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PUBLIC GOODS: A SURVEY OF EXPERIMENTAL RESEARCH

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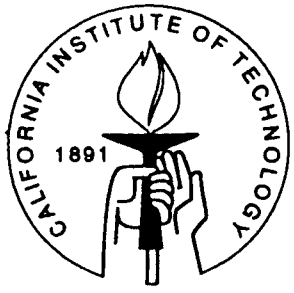

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Public Goods: A Survey of Experimental Research

John O. Ledyard*

Environments with public goods are a wonderful playground for those interested in delicate experimental problems, serious theoretical challenges, and difficult mechanism design issues. In this chapter I will look at one small but fundamental part of the rapidly expanding experimental research. In Section 1, I describe a very simple public good experiment – what it is, what some theories predict, what usually happens, and why we should care – and then provide a methodological and theoretical background for the rest of the chapter. In Section 2, I look at the fundamental question: are people selfish or cooperative in volunteering to contribute to public good production? We look at five important early experiments that have laid the foundations for much that has followed. In Section 3, I look at the range of experimental research which tries to identify and study those factors which increase cooperation. In order to help those new to experimental work I have tried to focus on specific experimental designs in Section 2 and on general results and knowledge in Section 3. The reader will find that the public goods environment is a very sensitive one with much that can affect outcomes but are difficult to control. The many factors interact with each other in unknown ways. Nothing is known for sure. Environments with public goods present a serious challenge even to skilled experimentalists and many opportunities for imaginative work.

1 Introduction

Some of the most fundamental questions about the organization of society center around issues raised by the presence of public goods. Can markets provide optimal allocations

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$1.50 to the others at no cost to them. From the point of view of this theory, individual self interest is at odds with group interest.

Another theory, which I will call the sociologic-psychologic prediction, is that each subject will contribute something. Although it is hard to find precise statements, it is sometimes claimed that altruism, social norms or group identification will lead each to contribute $5, the group optimal outcome. From the point of view of this theory, there is no conflict between individual and group interests.

What does Happen in a Public Goods Experiment? Examination of the data reveals that neither theory is right. In many cases, some contribute $0, some contribute $5 and some choose a middle course and contribute something less than $5.. Generally, total contributions can be expected to lie between $8 and $12, or 40% to 60% of the group optimum. The statement in Dawes and Thaler (1988) is "It is certainly true that there is a 'free rider problem' ... On the other hand, the strong free rider prediction is clearly wrong." This lack of precision is disconcerting. They seem to claim that a full range of behavior exists from fully selfish to fully altruistic. If so, outcomes in public goods environments can be almost anything depending on which subjects walk into the room and we can learn no more from further experiments. More likely, the imprecision of results is due to the fact that we have simply not yet achieved sufficient control in our public goods experiments to be able to identify what is really happening. It is only recently that careful experimental work has begun to uncover how changes in payoff parameters and in institutional features can change the amounts contributed for the production of public goods. Being able to change amounts contributed by changing treatments means some measure of control can be achieved. We are thus beginning to understand behavior through better control and a growing accumulation of evidence.

Why should We Care about Public Goods Experiments? Both economists and sociologists recognize that the desired outcome is for all to contribute $5. The experimental evidence suggests that voluntary contributions will not produce that desired outcome. Economic theory suggests[1] that it may be possible to change the institutions by which group choices are made in a way that causes the outcome to be closer to the group optimum. To know how to do that, however, requires anticipating how individual choices will change as the institutions change. Since both the economic/game-theoretic and socio-psychologic theoretical predictions are wrong, we need to discover more about behavior not only in the context of voluntary contributions but also in the presence of many institutional designs. Experiments are the only way to do so.

[1]See, for example, Groves and Ledyard (1977) or Ledyard and Palfrey (1992).

mistake but the rest of the group will not be too severely affected. A buyer may take advantage of a seller's error but the group still achieves near 100 percent efficiencies. Subtleties in behavior are difficult to identify and measure. In public good environments this "averaging" or "smoothing" phenomenon can not happen. A misstep by one is felt by all and can not be easily corrected. Subtleties in behavior are not only identifiable and measurable, they are endemic. Public goods and dilemma experiments appear to be the simplest environment within which to uncover variations in behavior in groups.[4]

Of course the sensitivity of the experimental medium is a double-edged sword. Control is made more difficult. Let me illustrate what I mean. When I was taking freshman physics, I was required to perform a sequence of rather dull laboratory exercises (which may be one reason I became an economic theorist). One standard experiment involved rolling a steel ball down a ramp with a ski jump at the end. The trajectory followed by the ball was to be filmed, using a strobe camera, so we could plot the parabolic arc of the ball and confirm that Newton's Laws were indeed consistent with experimental evidence. In an effort to enliven the proceedings, my lab partner and I substituted a ping-pong ball we had painted silver and, during its trajectory, we gently blew on it. The resulting experimental evidence captured on film, that Newton's Laws appeared to be rejected, was indisputable. Nevertheless, the lab instructor rejected the data as inconsistent with the theory. More correctly, he did not believe they were replicable with the original equipment. Ping-pong balls can allow the experimenter to display effects hidden by the insensitivity of metal balls; but ping-pong balls also allow unintended and uncontrolled intrusions to contaminate and mislead.[5]

Public goods and dilemma experiments are like using ping-pong balls; sensitive enough to be really informative but only with adequate control. For example, the experiment we described in Section 1.1 is neither particularly elegant nor carefully controlled. Even so, at least twelve major choices have been made in creating this design: (1) the number, (2) gender and (3) education of the subjects, (4) whether they are face to face or acting through computer terminals or in isolated rooms, (5) how much endowment to give to each and in what form (cash, tokens, promises, ...), (6) whether discussion is allowed and in what form, (7) whether contributions are private or public, (8) by how much to increase the total contributions, (9) how to divide up the larger pie (for example, in proportion to contribution or to number), (10) whether or when to announce the results, (11) whether to pay subjects publicly or privately and finally (12) whether to run the procedure once or, say, 10 times. Each of these choices represents a potential treatment or control. Each treatment has been shown by at least one experimenter to have a significant effect on the

[4]I emphasize groups here since single person decision experiments lack the ability to examine complicated feedback effects from interpersonal interactions.

