  $M = 0.7$  and will contribute nothing if  $M = 0.3$ .

To compute the equilibrium for a particular draw of parameters, one first counts how many types there are who will give nothing, those for whom  $0 \geq \left( M - \frac{1}{N} \right) \beta^i + M - 1$ . Let  $N_{fr}$  be the number of these types who will act as free riders in equilibrium in this environment. Next one counts how many types there are who will act altruistically and give  $w$  in equilibrium, those for whom  $\gamma^i \left( \frac{N-1}{N} \right) \leq \left( M - \frac{1}{N} \right) \beta^i + M - 1$ . Let  $N_a$  be the number who act altruistically in this environment. In equilibrium, the average percentage contribution is  $\bar{c} / w = N_a / (N_a + N_{fr})$ . If  $N_a = N_{fr} = 0$ , then there is a continuum of equilibria where  $c^i = \bar{c}, \forall i$ . But this is rare in the data.

One should note that the fact that subjects have other-regarding preferences does not eliminate the fundamental social dilemma. First, for the preferences in Eq. (1) it is still optimal for everyone to give everything and yet the equilibrium (as long as there is at least one player with  $0 > \left( M - \frac{1}{N} \right) \beta^i + M - 1$ ) is that many give less than that. Second, playing the equilibrium (Eq. 4) in every period is a sub-game perfect Nash Equilibrium for the finitely repeated game. So the SPE allocation remains less than optimal even if some agents have other-regarding preferences.

Finally, it is straight-forward to do comparative statics on this equilibrium. In particular, for any specific realization of the parameters,  $\partial N_{fr} / \partial M \leq 0$  and  $\partial N_a / \partial M \geq 0$ . So  $\partial \bar{c} / \partial M \geq 0$ . Further,  $\partial N_{fr} / \partial N \leq 0$

<sup>11</sup> We thank Mark Isaac and James Walker for providing us with all of their data from the 1984, 1988, and 1994 papers.

<sup>12</sup> We considered having preferences depend on the sum of payoffs to all,  $\sum \pi^i$ , but that swamps everything else when  $N$  becomes larger, which does not seem to be consistent with the data.

<sup>13</sup> We considered having preferences depend symmetrically and 2-sidedly on  $(\bar{\pi} - \pi^i)^2$ , as in Ledyard (1995), but that produced behavior that is inconsistent with the data since the decline in contributions over time predicted by the model is too slow under this hypothesis.

<sup>14</sup> Since we are dealing with a class of experiments in which subjects know only the sum or average of others' contributions and not the individual amounts, we only consider utility functions which depend on the average of others.

and  $\partial N_a/\partial N \geq 0$ . So  $\partial \frac{\bar{c}}{w}/\partial N \geq 0$ . If the distribution of the parameters,  $F(\cdot)$  is continuous, then the comparative statics of the expected value of  $\frac{\bar{c}}{w}$  all hold with strict inequality. That is, this model of other-regarding preferences is consistent in equilibrium with the stylized facts 3 and 4 at the beginning of this paper.

2.2.2. Relation to other ORP models

Others have proposed other-regarding preferences before us, suggesting a variety of functional forms and parameter restrictions. Eq. (1) is intimately related to those of Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Charness and Rabin (2002). The differences in functional form are to some extent irrelevant (particularly in their linear forms). All are equivalent up to linear transformations of the other, and equivalent utility functions will yield identical behavior for a wide range of behavioral models including Expected Utility Maximizing and our learning model below.

The Fehr–Schmidt asymmetric fairness utility function is:

$$u_{FS} = \pi^i - \zeta^i \max\{\bar{\pi}_{-i} - \pi^i, 0\} - \eta^i \max\{\pi^i - \bar{\pi}_{-i}, 0\} \tag{3}$$

where  $\bar{\pi}_{-i} = \frac{1}{N-1} \sum_{j \neq i} \pi^j$ .

To see the equivalence notice that  $(\bar{\pi}_{-i} - \pi^i) = \frac{N}{N-1} (\bar{\pi} - \pi^i)$  and that  $\max\{\pi^i - \bar{\pi}, 0\} = (\pi^i - \bar{\pi}) + \max\{\bar{\pi} - \pi^i, 0\}$ . So  $u_{FS} = \pi(1-\Delta) + \Delta\bar{\pi} - (\Gamma + \Delta) \max\{\bar{\pi} - \pi^i, 0\}$ , where  $\Gamma = \frac{N}{N-1} \zeta$  and  $\Delta = \frac{N}{N-1} \eta$ . Given  $u_{FS}$ ,

let  $\beta = \frac{\Delta}{1-\Delta}$  and  $\gamma = \frac{\Gamma + \Delta}{1-\Delta}$ . Then it will be true that  $u_{AL} = \Lambda u_{FS}$  where

$u_{AL}$  is our utility functional form and  $\Lambda = \frac{1}{1-\Delta}$ . Also, given  $u_{AL}$ , we can

derive  $u_{FS} = \frac{1}{1+\beta} u_{AL}$ .

Our functional form, Eq. (1), is also equivalent to that of Charness and Rabin (2002), if one removes the retaliatory term that adjusts for bad behavior. Their utility function is:

$$\begin{aligned} u &= (\chi + \theta q)\bar{\pi}_{-i} + (1 - \chi - \theta q)\pi^i \text{ if } \pi^i \geq \bar{\pi}_{-i} \text{ and} \\ u &= (\xi + \theta q)\bar{\pi}_{-i} + (1 - \xi - \theta q)\pi^i \text{ if } \pi^i \leq \bar{\pi}_{-i}. \end{aligned} \tag{4}$$

where  $q = -1$  if the others have misbehaved and  $q = 0$  otherwise. This is the same as Eq. (7) if  $\eta = \chi$ ,  $\zeta = -\xi$ , and  $\theta = 0$ .

Finally, Eq. (1) is also equivalent to a linear version of ERC from Bolton–Ockenfels. The version of that used in Cooper and Stockman (2002) is  $u^i = \pi^i - \gamma^i \max\{0, \bar{\pi} - \pi^i\}$ . Let  $\beta^i = 0$  in Eq. (1).

Given the equivalence of the varied functions, it is instructive to compare and contrast our restrictions  $\beta \geq 0$ ,  $\gamma \geq 0$  with those of Fehr–Schmidt and Charness–Rabin. We all pretty much agree on  $\beta$  and  $\zeta = \frac{N-1}{N} \frac{\beta}{1+\beta}$ . Our assumption that  $\beta \geq 0$  implies  $0 \leq \zeta \leq \frac{N-1}{N}$  which implies their restriction that  $0 \leq \zeta \leq 1$ . But we differ in our restrictions on  $\gamma$  and  $\eta = \frac{N-1}{N} \frac{\gamma - \beta}{1+\beta}$ . Fehr–Schmidt focus on difference

aversion with the restriction that  $\eta \geq \zeta$ . Charness–Rabin consider several parameter constellations but focus on social welfare preferences with the restrictions that  $\eta + \zeta \geq 0$  and  $-1/2 \leq \eta < 0$ . Our restriction that  $\gamma \geq 0$

is equivalent to  $\eta + \zeta \geq 0$  when  $\beta \geq 0$ , since  $\eta + \zeta = \frac{N-1}{N} \frac{\gamma}{1+\beta}$ . However,

differences arise over the rest. The restriction that  $\eta \geq \zeta$  is the same as  $\gamma \geq 2\beta$  while the restriction that  $\eta \geq -1/2$  is the same as  $\gamma \geq (1/2)$

$\left[ \frac{N-2}{N-1} \beta - \frac{N}{N-1} \right]$ . As we will see, these latter two are a little tenuous when confronted with data from VCM experiments.

The linear multiplier converting Fehr–Schmidt and others to (Eq. 1) and vice versa does depend on  $N$ . That is not a problem from an

individual decision theoretic point of view, since  $N$  is never a choice variable of any agent. However, the fact that  $N$  is involved in the transformation does matter when one carries the functional form across VCM experiments with different size groups. If the distribution of types is independent of  $N$  then how  $N$  enters the utility function does affect how average group play changes as  $N$  changes. An example of this can be seen in the, perhaps unintended, implication of the Fehr–Schmidt, Charness–Rabin preference model that equilibria for the VCM are independent of  $N$ . It is easy to show that, in an equilibrium of their models, an agent will be a free rider if  $\zeta \leq (1 - M)$ , will be conditionally cooperative if  $\eta \geq M - 1$  and  $\zeta \geq (1 - M)$ , and will be altruistic if  $\eta \leq M - 1$  and  $\zeta \geq (1 - M)$ . So, unless the distribution of types  $(\eta, \zeta)$  is dependent on  $N$ , an assumption they never suggest, their models imply that the average rate of contributions in a VCM experiment will be independent of  $N$ . But this is not consistent with the data. For example, in the Isaac and Walker (1988) data for  $M = 0.3$ , contributions are significantly higher for  $N = 10$  than for  $N = 4$ . Our ORP model picks up this variation.

We believe that the principles of social preference and fairness that lie behind the Fehr–Schmidt and Charness–Rabin other-regarding preferences, are sound and fundamental. However, because their functional forms and parameter restrictions conflict with the data from VCM experiments, we believe a new functional form is necessary. We propose Eq. (1) as the natural alternative.

But the static equilibrium misses two key features in the data: a wide variance among contributions and declining contributions over time. Other-regarding preference by itself is not a sufficient explanation for the behavior observed in experiments.

2.3. Individual evolutionary learning

Given the ORP utility function of each subject and the rules of the VCM experiment in a linear public goods environment, one has a game. The next step is to provide a model for how subjects play this game. Our model, IEL, is based on an evolutionary process which is individual, and not social. In IEL, each agent is assumed to carry a collection of possible strategies in their memory. These remembered strategies are continually evaluated and the better ones are used with higher probability. IEL is particularly well-suited to repeated games with large strategy spaces such as convex subsets of the real line. We first describe it for general experiments. In the next section we indicate how it is specialized for VCM.