[5]Using steel balls allows control but is not very illuminating.

(who knows what and to what extent might that be common knowledge), the technical details and possibilities for production, and so forth. Also included in the environment is a description of the range of possible outcomes of interest to agents.

Outcomes are what the furor is all about. An outcome describes the final distribution of resources and payoffs. How each individual feels about the outcome will depend on the particular environment since an individual's preferences for outcomes are part of the description of an environment. Similarly whether a particular outcome might be good for the group will depend on the details of the environment.

A *performance criterion* determines, for each environment, a ranking over outcomes. The idea is that in each environment the best outcome is the one which is ranked highest by the performance criterion. A standard performance criterion used in experimental work is a cost/benefit measure[11] which computes the sum of payoffs received as a percent of the maximum attainable. From a mechanism design point of view, if someone knew all the details of the environment (and were benevolent) we could simply ask them to announce the best outcome for that environment. One problem that might arise would be the difficulty in communicating all relevant details and the complexities in computing it. But one of the main contributions of modern economics is the recognition that information about the environment is dispersed and that individuals may have incentives not to provide the requested information. Further, even if the information is correctly known, self-interested agents may be unwilling to follow the suggested actions. Enforcement is, thus, another possible problem. We cannot readily rely on beneficient omniscience.

Instead, *institutions* arise to aggregate information and coordinate activities. An institution specifies who should communicate with whom and how, as well as who should take various actions and when. An example of a very simple institution designed to deal with public good production is the *Voluntary Contributions Mechanism* (without communication) in which each individual is told to contribute an amount of a private good privately and without any information about what others are doing, as in Section 1.1. The level of public good provided then equals that producible with the total private good contributed. The outcome describes the amount of public good produced and the amount of each contribution. Given a set of individuals, their preferences and their endowments, the outcome we observe is the result of both the mechanism rules and the choices made by the agents. Another more complicated institution is the modified *Lindahl mechanism* in which all agents write down a schedule of their willingness-to-pay (in private good) for various amounts of a public good. The level of public good is chosen to maximize the sum of the willingness-to-pay minus the production cost. Each individual is required to contribute (pay) an amount equal to their *marginal* willingness to pay (for that amount of the public good) times the amount of the public good. The outcome describes the amount of the public good produced and the amount of each contribution.

[11]This is sometimes incorrectly identified as efficiency or Pareto-optimality in environments with income effects.

does a given institution $(M,\ g)$ perform and does it perform optimally over a range of environments; that is, what is $\mu[e,(M,g)]$ and does $\mu(e,(M,g))\epsilon P(e)$ for all $e\epsilon E$? Examples of this type of question are: do markets efficiently allocate resources in private goods economies, and how efficient is the allocation of resources in a public goods environment if we rely on voluntary contributions? (2) Is μ a good theory; that is, do we observe $\mu(e,(M,g))$ as we vary both e and (M,g)? Examples of these types of questions are: do buyers in a first-price sealed bid auction follow Bayes-Nash strategies, and are agents in a public goods situation selfish or altruistic? (3) Can we design an optimal mechanism for a class of environments; that is, given (E,P) can we find (M,g) such that $\mu(e,(M,g)) = P(e)$ for all $e\epsilon E$? Examples of this type of question are: how can we fix up problems caused by market failure such as air pollution, how should we organize a firm, and how should we make decisions about public goods so that desirable outcomes occur? If we can simultaneously observe the details of the environment, e, the mechanism, (M,g), and the outcome for a wide variety of environments and mechanisms, we have a chance to answer these questions without making arbitrary assumptions about behavior. Experiments provide the opportunity.

1.4 The Range of Public Goods Environments

The range of experiments which have a public goods structure is more extensive than most realize. To see why, let me describe some very simple environments with public goods. There are two goods, one private and one public, and N individuals. Each individual $i = 1,..,N$ is endowed with some amount of the private good, z_i. The public good is produced from the private according to the production function $y = g(t)$ where t is the amount of private good used to produce y. An outcome is a level of public good, y, and an allocation of the private good for each agent $x^1,...,x^N$. Each agent values outcomes according to the utility function[13] $U^i(x^i,y)$. Feasible outcomes are $a = (y,x^1,...,x^N)$ such that $y = g\left[\Sigma_{i=1}^N(z^i - x^i)\right]$. We will call $t^i = z^i - x^i$ the amount of i's payment for the public good and occasionally restrict the range of possible t^i. For example, sometimes it is required that $t^i \in [0,z^i]$, the endowment is divisible but no one can contribute more than z^i, nor can they repeat compensation, and sometimes it is required that $t^i \in \{0,z^i\}$, either z^i is contributed or nothing is contributed. We can summarize the environment as $e =< g,U^1,\ldots,U^N,z^1,\ldots,z^N >$.

Virtually any public good or social dilemma experimental environment is a special case of e in which specific forms for $(g,U^1,...,U^N)$ and specific values for $z^1,\ldots,z^N$ are

[13]This assumes i is "selfish". We will see later why one might want to relax this assumption. In fact, we will need to go further and distinguish the payoff to subjects, say $p^i(x^i,y)$, from the utility they get, $V^i = V^i(p^i,\beta^i)$ where β^i may be a collection of variables which are difficult to observe or control or β^i may include the payoffs to others. If we knew β^i then $U^i(x^i,y) = V^i(p^i(x^i,y),\beta^i)$.

Another example arises in a totally different context. In Cournot Oligopoly models, (see, e.g., Chapter (Holt)) firms choose quantities q_i, the market price which depends on the total amount brought to market is $P(\Sigma q_i)$, and firms are paid $\pi_i = P(\Sigma q_i)q_i - C^i(q_i)$ where $C^i(\cdot)$ is i's cost function. Let $g(t) = P(t)$ and $U^i(x_i, y) = x^i y - C^i(x^i)$ to see why this is a public good environment.

I have listed many of the examples I am aware of[18] and the appropriate references in Appendix A. One may think it is stretching a bit to include all of these as public goods environments, but the advantage gained by recognizing that these are all the same structure is that it brings more experimental data to bear on the really difficult question: what is behavior in the presence of public goods?

1.5 What Is And Is Not To Be Surveyed

The contents of a complete survey on public goods experiments would include material from four main categories: (1) experiments with voluntary contributions mechanisms over a wide range of environments, (2) experiments with a wide range of mechanisms over a limited class of economic environments, (3) experiments with mechanisms in political environments, and (4) experiments with applications or policy problems as the focus.

Category (1) includes work by sociologists, social psychologists, political scientists, and economists intended to isolate fundamental aspects of group behavior when voluntary contributions are socially desirable but individually bad. In this paper we will concentrate on this category of work.[19]

Category (2) includes work primarily by economists aimed at identifying those aspects of mechanisms which might lead to socially optimal outcomes even if basic individual incentives operate to foil such goals. Much of this work is motivated by the theoretical findings of Hurwicz (1972) and others.[20] A good example of early work in this area is found in Smith (1979a, 1979b, 1980). A follow-up study to Smith's research can be found in Ferejohn, Forsythe, Noll, and Palfrey (1982). An example of more recent work, is found in Banks, Plott, and Porter (1988). Work from psychology would include Shepperd (1993).

Research in Category (3) has been predominantly generated, as one might expect, by political scientists. In political environments, no compensation is available to ease the group decision making process. As opposed to economic environments in which transfers of the private good from winners can be used to compensate losers, in political environments there is more of a flavor of multilateral bargaining. A classic example of

[18]See Schram and Sonnemans (1992) for another involving voter turnout.

[19]I will, however, not survey two-person games.

[20]For a recent survey of the theoretical literature see Groves and Ledyard (1987).

2 Are People Selfish or Cooperative?

Research on the voluntary provision of public goods must come to grips with this simple but still unanswered question about the fundamental nature of humankind. The debate has been long-standing with much heat and little light.[24] Economists and game-theorists argue that the hypothesis of selfish behavior is the only viable one as an organizing principle yet they also contribute to public television and vote in elections. Sociologists and political scientists argue that societies are naturally cooperative through the evolution of social norms or altruism. Preconceived notions bordering on the theological have sometimes been rejected by data. But those who are reluctant to part with cherished theories have in turn rejected the data. Disciplinary boundaries have been drawn, breached, and redrawn. It is into this fray that experimentalists have come, trying to generate light where previously there was little.

Although many have contributed to the development of our knowledge, the systematic experimental effort of three research groups has been fundamental. Marwell in Sociology at Wisconsin,[25] Dawes in Psychology at Oregon and then at Carnegie-Mellon University and Orbell in Political Science at Oregon,[26] and Isaac and Walker in Economics at Arizona and Indiana[27] have all carried out sustained efforts to understand whether and why cooperation might occur in public goods problems. Many of these still continue the study. The result of this effort and the sometimes heated interaction has been just what one might hope for; a slowly emerging consensus, which would have been impossible without carefully controlled experiments. Let us see how this has happened by trying to discover what we know now and why.

A reasonable reading of the literature[28] on voluntary contribution mechanisms and social dilemmas would probably lead one to conclude that the major findings to date are:

1. In one-shot trials and in the initial stages of finitely repeated trials, subjects gen-

[24]For an example of the often silly rhetoric of the debate see Mansbridge (1990).

[25]Work from this group includes Marwell and Ames (1979), Marwell and Ames (1980), Alfano and Marwell (1980), Marwell and Ames (1981), and Marwell (1982).

[26]Work from this group includes Dawes, McTavish, and Shaklee (1977), Dawes (1980), Orbell and Dawes (1981), Dawes and Orbell (1982), van de Kragt, Orbell, and Dawes (1983), Dawes, Orbell, and van de Kragt (1985) Orbell, van de Kragt, and Dawes (1988), Dawes, Orbell, Simmons, and van de Kragt (1986), Dawes, van de Kragt, Orbell (1987), Orbell, Dawes, and van de Kragt (1990), Orbell and Dawes (1991).

[27]Work from this group includes Isaac and Walker (1983), Isaac, Walker, and Thomas (1984), Isaac, McCue, and Plott (1985), Isaac and Walker (1987), Isaac, Schmidtz, and Walker (1988), Isaac and Walker (1988a), Isaac and Walker (1988b), Fisher, Isaac, Schatzberg, and Walker (1988), Isaac and Walker (1989), Isaac and Walker (1991), Isaac, Walker, and Williams (1990), Walker, Gardner, and Ostrom (1990).

[28]Andreoni (1988b), p. 291. See also Isaac and Walker (1987), Mansbridge (1990), p. 17, and Dawes and Thaler (1988), p. 189, for examples of these claims.

was that "the well-known risk for misrepresentation of preferences in this context may have been exaggerated" and people may be willing to contribute to the public good even if their own self-interest runs counter. What did Bohm do and was his conclusion correct?