An experiment can often be modeled as a repeated game  $(\mathcal{G})$ . The repeated game has a stage game  $\mathcal{G}$  and a number of rounds,  $T$ . The idea is that  $\mathcal{G}$  will be played for  $T$  rounds. In  $\mathcal{G} = \{\mathcal{N}, \mathcal{X}, \subseteq, \mathcal{I}\}$ ,  $N$  is the number of subjects,  $X^i$  is the action space of  $i$ ,  $v^i(x^1, \dots, x^N)$  is  $i$ 's payoff if the joint strategy choice is  $x$ , and  $I^i(x_t)$  describes the information reported to subject  $i$  at the end of a round. These are all controlled by the experimenter. In round  $t$ , each subject chooses  $x_t^i \in X^i$ . At the end of round  $t$ , subject  $i$  will be told the information  $I^i(x_t)$  about what happened. Then the next round will be played. A behavioral model must explain how the sequence of choices for  $i$ ,  $(x_t^1, x_t^2, \dots, x_t^N)$  is made, given what  $i$  knows at each round  $t$ .

The primary variables of our behavioral model are a finite set of remembered strategies for each agent  $i$  at each round  $t$ ,  $A_t^i \subset X^i$  and a probability measure,  $\psi_t^i$  on  $A_t^i$ .  $A_t^i$  consists of  $J$  alternatives. A free parameter  $J$  can be loosely thought of as a measure of the processing and/or memory capacity of the agent. In round  $t$ , each agent selects an alternative randomly from  $A_t^i$  using the probability density  $\psi_t^i$  on  $A_t^i$  and then chooses the action  $x_t^i = a_t^i$ . One can think of  $(A_t^i, \psi_t^i)$  as inducing a mixed strategy on  $X^i$  at  $t$ . At the end of each round  $t$ , agents are told  $r(x_t)$ . At the beginning of the next round  $t + 1$ , each agent computes a new  $A_{t+1}^i$  and  $\psi_{t+1}^i$ . This computation is at the heart of our behavioral model and consists of three pieces: experimentation, replication, and selection.

We begin at the end of round,  $t$ , knowing  $A_t^i, \psi_t^i$ , and  $I^i(x_t)$ .

*Experimentation* comes first. Experimentation introduces new alternatives that otherwise might never have a chance to be tried. This insures that a certain amount of diversity is maintained. For each  $j = 1, \dots, J$ , with probability  $\rho$ , a new contribution is selected at random from  $X^i$  and replaces  $a_{j,t}^i$ . We use a normal density, conditional on  $X^i$ , for this experimentation. For each  $j$ , the mean value of the normal distribution is set equal to the value of the alternative,  $a_{j,t}^i$  that is to be replaced by a 'new' idea. That is the new alternative  $a \sim N(a_{j,t}^i, \sigma)$ .  $\rho$  and  $\sigma$  are free parameters of the behavioral model that can be varied in the simulations.

*Replication* comes next. Replication reinforces strategies that would have been good choices in previous rounds. It allows potentially better paying strategies to replace those that might pay less. The crucial assumption here is the measure of "potentially better paying strategies". We let  $v^i(a_j^i | I^i(x_t))$  be the forgone utility of alternative  $j$  at  $t$  given the information  $I^i(x_t)$ . This measures the utility  $i$  thinks she would have gotten had she played  $a_j$  in round  $t$ .  $v^i(a_j | I^i(x_t))$  is a counter-factual valuation function and must be specified for each application.

Given a forgone utility function,  $v^i$ , we can describe how replication takes place. For  $j = 1, \dots, J$ ,  $a_{j,t+1}^i$  is chosen as follows. Pick two members of  $A_t^i$  randomly (with uniform probability) with replacement. Let these be  $a_{k,t}^i$  and  $a_{l,t}^i$ . Then

$$a_{j,t+1}^i = \begin{cases} a_{k,t}^i \\ a_{l,t}^i \end{cases} \text{ if } \begin{cases} v^i(a_{k,t}^i | I^i(x_t)) \geq v^i(a_{l,t}^i | I^i(x_t)) \\ v^i(a_{k,t}^i | I^i(x_t)) < v^i(a_{l,t}^i | I^i(x_t)) \end{cases}$$

Replication for  $t + 1$  favors alternatives with a lot of replicates at  $t$  and alternatives that would have paid well at  $t$ , had they been used. So it is a process with a form of averaging over past periods. If the actual contributions of others have provided a favorable situation for an alternative  $a_{j,t}^i$  on average then that alternative will tend to accumulate replicates in  $A_t^i$  (it is fondly remembered), and thus will be more likely to be actually used. Over time, the sets  $A_t^i$  become more homogeneous as most alternatives become replicates of the best performing alternative.

*Selection* is last. Each contribution  $a_{k,t+1}^i$  is selected with the following probability<sup>15</sup>:

$$\psi_{k,t+1}^i = \frac{v^i(a_{k,t+1}^i | I^i(x_t)) - \varepsilon_{t+1}^i}{\sum_{j=1}^J (v^i(a_{j,t+1}^i | I^i(x_t)) - \varepsilon_{t+1}^i)}$$

for all  $i \in \{1, \dots, N\}$  and  $k \in \{1, \dots, J\}$  and where<sup>16</sup>

$$\varepsilon_{t+1}^i = \min_{a \in A_{t+1}^i} \{0, v^i(a | I^i(x_t))\}.$$

We now have a complete description of the way that an IEL agent moves from  $A_t^i$  and  $\psi_t^i$  to  $A_{t+1}^i$  and  $\psi_{t+1}^i$ . The only remaining thing to pin down is the initial values,  $A_1^i$  and  $\psi_1^i$ .

*Initialization.* We use the simplest possible initialization. It is very naive behavior but this has proven successful in our previous work and we see no need to change it now. We assume that things begin randomly. We let  $A_1^i$  be populated randomly with  $J$  uniform draws from  $X^i$ . We let  $\psi_{k,1}^i = 1/J \forall k$ .

We now have a complete model of behavior for a general repeated game. The two determining components of IEL are  $A$  and  $v(a | I(x))$ . The three free parameters are  $(J, \rho, \sigma)$ .

### 2.3.1. Relation to other learning models

**Andreoni and Miller (1991)** use the replicator dynamic to simulate behavior in linear public goods VCM environments. They show that the replicator dynamic will produce decay towards free riding over time as more cooperative strategies will be replaced by those that give less to the public good, although the decay seems much slower than that occurring in the experiments. Given sufficient time, the entire population will use the strategy that does the most free riding. The decay to free riding is slower the larger the group size and the larger the marginal return. But the decay always goes to zero which is inconsistent with the higher rates of contribution seen in experiments with high marginal value of the public good. Any learning rule that searches for and finds better paying strategies will have this problem as long as individual preferences are selfish. Since contributing zero is a dominant strategy, it is always better paying and will eventually be found. So contributions will always converge to zero. Learning by itself is not a sufficient explanation for the behavior observed in experiments.

Three papers have taken the obvious step and combined other-regarding preferences and learning.<sup>17</sup> **Anderson et al. (2004)** introduce a learning model "in which agents adjust their decisions toward higher payoffs, subject to normal error." This dynamic does converge to the equilibrium with errors described earlier. But it is a continuous time model and it is not clear how one would calibrate its time pattern to a repeated experiment where contributions, and therefore information, come in discrete intervals.

**Cooper and Stockman (2002)** merge a **Bolton and Ockenfels (2000)** type utility function with the Reinforcement Learning (RL) model of **Roth and Erev (1995)**. **Janssen and Ahn (2006)** merge a **Charness and Rabin (2002)** type social utility function with the Experience Weighted Attraction (EWA) learning model of **Camerer and Ho (1999)**. In both cases, the authors are reasonably successful in generating behavior similar to that observed in their respective experiments. But, in both cases there are some problems with their approaches.

One drawback is in the utility functions they use. The utility function used by Cooper and Stockman ignores altruistic motives. This is not a problem for the environment they study—a 3 player minimal contributing set (MCS) game. However, as we will see below, the data from VCM experiments reject the hypothesis that there is no social preference involved. The utility function used by Janssen and Ahn has the property that individual equilibrium contributions are independent of the size of the group. However, as we will see below, the data from VCM experiments rejects the hypothesis that the size of the group does not matter.

Another drawback is in the learning models they use. Both Reinforcement learning used by Cooper and Stockman and the EWA learning used by Janssen and Ahn are ill suited to repeated games with strategy spaces that are a continuum. The models generate learning behavior that is generally much slower than that of experimental

<sup>15</sup> An alternative selection model would change the probabilistic choice function to  $\psi(a^k) = \frac{e^{\lambda v^i(a^k)}}{\sum_{a \in A^i} e^{\lambda v^i(a^k)}}$ . We have found (see **Arifovic and Ledyard (2004)**) that the behavior predicted for any  $\lambda$  differs very little from that generated by our proportional selection rule. This is because the set  $A$  tends to become homogeneous fairly fast, at which point the selection rule is irrelevant. We therefore use the proportional rule since it eliminates another parameter.

<sup>16</sup> This implies that if there are negative foregone utilities in a set, payoffs are normalized by adding a constant to each payoff that is, in absolute value, equal to the lowest payoff in the set.

<sup>17</sup> A paper by **Wendel and Oppenheimer (2007)** uses an agent-based model as we do, but they take a much different approach to learning and fairness. Agents' preferences include the other-regarding component that can be dampened by a 'sense' of exploitation, i.e. a sense of contributing more than the average. The sensitivity to the average contribution (that can change over time) makes preferences 'context-dependent'. Agents make probabilistic decisions given their utility functions in each time period. We prefer to maintain the approach of game theory and model utility separately from strategy. By doing so we believe the model will have broader applicability than to just VCMs.

subjects.<sup>18</sup> This does not seem to be much of a problem for the research reported in each of the papers, but it would limit transferability to more complex environments.

2.4. Merging VCM, IEL, and ORP

In applying IEL and ORP to the VCM experiments, the key choices one needs to make are the set of strategies,  $A$ , the forgone utility function,  $u^i$ , the form of the distribution of types,  $F$ , and the parameters of IEL  $J, \rho$ , and  $\sigma$ .

The choice of  $A$  is easy. In the VCM experiments subjects are given an endowment,  $w$  and told to choose at each iteration how much of that to contribute to the public good. That is, their contribution  $c^i \in [0, w]$ . Let  $A = [0, w]$  and let  $I^i(c_t) = \mu_t^i$  where  $\mu_t^i$  can be computed from  $\hat{c}_t = \sum_j c_t^j$  because  $i$  also knows  $c_t^i$  and  $u^i = \frac{\hat{c}_t - c_t^i}{N-1}$ .

For the choice of  $u^i$ , we assume that a subject will use the linear utility (Eq. 1) in evaluating their foregone utility. We can write  $i$ 's foregone utility as a function of  $i$ 's strategy,  $c^i$ , and the average of the others' contributions,  $\mu^i$ .

$$u^i(c^i | \mu^i) = \left[ (M-1) + \beta^i \left( M - \frac{1}{N} \right) - \gamma^{*i} \frac{N-1}{N} \right] c^i + \left[ M + \beta^i \left( M - \frac{1}{N} \right) + \frac{\gamma^*}{N} \right] (N-1) \mu^i + (1 + \beta^i) w \tag{5}$$

where

$$\gamma^{*i} = \begin{cases} \gamma^i & \text{if } \left\{ \bar{\pi} \geq \pi^i \right\} \\ 0 & \text{otherwise} \end{cases}$$

It is this function,  $u^i(a | I^i(c_t) = \mu^i)$ , we will use in the replication and selection stages of the IEL simulations.

For the distribution of types  $F(\cdot)$ , we assume first that  $P\%$  of the population are purely "selfish"; that is, they have the type (0,0). The rest,  $(1-P)\%$  of the population, have other-regarding preferences where the  $(\beta^i, \gamma^i)$  are distributed identically, independently, and uniformly on  $[0, B] \times [0, G]$ . We began with just a uniform distribution on  $\beta$  and  $\gamma$  but that did not fit the data at all, since it generated too few subjects who were free riders in equilibrium. So we added the atom at (0,0). One would ultimately like to have a distribution that would hold up over a lot of experiments and not just VCM experiments, but this will clearly require a lot more data.

The last choice to make when using IEL is which values of  $J, \rho$ , and  $\sigma$  to use. We will show, in Section 6.2, that the precise values of these parameters are of little importance. So, in this paper we will set  $J = 100, \rho = 0.033$ , and  $\sigma = w/10$ . These values are similar to the numbers we have used in our other IEL papers.

For the rest of this paper, we will refer to the model that combines IEL, ORP, and VCM as IELORP.

3. VCM, IEL, and ORP: the data

In this section we pin down the parameters of the distribution of preferences, examine the closeness of the fit to some experimental data, and discuss the implications for the distribution of types in the population.

3.1. Estimating (P, B, G)

In our model of other-regarding preferences, we assumed heterogeneity in the parameters  $(\beta, \gamma)$  of the utility functions. In particular we assumed that they were distributed identically and independently according to a distribution where the probability that  $(\beta, \gamma) = (0, 0)$  is

<sup>18</sup> See Arifovic and Ledyard (2004).

$P$  and the rest of the distribution is  $F(\beta, \gamma) = U([0, B]) \times U([0, G])$  where  $U(D)$  is the uniform density on the interval  $D$ . To provide an estimate of these subject population parameters  $(P, B, G)$  we calibrate our model to the data from Isaac and Walker (1988) described earlier.<sup>19</sup>

Using IELORP, we generated a number of simulated experiments varying the treatment  $(M, N)$  and the parameter triple  $(P, B, G)$ . We used the same pairs of  $(M, N)$  as in Isaac and Walker (1988). We varied  $P$  from 0.1 to 1, in increments of 0.1, and did a finer local search in the increments of 0.01. We varied  $B$  and  $G$  from 0 to 60, in increments of 1. We did not vary the IEL parameters  $(J, \rho, \sigma)$  since, as we will show in Section 6.2, such variation is really unnecessary.

For each treatment and for each parameter triple, we conducted 40 trials. Each trial involves a draw of a new type from  $F(\cdot)$  for each agent. Those types were selected randomly as follows. Each agent became selfish with probability  $P$ . If his type turned out to be selfish, then his utility was based only on his own payoff. That is,  $\beta^i = \gamma^i = 0$ . If the agent did not become selfish, then we drew values of  $\beta^i$  and  $\gamma^i$  uniformly and independently from the ranges  $[0, B]$ , and  $[0, G]$  respectively.

After running all of these simulations, we followed a standard approach to determining a good choice of  $(P, B, G)$ . We chose the values that best fit the Isaac and Walker (1988) data. In doing so we did not want to "force" the fit to be too tight. That is, we wanted the fit to be not only good but also robust so that it has some chance of serving for other experiments. To determine a loose fit, we computed the average contribution over all agents and over all simulations for all of the ten periods,  $\bar{c}_{IEL}^{10}$ , as well as the average contribution over the last 3 periods,  $\bar{c}_{IEL}^3$ . We then computed the squared deviations of each of these measurements from the same measurements computed for the Isaac and Walker (1988) experimental data. The objective was to find the minimum of the sum of these squared deviations, MSE, i.e.:

$$\text{Min} \sum_{r=1}^R \left[ \left( \bar{c}_{\text{exp}}^{10}(r) - \bar{c}_{IEL}^{10} \right)^2 (r) + \left( \bar{c}_{\text{exp}}^3(r) - \bar{c}_{IEL}^3 \right)^2 (r) \right] \tag{6}$$

where  $r$  is a particular treatment for a given  $(N, M)$  and  $R$  is a total number of treatments. For the Isaac and Walker (1988) data,  $R = 4$ . Since we want to be able to compare these results to other experiments where  $R$  will be different, we normalize the MSE and report the normalized mean squared error, NMSE which is given as:

$$\text{NMSE} = \sqrt{\frac{\text{MSE}}{2R}}$$

In our comparison of IEL with other-regarding preferences to the Isaac and Walker (1988) data, the probability distribution of  $(P, B, G) = (0.48, 22, 8)$  generates the lowest value of mean squared error equal to 0.9347. The normalized measure is given by  $\sqrt{0.9347/8} = 0.341$ . One can get an intuitive feeling for the goodness of fit by noting that the measurements, such as  $\bar{c}_{IEL}^{10}$ , used to compute the NMSE belong to  $[0, 10] = [0, w]$ . That is they are the average contributions for an endowment of 10 tokens. So a NMSE of 0.34 means that there is an average error of only 3.4% in our fit across the 4 treatments in Isaac and Walker (1988).

To provide a visualization of the high quality of the fit to the Isaac and Walker (1988) data, in Fig. 1 we present the average contribution in IELORP\* simulations for  $N = 4$ , for both values of  $M$ . We also present the time paths of the experimental sessions for the same parameter values. In comparing the IELORP\* and experimental averages, keep in mind that the experimental results are the average of 6 observations while the IELORP\* results are the average of 40 observations. It is thus not surprising that the IELORP\* curves are smoother than those from the experimental data. Fig. 2 presents the paths for  $N = 10$ .

<sup>19</sup> We will show later that this will also be a good calibration for other subject pools.



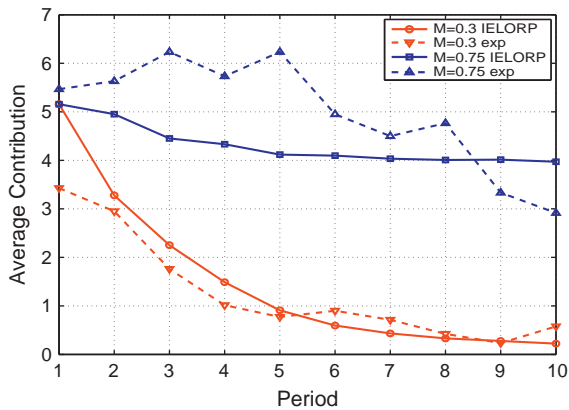


Fig. 1.  $P = 0.48, B = 22, G = 8, N = 4$ .  
Experimental data source: Isaac and Walker (1988).

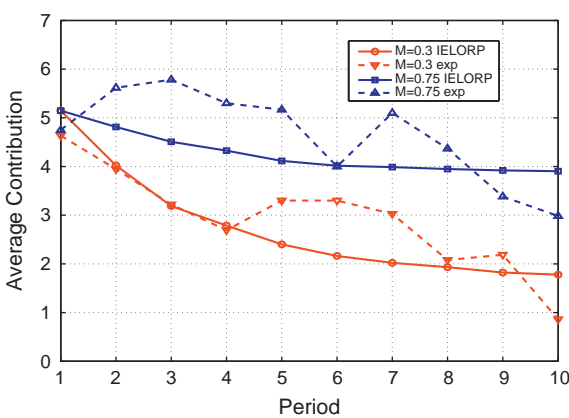


Fig. 2.  $P = 0.48, B = 22, G = 8, N = 10$ .  
Experimental data source: Isaac and Walker (1988).

For the rest of this paper we will refer to the particular version of IELORP with parameters  $(J, \mu, \sigma) = (100, 0.033, 1)$  and  $(P, B, G) = (0.48, 2, 8)$  as IELORP\*.<sup>20</sup>

### 3.1.1. Statistical analysis

Since IEL generates a Markov process on the sets  $A_i^t$ , and not on the actions,  $a_i^t$ , standard statistical techniques for testing the validity of learning models are difficult to use. We have indicated how low the NMSE is for the averages but that gives little indication of the distribution of those averages. To give a more precise idea as to how well IEL corresponds to the data we take a bootstrapping approach.<sup>21</sup> For each treatment in  $(N, M)$ , using IELORP\* we generated 10,000 simulations. We kept track for each simulation what the average group contribution was over all 10 periods and for the last 3 periods. We collected these in bins of size 1 each. This gives us a density function of  $C_{10}$ , and  $C_3$  for each treatment.

We computed Kolmogorov–Smirnov goodness of fit statistics between the simulated cumulative distribution functions and the empirical distributions generated in the IW experiments. For the average of the final 3 periods these are 0.28 for  $(N, M) = (4, 0.75)$ , 0.26 for  $(4, 0.3)$ , 0.45 for  $(10, 0.75)$ , and 0.43 for  $(10, 0.3)$ . Since there are 6 observations for each of these, they are all within a critical value of  $\alpha = 0.15$ ,  $d = 0.45$ , where  $\alpha = 1 - \text{Prob}(D_n \leq d)$  and  $D_n$  is the K–S statistic.

We also did this for the 10 period averages and got statistics of 0.21, 0.67, 0.25, and 0.58.

Table 2  
Probability of equilibrium behaviors and average contributions.

	N = 4	N = 10
	$Q_{fr}, Q_a, Q_{cc}, \bar{c}^e, \bar{c}_3$	$Q_{fr}, Q_a, Q_{cc}, \bar{c}^e, \bar{c}_3$
M = 0.3	0.81, 0.01, 0.18, 1, 4	0.56, 0.11, 0.33, 17, 17
M = 0.75	0.49, 0.37, 0.14, 43, 37	0.49, 0.38, 0.13, 44, 36

$Q_k$  = the probability of behavior k in equilibrium.