2.1.1 Procedures

Let me first describe his experimental procedures and then explain why his study raised more questions than it answered. In his own words:

> The test was carried out by the Research Department of the Swedish Radio-TV broadcasting company (SR) in November, 1969. A random sample of 605 persons was drawn from the age group 20 to 70 of the population of Stockholm. They were asked to come to the premises of the broadcasting company to answer some questions about TV programs and were promised a fee of Kr. 50 ($10) for a one-hour "interview." Normally, some 35-50% show up in tests of this kind. (Bohm (1972) p. 118.[31])

After dividing the sample,

> The persons in each subgroup were placed into a room with two TV-sets and were, for allegedly "practical reasons," immediately given the fees promised them in four ten-Crown bills, one five-Crown bill and small change to make Kr.50. The administrator gave an oral presentation of the test which involved a half-hour program by Hasse Alfredsson and Tage Danielsson,[32] not yet shown to the public. The subjects were given the impression that there were many groups of the same size simultaneously being asked the same questions in other rooms elsewhere in the broadcasting company. The responses, given in writing by the persons in each subgroup, were taken away and said to be added to the statements from other groups. ...The main part of the instructions given to groups I to V was as follows: Try to estimate in money terms how much you find it worth at a maximum to watch this half-hour program in this room in a little while, i.e. what is the largest sum you are willing to pay to watch it. If the sum of the stated amounts of all the participants covers the costs (Kr. 500) of showing the program on closed-circuit TV, the program will be shown; and you will have to pay

[31] I would like to thank Elsevier Science Publishers for permission to quote from this report.
[32] Well-known Swedish comedians.

Table 1: Amounts stated at instructions I-VI: 2.

Kr.	I	II	III	IV	V	VI:1	VI:2	
0-0.50				1	1	2	5	
0.60-2.5	2	2		4	3	4	4	
2.60-4.50	4	5	2	1	4	4		
4.60-6.50	8	6	15	13	8	10	10	55 out of 70 stating Kr. 5
6.60-8.50	4	3	2	6	7	3	3	
8.60-10.50	1	7	9	4	8	13	12	All 54 stating Kr. 10
10.60-12.50		1		1	1	3	1	
12.60-17.50	3	1		6	3	11	12	35 out of 36 stating Kr. 15
17.60-22.50		3		1	1	1	4	All 10 stating Kr. 20
22.60-27.50		1	1		3	2	2	8 out of 9 stating Kr. 25
27.60-32.50	1						1	Both stating Kr. 30
50						1		
Number	23	29	29	37	39	54	54	
Mean	7.61	8.84	7.29	7.73	8.78	10.19	10.33	
Standard Deviation	6.11	5.84	4.11	4.68	6.24	7.79	6.84	
Median	5	7	5	6.5	7	10	10	

> ...it could pay for you to give an understatement of your maximum willingness to pay. But, if *all* or *many* of you behave in this way, the sum won't reach Kr. 500 and the program won't be shown to you. (p. 128)

It is well known now that subjects may actually be trying to do what they think the experimentalist thinks they should be doing. Even subtle cues in the instructions can cause subjects' decisions to vary. Strong moral imperatives such as those used by Bohm are equivalent to blowing on ping-pong balls. There may be economic principles involved but we will never find them this way. We might, however, find out whether such mechanisms can increase contributions. I will take up a discussion about the role of moral suasion in Section 3.5.

Bohm's imaginative study was, for its time, a major advance in the attempt to identify the extent of voluntary behavior in the presence of public goods. Although he tentatively concluded that that misrepresentation of preferences was less a problem than believed by economists, his experiment was seriously flawed in at least three ways. As a result, the data were not convincing and he was forced to conclude correctly that "the test would seem to encourage further work in the field of experimental economics." The question of cooperative vs. selfish behavior remained open.

2.2 Dawes et al.: Social Dilemmas

While economists were struggling to get their experiments under control, social psychologists were independently studying a phenomenon which, I would argue, is a special case of public goods; social dilemmas. One of the best and most persistent groups has included Robyn Dawes and John Orbell. Let us look at Dawes, McTavish, and Shaklee (1977) for an example of this type of work that avoids many of the flaws of Bohm.

2.2.1 Procedures

The experiment is simple.[35] Eight person groups were created although sometimes less showed up. A total of 284 subjects were used in 40 groups. Each individual in each group marked an X or an O on a card in private. They were told[36]

> If you choose an O, you will earn \$2.50 minus a \$1.50 fine for every person who chooses X. If you choose X, you will earn \$2.50 plus \$9.50 minus \$1.50

[35]Simplicity is a good feature of experiments. You are more likely to understand what you have learned.

[36]The subjects were also asked to indicate beliefs about others' choices. We will comment on this aspect of their experiments later in Section 3.4.2.

earnings). One member from each friendship group was sent to each of the four communication conditions. Two went to groups in which it was possible to lose money (the loss condition), two to groups in which negative payoffs were truncated at zero (the no-loss condition). Thus the eight groups of four friends separated and formed four groups of eight strangers to play the commons dilemma game. (Dawes-McTavish-Shaklee (1977) p.4.)

The design was intended to identify, among other things, the effect of communication on contributions.

2.2.2 Results

The data on non-contributions (X) is displayed in Table 3.

Table 3. Non-Contribution (frequency of choosing X)

	Condition			
Condition	No Communication	Irrelevant Communication	Unrestricted Communication	Communication Plus Vote
Loss	.73	.65	.26	.16
No Loss	.67	.70	.30	.42

(Dawes-McTavish-Shaklee (1977) p.5.)

The main result appears to be (see Dawes (1980)) that only 31% contribute without communication or with irrelevant communication while 72% contribute when relevant communication occurs. A secondary but puzzling result is that the no-loss treatment had apparently no effect.

2.2.3 Comments

The first thing to notice is that this really is a public good environment as described in Section 1.4. Let z_i, the initial endowment, be 0. Require that $t_i \in \{0, 9.50\}$. Let $g(t) = [(12/9.5)t]/8$. Finally, let $U^i(t_i, y) = z_i - t_i + g(t)$. Then, for example, if 2 individuals contribute, their $t = 9.50$, and 6 do not, their $t = 0$, then contributers receive $U^i = 0 - 9.50 + [(12/9.5)(2 \times 9.50)/8] = -6.50$ and non-contributers receive $U^i = 0 - 0 + (12/9.50)(2 \times 9.50)/8 = 3.00$. Compare this to Table 2 under the loss condition.