$\bar{c}^e$  = the expected average % contribution in equilibrium given  $Q$ .

$\bar{c}_3$  = the average contribution in the last 3 periods in Isaac and Walker, 1988.

### 3.2. Implications for the distribution of types

The parameters  $(P, B, G)$  determine the distribution of types  $(\beta, \gamma)$  which in turn determines the expected average equilibrium rate of contribution for a particular value of  $(N, M)$ . In Section 2.2, following Eq. (2) we saw that for any specific draw of individual parameters the equilibrium rate of contribution is  $\frac{N_a}{N_a + N_{fr}}$ . Therefore an approximation of the average equilibrium rate of contribution in equilibrium is

$\bar{c}^e = \frac{Q_a}{Q_a + Q_{fr}}$  where  $Q_a$  is the probability that an agent will act altruistically (contribute w) and  $Q_{fr}$  is the probability an agent will act as a free rider (contribute 0). The probability an agent will act as a conditional cooperator is  $Q_{cc} = 1 - Q_{fr} - Q_a$ . We can easily compute these probabilities. Let  $\Phi = \frac{1-M}{M-\frac{1}{N}}$  and  $\Psi = \frac{N-1}{NM-1}$ . Then from Eq. (2):

$$\begin{aligned}
 Q_{fr} &= \text{prob}\{\beta \leq \Phi\} = P + (1-P)(\Phi/B) \\
 Q_a &= \text{prob}\{\beta \leq \Psi\gamma + \Phi\} = (1-P)(\Phi - .5G\Psi)/B \quad \text{if } B - \Phi - \Psi G \geq 0 \\
 &= (1-P)(.5)(B - \Phi)^2 / BG\Psi \quad \text{if } B - R - \Psi G \geq 0.
 \end{aligned}$$

In Table 2 we provide the values of these probabilities for the Isaac–Walker treatments for the parameters  $(P, B, G) = (0.48, 22, 8)$ . We also include in that table, the expected average contribution for each of the four treatments based on those probabilities and the actual average contributions for the last three periods from the Isaac and Walker (1988) experiments.<sup>22</sup> IELORP\* is providing a good equilibrium prediction about the rate of contributions to which the IW88 experimental groups are converging. The average difference between the prediction of IELORP\* and the data is 4.26% of the endowment, w.

The numbers in Table 2 highlight the fact that in the ORP model, free riding and altruism are behaviors and not types. This may provide an explanation for the disparity in reports on the percentages of heterogeneous types in other experiments. For example, Kurzban and Houser (2005) report they find 63% conditional cooperators, 20% free riders, and 13% altruists. Burlando and Guala (2005) report 35% conditional cooperators, 32% free riders, 18% altruists, and 15% unknown. Fischbacher et al. (2001) find 30% free riders, 50% conditional cooperators (with a selfish bias), and 20% hump-shaped. It is interesting that they did not identify any altruists. From Table 1 and Fig. 2 of Muller et al. (2008), there are, in the second period of a 2 period situation, 6% altruists, 24% free riders, 53% conditional cooperators, and 17% hump-shaped. It is possible that some of these differences are the result of different environmental parameters  $(M, N)$  in their experiments. Whether a particular subject acts altruistically or selfishly will depend on the situation in which they find themselves—the number of participants and the marginal benefit of their choices. Higher  $M$  really brings out the altruistic behavior in an ORP agent, especially when there are few participants. Higher  $N$  also brings out altruistic behavior in an ORP agent especially when  $M$  is low. It could also be an artifact of small numbers which could lead to different draws in the parameter  $(\beta, \gamma)$ .

<sup>20</sup> Interested readers can find Matlab codes for the computational model in the folder demo.zip at [www.sfu.ca/arifovic/research.htm](http://www.sfu.ca/arifovic/research.htm).

<sup>21</sup> We thank Nat Wilcox for suggesting this direction.

<sup>22</sup> We are implicitly assuming that the average contributions in the last 3 periods are an estimate of where the group is converging to, the equilibrium of the group.

**Table 3**  
Partners vs. strangers.

Run	Partners Group 1	Partners Group 2	Partners Group 3	Partners Group 4	Partners Average %	Strangers
1	(4,0,1) 0%	(0,1,4) 100%	(3,0,2) 0%	(3,1,1) 25%	31.25%	(10,2,8) 16.67%
2	(2,3,0) 60%	(1,0,4) 0%	(4,0,1) 0%	(2,1,2) 33.3%	23.33%	(9,4,7) 30.77%
3	(1,3,1) 75%	(3,0,2) 0%	(3,1,1) 25%	(3,0,2) 0%	25.00%	(10,4,6) 28.57%
4	(2,1,2) 33.33%	(1,3,1) 75%	(1,3,1) 75%	(5,0,0) 0%	45.83%	(9,7,4) 43.75%
5	(4,1,0) 20%	(3,1,1) 25%	(3,1,1) 25%	(3,1,1) 25%	23.75%	(13,4,3) 23.53%

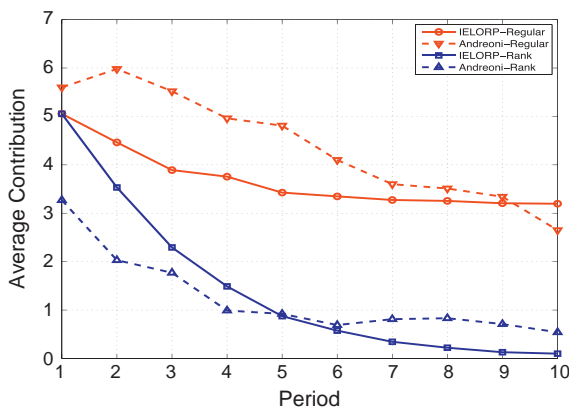
Data are: (#selfish, #altruistic, #conditional cooperators) equilibrium contribution %.

One might wonder how our distribution on types differs from that in Fehr and Schmidt (1999). There it is suggested in their Table 3 on p.843 that a discrete distribution on  $(\eta, \zeta)$  where the  $prob(\eta=0)=0.3$ ,  $prob(\eta=0.5)=0.3$ ,  $prob(\eta=1)=0.3$ ,  $prob(\eta=4)=0.1$ ,  $prob(\zeta=0)=0.3$ ,  $prob(\zeta=0.25)=0.3$  and  $prob(\zeta=0.6)=0.4$  will explain much of the data. What would these imply for the probability of each behavior? First, with  $\eta \geq 0$ , the probability of altruistic behavior is 0. In the Fehr–Schmidt model, an agent behaves altruistically iff  $\eta \leq M - 1 < 0$ . So if  $\eta \geq 0$  there can be no altruism. This is not consistent with the data since there are those subjects who contribute most or all of their endowment even in round 10. Second, for their proposed distribution on types, the probability of type does not depend on  $N$ . This is not consistent with the data for  $N=4$ . Third, it is true that for the Fehr–Schmidt distribution we should expect to see different dynamics as  $M$  varies. For  $M=0.3$  in their model an agent is selfish if and only if  $\zeta \leq 0.7$ . So the probability of selfish behavior is 1. For  $M=0.75$ , in their model, the probability of free riding is 0.6 and the probability of conditional cooperation is 0.4. But without some altruistic behavior to pull up the contributions of the potential conditional cooperators, the equilibrium is that all contribute 0. With learning, in later periods, the average rate of contribution goes to 0 no matter what  $M$  is. This is also not consistent with the data.

**4. Transferability**

How excited should one be about the results above? There are number of challenges one can make. In the next three sections we look at three. (a) Is IELORP\* transferable? That is, is it consistent with data from other experiments and other experimenters without re-calibration of the parameters? (b) Can IELORP\* explain outstanding puzzles in experimental data? (c) Is IELORP\* sensitive to the parameters chosen? That is, how robust are the results to small parametric changes in either ORP or IEL?

In this section we ask what happens if we use IELORP\* to generate simulations to compare with data from other experiments. We begin with a classic, well-know experiment of Andreoni (1995) and end



**Fig. 3.** Average contributions,  $N=5$  (total of 20 subjects)  $M=0.5$ . Experimental data source: Andreoni (1995).

with other Isaac–Walker experiments. The key finding is that without changing any of its parameters, IELORP\* fits all of these.

**4.1. Andreoni (1995): kindness or confusion?**

To check the transferability of our model to other data sets, we began with Andreoni's (1995) experimental data. His Regular treatment corresponds closely to the Isaac and Walker (1988) experimental design with two exceptions. First, he used only one treatment of  $(N, M) = (5, 0.5)$ . Second, in the Isaac–Walker, each subject was put into a group at the beginning of a session and remained in that group through the ten rounds. In Andreoni, twenty subjects were randomly rematched in each round into groups of size  $N=5$  with  $M=0.5$ . In checking our model and parameter values against his data, we modified our IEL simulations to reflect the random matching treatment.<sup>23</sup> Thus, in each simulation, we randomly select the  $(\beta, \gamma)$  for 20 IEL agents. In each period of the simulation, we rematch these agents randomly into groups of  $N=5$ . We used Andreoni's data set on average contributions per round (also 10 rounds) to do another grid search for the values of  $(P, B, G)$  that minimize the mean squared error. We found that  $(0.39, 36, 20)$  resulted in the minimum NMSE of 0.41. This is a bit off of our values of  $(0.48, 22, 8)$ . But the difference is really slight. Using our values, IEL generates a NMSE of 0.49, virtually the same as the minimal value of 0.41. In Fig. 3, we present the pattern of average contributions for Andreoni's data and for IEL simulations using our parameters  $(4.48, 22, 8)$ .

Andreoni (1995) also reports on another treatment called Rank. In this treatment, subjects are paid not on the value of  $\pi^i$ , but on the basis of where their  $\pi^i$  ranked with others. Andreoni uses a payoff of  $V^i = V^i(\pi^1, \dots, \pi^N)$  where  $V^i$  depends only on the relative rank of  $\pi^i$ . Note that  $V^i(\pi) = V^i(w - c^1, \dots, w - c^N)$ . That is, it is only the selfish part of the payoff that is important. Let us see how agents with utility of  $u^i = \pi^i + \beta\bar{\pi} - \gamma \max\{0, \bar{V} - V^i\}$  would behave in the Rank experiment. Here,  $u^i = V^i(w - c) + \beta\bar{V} - \gamma \max\{0, \bar{V} - V^i\}$ . Note that  $\bar{V}$  is known and constant over all choices of  $c$  and so can be ignored in any IEL computations. Therefore, the foregone utility for  $i$  is  $u(c|c_t) = V^i(c^i|c_t) - \gamma\{0, \bar{V} - V^i(c^i|c_t)\}$ . The analysis is easy if  $\gamma=0$ . Here  $u^i(c) > u^i(c')$  iff  $w - c^i > w - c'^i$ . The agent only cares about  $w - c^i$ , so Rank should produce IELORP\* results equivalent to those that would be generated when all types are selfish; that is when  $\beta^i = \gamma^i = 0$  for all  $i$ . But when  $\gamma > 0$  this becomes more interesting. Now  $u^i(c) = V^i(w - c) + \gamma \max\{0, \bar{V} - V^i(c)\}$ . If  $i$ 's rank payoff is higher than average, then this is just like  $\gamma=0$ . However, if  $i$ 's rank payoff is lower than average, then  $i$  will have stronger incentives to push  $c^i$  to zero. So Rank should produce IELORP\* results below the purely selfish IEL.

<sup>23</sup> As is explained in more detail below in Section 5.1, simulations with this Strangers treatment of random rematching are significantly different from the Partners treatment with no rematching. So the structure of the experiment is important in what data are produced.

As we did for Andreoni's *Regular Treatment*, in each simulation of the *Rank treatment*, we generated 20 IEL agents, and randomly matched them into groups of 5 in each period of the simulation. A grid search over different values of  $(P, B, G)$  resulted in the minimum NMSE of 0.39 for  $(0.31, 4, 12)$ . Using our parameters,  $(0.48, 22, 8)$ , to generate the data yields a NMSE of 0.41 for Andreoni's rank data. We plot average contributions (over 40 simulations) for  $(P, B, G) = (0.48, 20, 8)$  and compare it to the experimental data from Andreoni's *Rank Treatment* in Fig. 3.

So it seems that IELORP\* transfers without problems. The data generated by IEL, using exactly the same parameters that we used in the Isaac and Walker (1988) simulations, produces data similar to that of the Andreoni experiments in both the *Regular* and *Rank* treatments.

#### 4.2. Other Isaac–Walker small group experiments

At this point in the paper, it is important to recognize a problem with small sample sizes. If our model of IELORP\* is correct, then we should be very careful about small samples for at least two reasons that are not generally raised. First, consider an experiment with  $N = 4$  and  $T = 10$ . The particular outcome for one such experiment depends on the configuration of the  $(\beta, \gamma)$  draws. With 3 equilibrium behaviors (free riding, altruistic, conditional cooperation) there are 16 possible configurations ranging from all behaving altruistically to all behaving selfishly. The equilibrium average contributions for these groups will range from 100% to 0%. Of course, with the probabilities as shown in Table 2, the probability of any extreme is low. If the sample size is small, it is very possible that the observed behavior will not coincide exactly with the average predicted by the theory. This is even more true as  $N$  increases to 5 or 10. Second, even if the configuration of types were constant across all observations (a very unlikely event), if subjects experiment and learn and play somewhat randomly then any one observation may not look at all like the average contributions even if the theory is correct. For these reasons, we will ignore those experiments for which there are very few replications. And we suggest caution even if there are 6 or more replications. With that in mind, we turn to other Isaac–Walker experiments.

Isaac and Walker have conducted a number of other VCM experiments beyond those on which we have focused so far. In their seminal paper, Isaac and Walker (1988) varied experience, marginal rate of substitution, and the number in a group—a total of 8 treatments. Unfortunately, there is only one observation for each treatment. In Isaac et al. (1994), they created and ran some experiments with  $N = 40$  and  $N = 100$ . They ran a  $2 \times 2$  design with  $M = 0.75$  and  $0.3$ , and  $N = 40$  and  $100$ . There were 3 observations for each cell. But the subjects were not paid in money. Instead they were paid in grade points. They worked to justify this by running some sessions with  $N = 40$  and  $M = 0.03$  where they paid subjects in cash. But there were only 3 of these too. Again we feel there are too few observations here to work with.

As Isaac and Walker were providing experience for their subject pool for the experiments reported in Isaac and Walker (1988) experiments, they conducted six sessions with  $M = 0.3$  and  $N = 4$  and inexperienced subjects. We did compare our model to these data. The NMSE for the difference between the average contributions from these data and those from IELORP\* is 0.66. This represents a 6.6% difference between IELORP\* and the averages from these data. This compares favorably with the NMSE of 0.34 for the Isaac and Walker, 1988 data with experienced subjects. Therefore, inexperience is not something that we need to control for in our IELORP\* model. It also compares well with the NMSEs of 0.49 and 0.41 for the *Regular* and *Rank* treatments of Andreoni (1995).

### 5. Two puzzles

Can IELORP\* explain outstanding puzzles in experimental data? One way to understand whether a new theory has any benefit is to ask it to

explain some facts that previous theories have had trouble dealing with. In this section we look at two well-known puzzles: the *restart effect* and *partners–strangers* data. Using the same parameters for IELORP\* as in the previous sections, and without adding any new common knowledge or strategic components, we are able to provide explanations for both.

#### 5.1. Andreoni and Croson (2008): partners versus Strangers

In their 2008 article on partners versus strangers, Andreoni and Croson report that out of 13 studies of repeated play (Partners treatment) versus random re-matching (Strangers treatment), “four studies find more cooperation among Strangers, five find more by Partners, and four fail to find any difference at all.” (p.777). What explains this? They consider several explanations for the data but are unable to really pin things down, although they point to Palfrey and Prisbrey (1997) and Croson (1996) to suggest that the best explanation is that “Strangers exhibit more variance in their contributions.”<sup>24</sup> This of course leaves open the question: why?

We think that the explanation lies in another idea in Andreoni and Croson (2008); that there may be something about the Partners condition that makes learning easier. We think that is especially true if subjects have other-regarding preferences. If our model is correct, then subjects in an experiment session would play one of three types of equilibrium strategies: free riding, altruism, or conditional cooperation. The altruists and free riders will learn essentially the same way whether they are in the Partners treatment or the Strangers treatment. They have a dominant strategy and who they play with is irrelevant. But the conditional cooperators have a problem. How much they contribute in equilibrium depends on the proportion of types of the others in their group. If that is changing from round to round, as it does in the Strangers condition, then they are trying to learn a randomly moving target. In essence they must learn to play against the whole group of 20 (in expectation) and they only see information on 4 others per round; they have partial feedback. Conditional cooperators in the Partners treatment have it much easier. They need only learn to play against 4 others and the feedback they get is more precise. It should be no surprise that there is more variation in the Strangers data.

This explanation also implies that convergence to equilibrium will be slower for Strangers than Partners. We can see the results of this in Figs. 4 and 5. There are displayed the average contributions from 5 simulated sessions with Strangers (randomly rematched groups) and 5 simulated sessions with Partners (no re-matching). In each run, 20 individual subjects are drawn at random from the IELORP\* distribution of preferences using the parameters  $(P, B, G) = (0.48, 22, 8)$ . For ease in comparison, the draws are identical in run  $k$  of Partners and run  $k$  of Strangers for  $k = 1, \dots, 5$ . We used the Andreoni *Regular* parameters with  $N = 5$  and  $M = 0.5$  to simulate the experiments.

With the exception of one session (run 4), average contributions in the Partners treatments all decline and converge to between 30% and 25%. But in the Strangers treatments, the results are more dispersed. Run 4 is similar to run 4 of the Partners treatment. But the

<sup>24</sup> It is possible that Palfrey–Prisbrey used an incorrectly specified model to analyze their data in a way that would lead to a conclusion of “more variance” when there wasn't. Their experiment was a similar to the linear public goods experiments of others with one exception: subjects were paid  $\pi^i = p^i(w - c^i) + M \sum c^i$  where  $p^i$  was randomly assigned. The model they used assumed an ORP utility function with  $u^i = \pi^i + W^i c^i$  where  $W^i$  was a parameter to represent “warm glow.” This gives a cut point where if  $p^i > W^i + M$  then the subject gives nothing and if  $p^i < W^i + M$  the subject gives everything. Palfrey and Prisbrey used the experimental data to estimate the  $W^i$ . If our form of ORP is used, there are two cut points:  $W_1^i = \frac{NM(1+\beta)}{N+\beta}$  and  $W_2^i = \frac{NM(1+\beta)}{N+\beta+(N-1)\gamma}$ . If  $p^i > W_1^i$  then  $i$  will contribute nothing in the stage game equilibrium. If  $p^i > W_2^i$  then  $i$  will contribute everything. When  $W_1^i < p^i < W_2^i$ ,  $i$  will be a conditional cooperator. One gets the Palfrey–Prisbrey model if and only if  $W_1^i = W_2^i$ ; that is, if and only if  $\gamma^i = 0$ . So if there are conditional cooperators, then their maintained hypothesis that  $W_1^i = W_2^i$  would be incorrect.

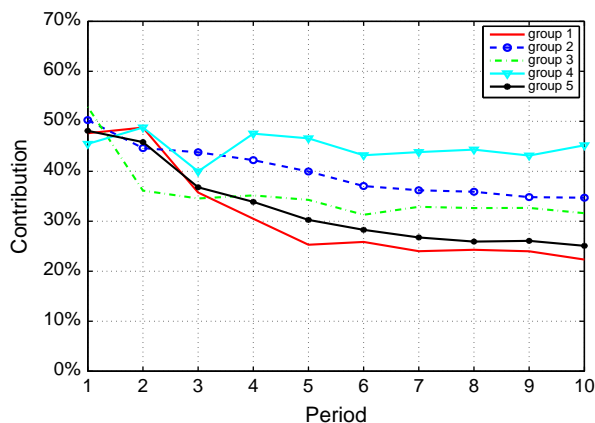


Fig. 4. Strangers-IEL-N = 5, M = 0.5.

other four end up at 35, 31, 25, and 22. If you were an experimenter who wanted to test Partners vs Strangers and ran just one of each with new subjects each time, and if subjects behave according to IELORP\*, you could get data consistent with Partners contributing more than Strangers or with Partners contributing less than Strangers or Partners contributing the same as Strangers. This is true even if you rule out a draw like run 4. These results are certainly consistent with the observations of Andreoni and Croson in the opening sentence of this section.