A second observation concerns the lack of impact of the no-loss treatment. Let us look first at the structure of the problem. In the loss condition (ignoring for now the

Table 5.

# other defectors	*loss* return on \$1	*no loss* return on \$1
0	.158	.158
1	.158	.12
2	.158	.05
3	.158	.093
4	.158	.16
5	.158	.3
6	.158	.75
7	.158	∞

condition should lower the incentive to defect by raising the marginal benefit of contributing.[39] As can be seen the incentive effects of the no-loss treatment are complex and out of control. This should give experimentalists reason to pause. A relatively simple appearing alteration in the payoff structure, replacing negative numbers with zeros, creates a very complex change in the incentive structure because the direction of the effect depends on the subjects' expectations which are not controlled by the experimenter.[40]

A third observation is that the fear of losses on the part of the experimenters that led them to create friendship groups and no-loss conditions could have been avoided by recognizing that the experiment is almost identical to that described in Section 1.1 if an initial endowment of \$9.50/subject had been provided. Of course that would have cost an additional \$9.50×284 or about \$2,700. An alternative way to save money and to avoid forcing subjects into losses would have been to add \$9.50 to each entry (so all payoffs are non-negative) and then divide all entries by some number to lower the total paid out.[41]

[39]In the extreme case if 7 others plan to defect then each subject faces no cost from contributing but can provide 1.50 to the others by doing so.

[40]I have not had the time to figure out in what way this might explain the data on predictions of others' behavior. Dawes et al. claim defectors expected more defection than did cooperators. But the incentive structure suggests that the no-loss incentives would lead those who expect defection by others to defect less often than those who expect more cooperation. It is further claimed by Dawes et al. that

> The possible loss manipulation was not only ineffective in eliciting differential cooperation, it was ineffective in eliciting differential predictions about others' behavior as well. (Dawes-McTavish-Shaklee (1977) p. 5.)

I remain suspicious and believe this needs more investigation.

[41]For example, a rough calculation for these Dawes experiments suggests a payoff of \$3.75 to 5.5 defectors and -\$5.75 to 2.5 (=.3×8) contributors for a total of \$52.50. A similar calculation for communication suggests \$1.25 to 5.5 (=.7×8) contributors and \$8.25 to 2.5 defectors for a total of \$137.50. Adding \$9.50 to each of 8 payoffs would yield a cost for each trial of 76+137.50=\$223.50. Dividing by 2 would then

depending on the total contribution to the public exchange. In the words of the experimenters:

> The experiment was conducted during a single summer and fall using 256 high school students between the ages of 15 and 17. Subjects were divided into 64 four-person groups, resulting in eight groups assigned to each treatment condition ... Since each group contained two female and two male subjects, each cell contained 16 males and 16 females[42] High school-age subjects were selected for study because we felt that the amount of money at stake in their decision (about $5.00) would be most meaningful to young people and that at the same time these subjects would be old enough to understand the investment decision they had to make. (Marwell-Ames (1979) p. 1341.[43])

> The study was performed in a "natural" setting, in that all interaction with the subjects was by telephone and mail, with subjects remaining in their normal environments throughout the course of the research.
>
> After willingness to participate had been established by phone, the subject was mailed a set of instructions appropriate to the experimental condition to which he or she was assigned ...
>
> Within a few days an experimenter telephoned the subject to go over each point in the mailed instructions. This discussion usually lasted 15-20 minutes ... An appointment was then made for another telephone conversation the next day (or as soon as possible), in which the subject could invest the study tokens.
>
> In this next telephone call the subject invested the tokens in either of two exchanges (which are explained below) or split them between the two. (Marwell-Ames (1979) pp. 1342-43)

The payoff table, given to the subjects, for a large group[44] of 80 with unequal benefits (designated blue and green) and unequal resources is provided in Table 6.

One unusual feature (corrected and tested in Marwell-Ames (1980)) about this induced valuation structure is the peak at 7,999 total tokens. At all other levels the marginal benefit from contributing 1 more token (worth 1¢) is less than 1¢ whereas at

[42]This is a Marwell-Ames footnote: "One male subject named Chris was inadvertently classified as female and the mistake was not discovered until long after completion of the experiment. Thus, one group was composed of three males and one female. Deletion of this group or this subject makes no meaningful change in the results."

[43]I would like to thank the University of Chicago Press for permission to quote from this report.

[44]How a group of 4 becomes a group of 80 is discussed below.

possible to tell them that there were any number of members in their group and have them make their investment decisions in terms of this assumption. Telling half our subjects that they were in large, 80-person groups was the only element of deception in this experiment. (Marwell-Ames (1979) p. 1345.

2.3.2 Results

The finding claimed by Marwell and Ames was "a lack of support for ...the strong free rider[46]." Approximately 57% of available resources are invested in the public good. If those subjects whose endowments are greater than the provision point are excluded, then the contribution rate is 41%.

> In all, tests of the hypotheses derived more or less directly from the economic theory support a *very* weak free-rider hypothesis, with the proviso that groups containing a member whose interest is greater than the cost of provision invest substantially more in public goods than do other groups. No other hypothesized process demonstrated a substantial effect on group investment. (Marwell-Ames (1979) p. 1352)

A second finding which we will examine more closely in Section 3.4.1. was that the rate of contribution was less if initial endowments were unequal.

2.3.3 Comments

A number of issues are raised by this study. Many have since been addressed either by Marwell's group (see Marwell and Ames (1980), Alfano and Marwell (1980), and Marwell and Ames (1981)) or by the economists who initially thought something must be wrong if there was so much contribution.

The existence of a provision point could quite obviously have increased contributions to 44%. But in a later study by Marwell and Ames (1980) the provision point was removed as in Table 7.