A closer look at the simulations can provide some more insights into the Partners and Strangers data. What data one observes is dependent not only on the characteristics of the 20 subjects drawn, but also on how they are allocated to groups. In Table 3, we provide the basic data from the 5 runs: the number of each type and the implied equilibrium % contribution rates for each group in the Partners treatment and for the total group in the Strangers treatment. The importance of the distribution of subjects to groups can be most easily seen by referring to runs 1 and 2. In run 1, the distribution causes the average equilibrium contribution rate to be 31.25% in the Partners treatment and 16.67% in the Strangers treatment, almost half as much. In run 2, the distribution causes the average equilibrium contribution rate to be 23.33% in the Partners treatment and 30.77% in the Strangers treatment, almost 50% more. What one observes also depends on the characteristics of the subjects. Consider run 4 and run 1. In run 4 there are 7 altruists and 9 selfish agents. In run 1 there are 10 selfish agents (about the same) but only 2 altruists. The rate of contribution in group 4 is obviously higher than that in group 1.

Finally, with respect to rates of convergence the simulations are mixed. In both cases groups 4 and 5 seem to converge by 10 rounds. For each of the other 3, the Partners condition seems to be closer by

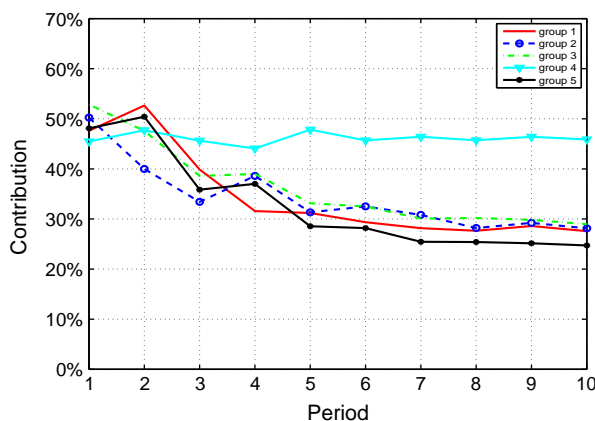


Fig. 5. Partners-IEL-N = 5, M = 0.5.

10 rounds than the Strangers. This confirms the intuition that the Strangers condition makes it harder to learn. We believe that the main reason for the difference in runs 4 and 5 and in groups 1–3 is the number of conditional cooperators. Altruists and selfish individuals have little to learn since they have a dominant strategy. It is only the conditional cooperators who need to figure out what the average contribution rate is for their particular group. The more of them there are, the harder it seems to be. We leave a more detailed theoretical analysis of this to future research.

Overall IELORP\* seems to account for many of the previously confusing features of Partners vs. Strangers data. To confirm this, a lot more experiment data are needed to overcome the randomness from the distribution of types, from the allocation of types to groups, and from decisions while learning. This also awaits future research.

5.2. Andreoni (1988): the restart effect

In his 1988 paper, Andreoni presents the results of an experiment designed to determine whether the data from repeated VCM games are the result of learning or strategic behavior. The key new finding was a restart effect. In those experiments, subjects were initially given the standard instructions for 10 rounds. At the end of those 10 rounds, however, they were given a surprise announcement by the experimenter that they would play another game of 3 rounds.<sup>25</sup> There was one configuration with Partners and one with Strangers. Andreoni had 20 subjects in his strangers configuration, divided into 4 groups of size 5, M = 0.5. His Partners configuration had only 15 subjects divided into 3 groups of size 5. He ran each configuration twice. Croson (1996) reports on a replication of Andreoni's experiment, where the session ran for 10 more rounds after the restart. Croson had 12 subjects per configuration. Both configuration divided the subjects into 3 groups of 4 players, with M = 0.5 and the length equal to 10 periods. She also ran each configuration twice. The key finding was that average contributions increased after the restart but then began to decline again. A second finding was that the effect was larger in the Partners configuration than in the Strangers configuration. It has been argued by some that this must mean that subjects are strategic.<sup>26</sup> We, however, have found an explanation within the context of our IELORP\* model where strategic elements are largely absent.

To understand the effect of the surprise announcement, it is important to recognize the role of the experimenter in the process. Without the announcement, the experiment would just be a standard 20 period experiment. Instead, with the announcement, it is two 10 period experiments with a "surprise" interjection by the experimenter between them. It is our view that such an announcement triggers some reflection and rethinking by the subjects.<sup>27</sup> Normally our models do not account for this possibility since the experimenter is viewed as something outside the model. We model the reaction by allowing the subjects, between the 10th and 11th round of the simulation, to ask "Have I been getting it right?" and to reconsider. To model this reflection and re-computation, we modify IELORP\*. At the time of the experimenter interjection between periods 10 and 11, we have the IELORP\* subjects repopulate their "possible strategy set" with random strategies, before experimentation and replication. Everything else remains the same. In particular, in period 11 subjects remember what was played in period 10. It is this simple thing that connects the first 10 periods with the second 10. It is also the only thing that differentiates the periods after the announcement from a new 10 period experiment. Finally, we

<sup>25</sup> The restart ended after 3 periods, due to budget constraints.

<sup>26</sup> See for example Ambrus and Pathak (2011) who work hard to generate the restart effect in equilibrium.

<sup>27</sup> We are agnostic about whether this is a "demand effect" where subjects infer that they have done something wrong, a signal that they are now in a "new" experiment, or something simpler.

emphasize that we have introduced no new strategic elements with this modification.<sup>28</sup>

For each of the strangers and the partners configurations, we conducted two sets of trials. One simulation, following Andreoni, used 20 agents,  $N = 5$ ,  $M = 0.5$ . The other simulation, following Croson, used 12 agents,  $N = 4$ ,  $M = 0.5$ . Each set consisted of 40 trials. We used the usual values of  $(P, B, G) = (0.48, 22, 8)$  In Fig. 6, we display the average contributions over 20 periods for both their data and the IEL simulations for the Partners configuration. In Fig. 7, we display the average contributions over 20 periods for both their data and the IEL simulations for the Strangers configuration. Before we discuss these results further, we remind the reader of the observations we made in the previous section. For the strangers configuration, the Andreoni and Croson data represent just two observations, whereas ours represents 40. The situation is a little better in the partners configuration where both the Andreoni and Croson data represent six observation. Our simulations of partners represent 120 observations. As we noted earlier, this means that one should not expect all of the curves to lie right on top of each other.

Nevertheless, there are some conclusions one can draw. First and foremost, IELORP\* is capable of generating the restart effect without sophisticated strategic assumptions. In period 11, the average contribution jumps up to higher level than the one from period 10, and that higher level is below the original starting point in period 1. Second, our simulations do not generate the conclusion of Andreoni and Croson that the effect is smaller in the strangers configuration. In order to understand what the cause of the differences might be, let us look at each configuration separately.

For the Partners configuration with no re-matching, see Fig. 6, we highlight three thoughts. One, the average initial contributions in period 1 for Andreoni and Croson are close to the average for IELORP\*. Two, the average contributions after the restart in period 11 for Andreoni and Croson are close to the average for IELORP\*. Three, to us the only disturbing difference between the Andreoni and Croson and the IELORP\* data is found in periods 6–10 and periods 17–20 where the average contributions in the experiments lie below the averages from the simulations. One possibility, since there are only six experimental observations for each of Croson and Andreoni, is that the difference is due to the random effects of a small sample size. A more reasonable conjecture, however, is that the populations that they (particularly Andreoni) drew from contained a few more types who would be selfish in these environments. This would explain the steeper drop in contributions than predicted on average by IELORP\*.

For the Strangers configuration with re-matching, see Fig. 7, we highlight three thoughts. One, as with the Partners data, the Andreoni and Croson data seem to have a few more agents behaving selfishly than IELORP\* averages would predict. Two, clearly the Croson and Andreoni restart average contribution in period 11 is less than that predicted by IELORP\*. Three, both one and two could be due to the fact that their data are really only two observations. It is entirely possible that this is due to the randomness caused by a small sample size instead of a fundamental difference in the underlying distribution of types. That is, with more observations we could observe other data which stayed at higher levels of contributions and for which the restart average in period 11 would be higher than 40%. Of course, our modification of IELORP\* could just be wrong about the reason for a restart effect. More data are clearly needed to distinguish the possibilities.

## 6. Sensitivity

Is IELORP\* sensitive to the parameters chosen? In this section, we ask; if we change a parameter value, will the contributions generated change dramatically? If the answer is yes then one might worry that

<sup>28</sup> It should also be noted that we modify the model in exactly the same way, whether it is a Partners treatment or a Strangers treatment. This does not mean the effect will be the same, just that the cause is the same.

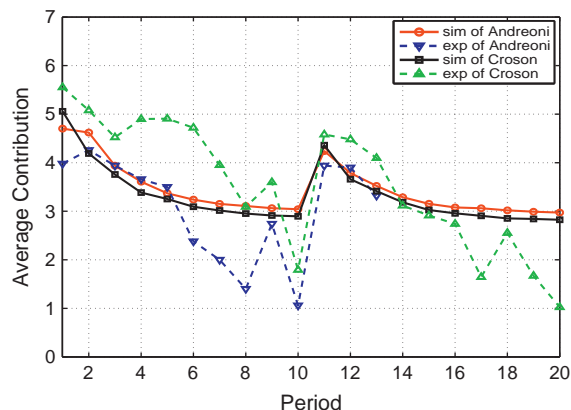


Fig. 6. Restart effect with partners—IEL compared to Andreoni (1988) and Croson (1996).

new data sets from new experimental situations might require new simulations to re-calibrate those parameters, rendering any particular results, including the ones above, as less interesting. On the other hand if there is a significant range of values of the parameters  $(J, \rho, \sigma)$  and  $(P, B, G)$  over which IELORP generates data consistent with the experimental data, then one should be a bit more excited.

There are two sets of parameters of interest. For IEL there are  $(J, \rho, \sigma)$  and for ORP there are  $(P, B, G)$ . We will take up each in turn. But before we do so, it is interesting to note that the two sets will affect the simulated behavior, the choices of  $c_i^t$ , in fundamentally different ways. The population parameters  $(P, B, G)$  are crucial in determining the type of behavior any agent will adopt. The particular parameters that agents take into a session will affect the contribution levels to which a collection of agents will converge. The utility parameters will have relatively little effect on the rate of convergence. So the distribution of the ORP parameters should affect the average of the last three contributions more than the average of all ten. The IEL parameters  $(J, \rho, \sigma)$ , on the other hand, have little to do with determining the equilibrium and very much to do with the rate and direction of learning. We would expect changes in the IEL parameters to affect the average of ten periods of contributions but not the averages over the last three.

### 6.1. Changes in ORP parameters

We consider two types of changes in the parameters of the population type distribution function: changes in the parameters  $(P, B, G)$  and changes in the form of the distribution function.

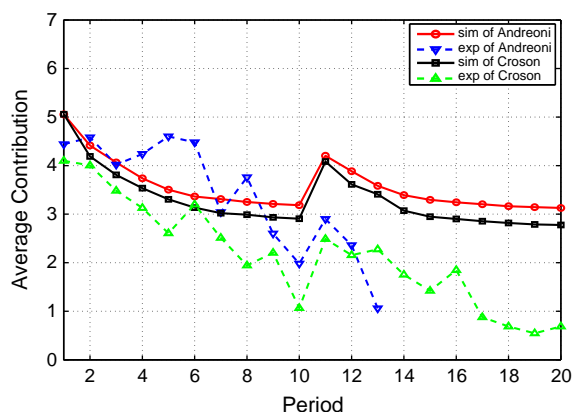


Fig. 7. Restart effect with strangers—IEL compared to Andreoni (1988) and Croson (1996).

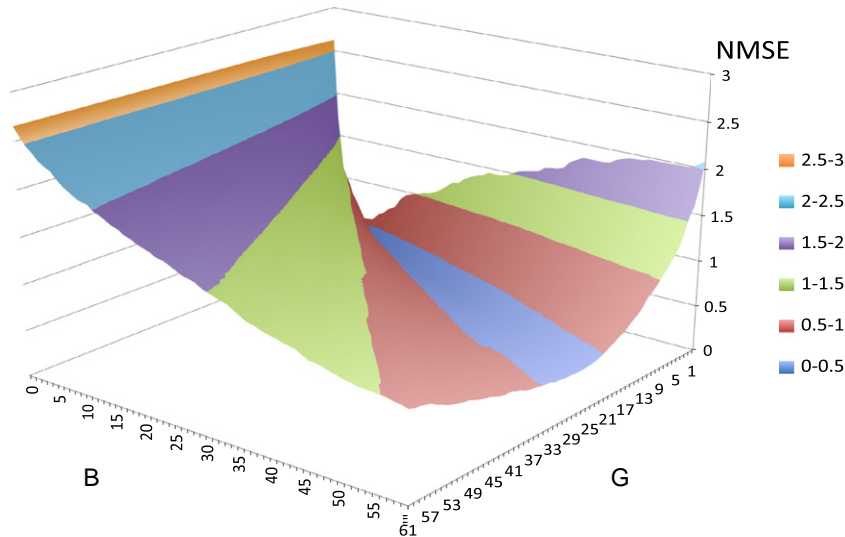


Fig. 8. NMSE landscape for P=0.48.

The results of our grid search in Section 3.1 indicate that there is a wide range of the values of B and G for which the value of NMSE is equal or below 0.5. This is illustrated in Fig. 8 which plots NMSE as function of combinations of G (given on the x-axis) and B (given on the y-axis). The extensive blue colored valley is the region where NMSE reaches its minimal values below 0.5. A good approximate relation for the B and G that are at the bottom of this valley is  $G = 0.5(B - 6)$  for  $B > 6$ . That is, if you pick any  $B^* > 6$  and we then pick  $G^* = 0.5(B - 6)$  and group simulations, drawing  $(\beta, \gamma)$  from the distribution of  $(0.48, B^*, G^*)$ , we will generate data with an NMSE of less than 0.5 from the Isaac and Walker (1988) data.

In Table 4 we list the values of average percentage of contribution,  $C^c$ , for values of  $(P, B, G) = (0.48, B, .5(B - 6))$  where B takes on a variety of values. We do this for 8 combinations of  $(N, M)$ . The key fact one should take away from this table is that, for a given  $(N, M)$ , there is very little variation in  $C^c$  as we vary B. So the equilibrium changes little as we vary B and, thus, the NMSE relative to any data set will vary little as we change B as long as G is adjusted accordingly.

To illustrate how similar the behavior generated by IELORP\* is for a wide range of population type distributions, we plot some results from 40 simulations for each in Fig. 9 with  $(N, M) = (10, 0.3)$ . There are 3 curves varying  $(B, G)$  for  $P = 0.48$ . B groups from 8 to 58 with the appropriate G. There is virtually no difference. The NMSEs are 0.34, 0.42, and 0.44. There is one curve for the Beta distribution with  $P = 0$ . The NMSE for this is 2.8, much larger than the other NMSEs. But, there is not much difference in the average rate of contribution until  $t = 7$  at which point divergence occurs. This highlights the point that the probability distribution affects the point to which contributions converge. It also indicates that in some cases behavior with an NMSE of 2.8 is not that far off of behavior with an NMSE of 0.34. Finally, to illustrate robustness with respect to P, there is one curve for  $P = 0.3$  which again lies pretty much on top of the baseline model with  $P = 0.48$ . The NMSE for this is 0.37.

Finally, one can ask what happens if we use a distribution other than the uniform distribution. We considered the Beta distribution proposed by Janssen and Ahn (2006). They jointly estimated, for each individual, EWA parameters and the parameters of Charness–Rabin other-regarding preferences. Based on these individual estimations, Janssen and Ahn propose a Beta distribution that best fits the distributions of the parameters  $\rho$  and  $\xi$  from Eq. (4). They suggest that  $\rho \sim \text{beta}(2, 0.75)$  and that  $\xi \sim 9 + 10 * \text{beta}(3, 0.5)$ . We take their suggestions for the Beta distributions of  $\rho$ , call its cumulative  $H^1(\rho)$ ,

and  $\xi$ , call its cumulative  $H^2(\xi)$ , and convert these to distributions on  $\beta$  and  $\gamma$ . The distribution on  $\beta$  is then  $F^1(\beta) = H^1\left(\frac{\beta}{1 + \beta}\right)$ . The distribution on  $\gamma$  is  $F^2(\gamma|\beta) = H^2\left(\frac{\beta - \gamma}{1 + \beta}\right)$ . Note that the distribution on  $\gamma$  will no longer be independent of  $\beta$ .<sup>29</sup> We ran 40 sets of IELORP simulations using  $F^1$  and  $F^2$ . The NMSE from the Isaac–Walker data for those data is 2.8, a much larger number than is desirable. The model with the Janssen–Ahn Beta distributions coupled with IELORP does not fit the data very well.

To try to attain a better fit of the Beta distribution, we added an atom of probability for selfishness as we did for the uniform. So when a type is to be drawn it is first decided, with probability P, whether a subject is just selfish. If yes, then  $(\beta, \gamma) = (0, 0)$ . If no, then  $(\beta, \gamma)$  is drawn from  $(F^1, F^2)$ . We tried different values<sup>30</sup> of P and obtained the best fit for  $P = 0.32$ . The value of NMSE that we get in this case is 0.47. This is fairly close to that attained by our best fit with IELORP\*. This is also plotted in Fig. 5.

We conclude that there is a wide range of probability distributions that generate behavior close to that of economic experiments, although the range is certainly not arbitrary.

### 6.2. Changes in IEL parameters

How does IELORP perform when we change the IEL parameter values? How important were our choices of  $(J, \rho, \sigma)$ ? We re-conducted the grid search for two values of J, 50 and 200, and two values of the rate of experimentation,  $\rho$ , equal to 0.02 and 0.067. We also examined what happens to the NMSE values when we changed the standard deviation in the experimentation process. We tried a 10 times smaller, and a 2.5 times larger value of  $\sigma$  along with the values of P, B, and G that gave the best fit in our baseline model. The results of these simulations are displayed in Table 5.

The results of these parameter variations are, to us, astounding. There is virtually no change in the normalized mean square error between the IELORP\* model fit to the Isaac and Walker (1988) data and

<sup>29</sup> A more faithful application of their distributions would have  $F^1(\beta) = H^1\left(\frac{N-1}{N} \frac{\beta}{1+\beta}\right)$  and  $F^2(\gamma|\beta) = H^2\left(\frac{N-1}{N} \frac{\beta-\gamma}{1+\beta}\right)$ . But we wanted to preserve the independence from N and so ignored the effect of N.

<sup>30</sup> We did not explore the effect of changing the parameters of the Beta distributions.

**Table 4**  
C<sup>c</sup> for variations in B.

		B	10	22	30	40	50	60	100
M = 0.3	N = 4		0	1	2	3	3	4	5
	N = 10		16	17	17	18	18	18	19
M = 0.75	N = 4		45	43	42	42	41	41	41
	N = 10		46	44	43	43	42	42	42

those from the variations. The values of B and G that minimize NMSE for each of the variations are very close to the values, (22,8), that minimize NMSE using our baseline model. In fact, the values of (B, G) in Table 5 satisfy  $G = 0.5(B - 6)$ , the relationship discussed in the previous section.

This means that as long as the parameters ( $J, \rho, \sigma$ ) are in the set  $[50, 200] \times [0.02, 0.67] \times [0.1, 0.25]$  we will get the same contribution behavior from the IELORP\* model.<sup>31</sup> Thus if there is heterogeneity among subjects with respect to computational or memory capacity, the parameter  $J$ , or with respect to the rate and extent of experimentation, the parameters ( $\rho, \sigma$ ), then as long as that heterogeneity is within the bounds above, it should have little effect on the closeness of the fit to the data.

The insensitivity of IEL to the specific values of ( $J, \rho, \sigma$ ), as long as they are in a reasonable range, is not unique to VCM experiments. We have used IEL (with standard selfish preferences) in other repeated games. Notably we have used it to study call markets (in Arifovic and Ledyard (2007)) and to study Groves–Ledyard mechanisms for public good allocations (in Arifovic and Ledyard (2004, 2011)). The parameters for IEL that provided good comparisons to the data in these papers are essentially the same as those used in this paper. For call markets (5 buyers and 5 sellers with unit demands or supplies), the parameters were ( $J, \rho, \sigma$ ) = (100, 0.033, 0.5).<sup>32</sup> For the GL mechanisms (5 participants choosing actions in  $[-4, 6]$ ), the best parameters were (500, 0.033, 1) but (200, 0.033, 1) yielded results similar<sup>33</sup> to those for  $J = 500$ .