The result reported after the change was that "the subjects averaged 113 tokens invested in the group exchange or approximately 51% of the tokens they had available." (p. 932) This would seem to blunt the criticism that subjects were focused on a focal point equilibrium. However, notice that multiple Nash equilibria still exist at positive levels of contribution. For example at 1,999, 3,999, etc. a 1¢ contribution yields a personal return

[46]The strong free-rider hypothesis is that everyone contributes zero to the public good

of 80. Since all of the experimental interaction was over a phone, no subject could know for sure what the group size was other than relying on the veracity of the experimenter. How do we know for sure what the subject believed? Since the experimenter was deceptive about N=80, why not about N=4? It is believed by many undergraduates that psychologists are intentionally deceptive in most experiments. If undergraduates believe the same about economists, we have lost control. It is for this reason that modern experimental economists have been carefully nurturing a reputation for absolute honesty in *all* their experiments. This may require costlier experiments where not just 4 subjects but 80 are paid. It may require more clever procedures to get 80 subjects together at one time. But if the data are to be valid, honesty in procedures is absolutely crucial. Any deception can be discovered and contaminate a subject pool not only for that experimenter but for others. Honesty is a methodological public good and deception is equivalent to not contributing. It is important for the profession to remember this, especially since, as John Kagel pointed out to me, it is conventional wisdom that economists free ride.

2.4 Economists Begin to React

The work of Marwell and Ames described in section 2.3 provided stark and clean evidence against the standard economic predictions: data confirmed that subjects contribute and do not all fee ride. The research caught the attention of the new economic experimentalists who had been focusing on markets and who felt sure that the study by sociologists must be flawed. Theory could not be that wrong, could it?

In this section we will look at two studies which were created in direct response to Marwell and Ames. Indeed the purpose of both Kim and Walker (1984) and Isaac, McCue, and Plott (1985) was to show that Marwell and Ames were wrong and "to explore the behavior of groups within a set of conditions where we expected the traditional model would work with reasonable accuracy." (Isaac, McCue, and Plott (1985) p. 51.) By this they mean they expected to find free-riding and underprovision of the public good, a finding that would be at odds with Marwell and Ames (1979, 1980) and Dawes, McTavish, Shaklee (1977).

2.4.1 Procedures

The main divergence of both Isaac, McCue, and Plott and Kim and Walker from Marwell and Ames was the introduction of repetition; that is, subjects faced the same decision process for a series of periods rather than just making their decisions once. We will describe the Isaac, McCue, and Plott experiment.[48]

[48]Kim and Walker is covered in Section 2.4.3.

> There were two standard rules regarding the information of participants: first, the subjects were not allowed to communicate with one another during the experiment. Secondly, the individuals had no knowledge about the nature of any payoff charts other than their own. In a technical sense it was *public* information that no one had information about other subject preferences. Furthermore, it was public information that the final period was known with certainty to no one. (Isaac-McCue-Plott (1985) p. 57.)

2.4.2 Results

Did Isaac et al. find evidence that contradicts the Marwell-Ames results? The answer is yes and no. In the first period decisions, contributions strongly resemble those observed by Marwell-Ames. On average first period contributions yield a public good level of 8.8 which yields a group payoff of 50% of the maximum possible. So the first decisions of subjects are similar in both studies. However, by the fifth period the average number of units provided has dropped to 2.1 for a group payoff which is 9% of the maximum. So, after repetition, one can observe significant underprovision and the free-riding phenomenon.

2.4.3 Comments

The relatively high initial contribution rate which declines with repetition has been found by others and is discussed in more detail in Section 3.2. Kim and Walker (1984) with a similar design found contributions provided 41% of the maximal group payoff in the first period and declined to 11% by the third period. I have not emphasized their study more because, although they were extremely careful to try to eliminate nine experimental design features of earlier studies which they argued might be invalidating factors,[50] they misled their 5 subjects hoping they would think there were actually 100 subjects.[51] Whether the subjects believed that or not is unknowable.

An innovative feature of both the Isaac-McCue-Plott and Kim-Walker experiments was the use of a declining marginal payoff curve (in the public good) for each subject and no constraint on contributions within a period imposed by an initial endowment of tokens (just a total capital constraint across all periods). Such a payoff structure means that the private incentives not to contribute increase as the others' contributions increase. Let us look at that incentive. For the high types, contributing one dollar more to public good provision yields $\frac{1}{1.30}$ units of the good which yields an extra benefit, to that individual, of $m = [.44 - .011q]\frac{1}{1.30}$. When $q = 0$, $m = .3385$, when $q = 10$, $m = .25$, and when $q = 24$ (the group maximal amount) $m = .13$. For low types we have $m = .2123$ when $q = 0$,

[50] "Factors which, if they intrude into the experimental situation, will render the theory ...inapplicable" (p. 11). Such factors involve a loss of control by the experimenter.

[51] I have indicated in Section 2.3.3 how I feel about this design to save money.

Thomas (1984). Isaac and Walker continue today in systematic efforts to understand behavior in voluntary contributions situations. I include a description of their first work here because of the craftsmanship with which it was designed. But even with a careful design they were left with many unanswered questions. In particular they conclude that "free riding is neither absolutely all pervasive nor always nonexistent ... The extremes of strong free riding and near-Lindahl optimal behavior can and do occur." (p. 140.) So we still do not know what to expect – anything can happen.

Nevertheless because of the care taken, we do learn something about the existence of

> "... systematic effects of attributes of the decision setting upon the existence of free riding ... General theories about the importance of free riding are not failing because of some inexplicable randomness in previous experiments." (Isaac-Walker-Thomas (1984) p. 125.)