While we explore this sensitivity, or lack of it, at some depth in Arifovic and Ledyard (2011), perhaps a few comments are in order here. What we have found over and over again across different environments is that there is a type of diminishing returns to increases in  $J$ . That is, if  $J$  is really small (say 10), it takes a long time to learn. Then as  $J$  increases improvements occur, up to a point. At some point having more options under consideration provides no benefits to the agent. The exact point at which this occurs depends on the game in which they are involved. For example, if there were 50 simultaneous call markets it would take a much larger value of  $J$  than what is needed for the VCM experiment.

The parameters  $\rho$  and  $\sigma$  affect the rate of experimentation. If they are very large, then a lot of noise can be injected into the learning process. In that case, observed choices have a significantly increased variance and the learning process slows down. Small values of  $\rho$  and  $\sigma$  are not so much of a problem as long as  $J$  is big enough to probabilistically cover the set of actions in a reasonable manner. In that case, experimentation, which is designed to add strategies which can be equilibrium strategies, is not really needed to get learning close to the Nash Equilibrium. So if  $J$  is big enough then there is a wide range of  $\rho$  and  $\sigma$  such that experimentation is good enough and not noisy.<sup>34</sup>

<sup>31</sup> We did not explore the limits to which these sets can be pushed.  
<sup>32</sup> The range of possible actions was  $[0, 2]$  so normalizing gives  $\sigma = 0.2$ .  
<sup>33</sup> We refer the reader to that paper for a more precise understanding of the nature of the similarity.  
<sup>34</sup> This would not be true if we did replication before experimentation. Since replication follows experimentation in IEL, really bad ideas generated by experimentation are eliminated with high probability by replication and therefore are never used. The order of computation mutes the impact of changes in  $\rho$  and  $\sigma$ .

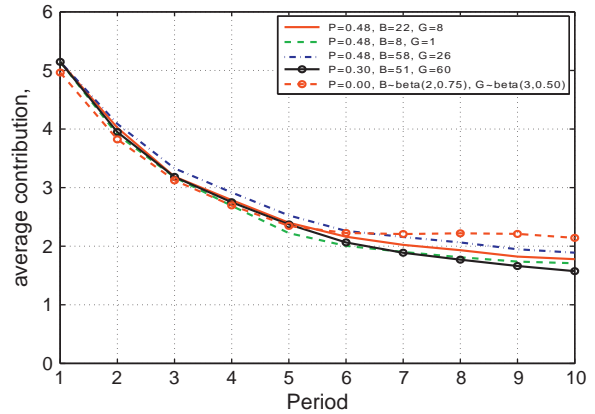


Fig. 9. Average contributions for different parameter sets using IELORP.

So as long as  $J$  is big enough and  $\rho$  and  $\sigma$  do not inject too much noise through the experimentation process, the type and rate of learning will be reasonably insensitive to their precise values.

**7. Conclusions**

Learning alone does not explain the VCM data. Other-regarding preferences alone do not explain the VCM data. But combining them in an appropriate way does the job. We have merged our learning model, IEL, with a modification of standard heterogeneous other-regarding preferences, ORP. The resulting model, IELORP\* is parsimonious and generates behavior similar to that in many VCM experiments.

In particular,

- IELORP\* is consistent with the five well-known stylized qualitative facts of VCM experiments: average contributions decline with repetition, there is variation in individual contribution rates, contributions increase with  $M$  and  $N$ , and there is a restart effect.
- IELORP\* is able to explain some puzzling features of data from Partner and Strangers experiments.
- IELORP\* generates average data that is quantitatively similar to the VCM data of various experiments and experimenters. Using a normalized mean squared error measure (NMSE), we find that the average contributions generated by IELORP\* differ from those in these experiments by only 3.4% to 6.6%.
- The NMSE measurements change very little with modest changes in the parameters of IEL, the learning model, and the distribution of the parameters of ORP, the other-regarding preferences model.

IELORP\* is a robust and parsimonious explanation for most behavior in VCM experiments.

*Some final thoughts.* Our study seems to provide evidence for a learning hypothesis and against a strategies hypothesis. Using a model grounded in other-regarding preferences and retrospective learning, without any common knowledge of rationality or information, we are able to generate conditional cooperation and declining rates of contribution consistent with the experimental evidence. We can explain the

**Table 5**  
Variations in ( $J, \mu, \sigma$ ).

IEL parameters	B	G	NMSE
IELORP*	22	8	0.34
$J = 50$	16	5	0.35
$J = 200$	23	8	0.35
$\rho = 0.02$	20	7	0.34
$\rho = 0.067$	23	8	0.34
$\sigma = 0.1$	22	8	0.345
$\sigma = 2.5$	22	8	0.345

restart effect and the differences between Strangers and Partners without resorting to sophisticated strategic considerations.

In our model, conditional cooperation is a strategy and not a pre-conceived belief or a hard wired characteristic. It is simply the behavior that emerges from the way those with other-regarding preferences play the game they are faced with in the laboratory. This also can explain why some subjects can be a conditional cooperator in one experimental session and then turn around and be a non-contributor in another. Such behavior would be consistent with our model if the marginal rate of substitution were lower in the second session.

One feature of our model, that is very important for its ability to conform to experimental data, is the possibility of altruistic behavior from subjects whose local marginal return from the payoff to the whole group outweighs both their selfish interests and their envy of others. If one's theory has no room for social preferences, then the only way to get conditional cooperation is to either hard-wire it or to create conditions such that it arises out of the strategic behavior of selfish subjects. In that case, observing conditional cooperation might lead one to believe subjects are strategic. But if, as in our model, there is room for a social preference, then conditional cooperation can easily arise even if subjects are not strategic. This highlights the importance of theory for interpreting experimental data.

Muller et al. (2008) have a very interesting paper in which they try to determine whether subjects are strategic or learning in a 2 period VCM experiment. They conclude that the strategic hypothesis is more likely than the learning hypothesis because the decline in contributions from period 1 to period 2 is larger than the decline in contributions across games. But the first decline could be from learning what type you are playing with and how to react to them while the second decline could be from learning how to play the game, independent of what type you are playing. If learning to play the game is reasonably easy, as it would be in a two period model, then one might expect little decline across games. The larger decline between periods could just be conditional cooperators learning to react to selfish types. Again, what one's theory is will really guide one's interpretation of the data.

Finally, if one is still skeptical about our model and its lack of strategic behavior, one could turn our ORP model into one with asymmetric information and apply Bayesian game theory to it. Using a model such as that in Kreps et al. (1982), it would not be hard to construct equilibria in which selfish individuals mimic conditional cooperators and in which conditional cooperators mimic altruists for some number of periods but then return to type near the end. Of course there would be many reputation equilibria in this model and that would dampen its ability to predict anything as experimental conditions changed. The only restriction from the theory would be that dominated strategies are not played.<sup>35</sup>

*Open issues.* There are some open questions that remain to be dealt with in future research.

One question involves the linear nature of the ORP model. It may be that the individual altruism parameter should depend on  $N$ . As we have modeled it, each agent cares only about the average profit of all agents and not the total. It may be, however, that agents care about the total payoff but with diminishing marginal utility as  $N$  increases. One would need a lot of data to tease out the precise relationship.

Another question involves the transferability of the IELORP\* model across widely varying mechanisms and environments. In IELORP\*, it is assumed that subjects come to the lab with their other-regarding preferences built in. That is, it is assumed that nothing in the experiment triggers the altruistic or conditional cooperative behavior other than the parameters of the game. If this is true then the IELORP\* model should work<sup>36</sup> in environments in which economists typically don't consider other-regarding behavior. For example, IELORP\* should work

in markets. If individuals really do have other-regarding preferences, then the fact that we don't seem to need those preferences to successfully model behavior in, say, continuous markets should mean that there is no observable difference in the behavior of those with and those without preferences over altruism and envy. It is an open theoretical question whether this is true or not. For example, Kucuksenel (2008) shows that in bi-lateral trading with asymmetric information, the existence of altruism leads to increased trading and efficiency. Perhaps that explains why some bargaining data violate individual incentive compatibility constraints.<sup>37</sup>

There are some features of experimental designs that IEL is currently insensitive to.<sup>38</sup> (i) There is no provision for communication although it is well known that can significantly increase contributions. (ii) There is no provision for experience although it did not appear that experience mattered in the Isaac–Walker experiments. (iii) In IELORP\* we treated all subjects the same with respect to IEL. That is, we made no provision for subject pool differences in computational or strategic skills that could affect the parameters  $J$  and  $\mu$ . One reason this was not necessary was the relative insensitivity of the performance of IELORP in VCMs to the values of those parameters. But in more complex experiments this could be very important.

Perhaps, as conjectured in Andreoni and Croson (2008, footnote 6) "reputation effects do matter, but it appears that these effects must be learned." IEL could be improved by having the agents learn more sophisticated strategies. Now they only learn a response that is naive but reasonably good. Instead, they could learn 2-period or 3-period strategies. Or, even more, they could learn to play as automata. Expanding the space of the set of retained strategies  $A$  could lead in this direction. This could be especially important for coordination games such as Battle of Sexes.

Even without this future research to improve the model, IELORP\* has proven to be a parsimonious, robust model of behavior that requires little calibration to generate observations similar to those of experimental subjects.

## Appendix A. The basics

### Voluntary Contributions Mechanism (VCM)

$$\pi^i = p^i(w^i - c^i) + y$$

$$y = M \sum_{i=1}^N c^i$$

$$c^i \in C^i = [0, w^i]$$

### Other-regarding Preferences (ORP)

$$u^i(c) = \pi^i + \beta^i \bar{\pi} - \gamma \max\{0, \bar{\pi} - \pi^i\}$$

$(\beta, \gamma) \sim F(\cdot, \cdot)$  where  $F(0,0) = P$  and otherwise  $F(\beta, \gamma) = U(0,B) \times U(0,G)$

Individual Evolutionary Learning (IEL)

$A \subset X, \psi$  is probability on  $A$ , forgone utility is  $v^i(a_t^i | I^i(a_{t-1}))$

Parameters are  $\alpha$

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<sup>35</sup> See Ledyard (1986) for the theory behind this statement.

<sup>36</sup> By work we mean it should generate behavior similar to that in the experiments without significant calibration or changes in the model parameters.

<sup>37</sup> See, for example, Valleya et al. (2002).

<sup>38</sup> See Zelmer (2003) for a list of features that seem to be important from the experimental evidence.



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