2.5.1 Procedures

Four undergraduate students at the University of Arizona were brought into a room and each was assigned to a (PLATO) computer terminal. All communication, including instructions to the subjects, was done through the terminals. As they indicate

> One feature of this set of experiments that differs from the previously cited experiments is the use of the Plato computer system for conducting the experiments. This system allows for minimal experimenter-subject interaction during experimental sessions as well as insuring that all subjects see identical programmed instructions and examples for a given experimental design. The use of the computer system also facilitates the accounting process that occurs in each decision period and minimizes subject's transactions costs in making decisions and recalling information from previous decisions. (Isaac-Walker-Thomas (1984) p. 116.)

Continuing the description:

> The programmed instructions described to the participants the following decision problem: given a specific endowment of resources (tokens) participants faced the decision of allocating them between an individual exchange (private good) and a group exchange (public good). The individual exchange was described as an investment which paid to the investor $.01 for each token invested. ... The group exchange was explained to the participants as an

incentives for the group interest. If we increase N but keep M constant by increasing a then the incentives for the group interest increase relative to the incentives for individual interest. It does not seem possible to change N without changing the incentives between group and self interest. Isaac, Walker, and Thomas deal cleverly with this by considering a 2 × 2 design with N =4 or 10 and M =.3 or .75. Always p=1. Then, since $a = NM$, we have four parameter choices (N, M, a):(4,.3,1.2), (4,.75,3), (10,.3,3), and (10,.75,7.5). These allow comparing a change in N keeping M constant (for both M=.3 and M=.75) and comparing a change in N keeping a =3 constant.

Finally, experience is measured as previous participation in similar experimental sessions.

2.5.2 Results

The only extant formal theory at the time of these experiments predicts no contributions. That is clearly false as can be seen in Table 8 and Figure 2.

Ave. % contributions for

		M=	
		.3	.75
N=	4	19	57
	10	33	59

Figure 2.

Table 8. Data for IWT (1984)

	Period										
	1	2	3	4	5	6	7	8	9	10	Ave.
% contrib. (all)	51.1	47.2	44.1	47.4	46.7	38.1	40.6	35.2	35.8	37.3	42.4
% contr.(M=.3)	43	35	28	32	26	25	20	17	20	17	26
% contr.(M=.75)	60	59	60	63	67	51	61	53	52	57	58
% contr.(inexper.)	53	53	45	50	55	43	50	41	39	44	47
% contr.(exper.)	49	41	43	45	38	33	31	30	33	30	37
% contr.(N=4)	50	50	38	40	38	30	36	32	38	30	38
% contr.(N=10)	56	50	40	41	41	34	32	33	37	35	40

The average % contribution across all treatments is 42% and the average across first periods is 51%. These look very much like Dawes et al. and Marwell-Ames. But the variance is high, with contributions ranging from 0% (period 8 with M=.3, N=4, experienced subjects) to 83% (period 5 with M=.75, N=4, and inexperienced subjects).

power. It seems pretty easy to demonstrate that subjects contribute. All experiments have periods with at least 40% contributions. But determined experimenters also seem to be easily able to extinguish most but not all of the altruistic impulse (if that is what it is) through low marginal payoffs and repetition. We need to better understand the causes of these observations. But none of these experiments is truly comparable with any of the others. Look at the summary of the designs and results in Table 9. At least two features, sometimes more, change between any two experiments.

Table 9. Summary of Designs and Results

	B	DMS	MA	IMP	KW	IWT
Numbers	?*	8	4,80*	10	100*	4,10
Marginal Payoff	?	.16, .16-.75	non-linear	.34[b]-.06	.02[b], .05, .07	.3, .75
Repetition	no	no	no	yes[c]	yes[c]	yes
Provision Point	yes	no	yes	no	no	no
Tokens	no	1/person	yes	no	no	yes
Heterogeneity	?	no	no, yes	yes	no	no
Experience	no	no	no	no	no	yes, no
Communication	no	yes, no	no	no	no	no
Moral suasion	yes	no	no	no	no	no
% contributions						
initial period	NA	31%[a]	41%	50%	68%	51%
last period	NA	NA	NA	9%	8%	19%

*= deception played a role.
?= uncontrolled.
a= w/o communication. (It was 71% with communication.)
b= declines as q increases.
c= subjects did not know number of repetitions.
Note= Two entries mean both treatments were tried.

B = Bohm (1972)
DMS = Dawes, McTavish, and Shaklee (1977)
MA = Marwell and Ames (1979)
IMP = Isaac, McCue, and Plott (1985)
KW = Kim and Walker (1984)
IWT = Isaac, Walker, and Thomas (1984)

The two closest designs may be Marwell and Ames (1979) and Isaac, Walker, and Thomas (1984) but even they differ in marginal payoff, provision point, and repetition. The difference in designs implies that sometimes subjects contribute and sometimes they

by existing research into three main categories: the *environment* (numbers, strength of incentives, extent of homogeneity, thresholds imposed by the production technology, initial information structure, gender, ...), *systemic variables* (fairness concepts, altruism, risk attitudes, beliefs, ...), and *design variables* (such as unanimity rules, structured communication, and moral suasion). The variables in the first two categories are aspects of what I have called the environment: I have split them into two parts to emphasize that some are more easily controllable with current experimental technologies. In particular, those identified as environmental are relatively straightforward to control, while those listed as systemic are currently more difficult. The variables in the category, labeled design variables, are factors identified by experimentalists which should be more properly thought of as aspects of institutional design. These variables are amenable to change and the mechanism designer can use them to improve solutions to the free rider problem.

In Table 10, I summarize what seems to be the consensus of experimentalists about the effect of a change in one of these variables on the change in total contributions as a percent of the efficient level. Some effects are more certain than others, in that replication has confirmed initial findings. Understanding behavior would be easier if each of these variables had a separable and identifiable effect on contributions.[61] Unfortunately that is not true: the details of the environment seem to matter. Left unexplained in the table are what I call cross-effects. The latter are very important and not well tracked in the literature.[62] In some cases, cross-effects may even reverse the direction of effect of a variable. We will see this below.

I organize the rest of this chapter as follows. In Section 3.1, I describe a very important structural feature in environments with public goods which must be tracked in order to make comparisons across experiments. In Section 3.2, I take up results dealing with repetition and the related issues of learning and experience. In 3.3, I cover the strong effects of marginal payoff (and its related problem of numbers) and communication. In 3.4, I turn to weak effects. In 3.5, I discuss some of the factors which may be important but of which little is known primarily because an inability to control their impact on an experiment. In Section 4, I conclude with some final thoughts on what we really know and where we might go.

[61] I have in mind here something like the robustness of the supply-demand equilibrium with private goods. See Chapter (Holt).

[62] For example, the effects of changes in the marginal per capita return seem to vary depending on group size. See Isaac and Walker(1988b) and Isaac, Walker, and Williams (1990).

are generally many non-cooperative equilibria, each of which may be optimal, and none of which is dominant, and the task of the players is to coordinate their actions to select one. The environments of Dawes et al. (1977) and of Isaac and Walker (1988b) are of the prisoners' dilemma variety. The environment of Marwell and Ames (1979) is more like a game of chicken. It is not surprising that we see different results in these two types of environments. For example, if the players can talk one might suspect that in the game of Chicken they would correlate their strategies. This is even easier in repeated play because they can then try to equalize sacrifice. But one might expect that communication would have a lesser effect in dilemma games since there is no problem of coordination.

Table 11.[64]

Prisoner's Dilemma (MPCR=.75)

	D	C
D	4,4	7,3
C	3,7	6,6

Chicken (require 1C)

	D	C
D	4,4	10,6
C	6,10	6,6

payoffs = (row player, column player)
D = do not contribute, defect
C = contribute, cooperate

For now let us address the simpler problem: do thresholds cause contributions to increase, *ceteris paribus*. One often sees campaign targets set when raising funds for charities or university endowments. Do these work? We do not have much evidence but what there is seems to suggest that increases in thresholds increase contributions but also increase the probability the target will not be reached.[65] There are many papers reporting on experiments with thresholds but six actually vary the threshold to determine its effect.[66] Marwell and Ames (1980) actually compare contributions with and without the provision point discussed in Section 2.3. They found no significant difference. However, as we mentioned in Section 2.3.3 there remained a problem: while they did eliminate the major jump in payoff at 8,000 tokens, in their no-provision point design there are still actually 9 provision points since the payoff is constant across 2,000 token intervals. (See Table 7 in Section 2.3.) What changed was marginal payoff at each provision point: some

[64]In the Prisoner's dilemma, each player's dominant strategy is D. There is one Nash equilibrium: (D,D). In Chicken, there are two Nash equilibria: (D,C) and (C,D). There are no dominant strategies. See Chapter 1 for an early history of these experiments.

[65]So if you are running a campaign you want a high enough target to encourage contribution increases but low enough to prevent failure to attain the goal. This is the fund-raiser's art.

[66]These are Marwell and Ames (1980), Dawes, Orbell, Simmons, and van de Kragt (1986), Isaac, Schmidtz, and Walker (1988), Rapoport and Suleiman (1993), Suleiman and Rapoport (1992), and Palfrey and Rosenthal (1991a).

and Rosenthal (1991a) find similar ambiguities in a heterogeneous environment. There N=3, marginal payoffs are heterogeneous, and each agent has one token. The threshold is K of N. They find that % contributions increase as K is increased from 1 to 2 but decrease as K is increased from 2 to 3.

In the Palfrey and Rosenthal (1991a) framework, pure strategy Bayesian equilibrium theory predicts a decrease from K=1 to 2 and from K=2 to 3 for their parameters. However, a careful look at mixed strategy equilibria for these environments with thresholds suggests that game theory would predict that changes in the threshold can have an ambiguous effect on changes in contributions. See, e.g., Palfrey and Rosenthal (1988). The ambiguity is resolved only when specific parameters are known. The theory is telling us we should not expect a definitive answer to "does an increase in threshold increase contributions" which is independent of other factors. The data are supporting that view.

3.2 Experience, Repetition, and Learning

A natural explanation for the large rate of contribution in many voluntary contribution experiments can be found in the inexperience of the subjects. Perhaps a 40-60% contribution rate occurs simply because if one must contribute a number between 0 and Z and does not understand the implications of the act then a natural choice is somewhere in the middle.[71] This would be especially true of experiments such as Isaac-Walker in which payoffs are linear. Clearly it is important to be able to discover whether the data are simply the result of confusion and inexperience or the result of some more purposeful behavior. One way to do this is to create payoffs such that the two key points of interest, the dominant strategy contribution and the group optimum contribution are moved to the interior of $[0, 100]$. That is discussed in Section 4. We explore another way here.

Repetition (not replication) has become a common feature[72] of much research in experimental economics in an effort to eliminate or control for at least two types of experience effects: learning how to play the particular class of games, such as what keys to press in a computerized continuous auction or how to read a particular payoff schedule, and learning about the specific game one is in, such as what the environment is and what the other subjects are like. One can easily control for the first type of experience by simply bringing back subjects who have previously participated in similar experiments. This has not been done as often as one might suspect. The data from Isaac, Walker, and Thomas (1984) and Palfrey and Prisbrey (1993) suggest that subjects who have previously been in a voluntary contributions experiment contribute less than those who are first-timers

[71] An alternative yielding the same data would be to randomize between contributing 0 and contributing z. This, however, does not appear to be supported by individual data. But I am not sure whether a more diffuse contribution strategy based on random behavior can be rejected since one only sees realizations and not the strategy itself.

[72] At least 25 of the 40 or so papers reported here have used this technique.

strategy is, given this environment and this collection of subjects, then a better model would be something like a learning algorithm found in Miller and Andreoni (1990), Boylan (1990), Crawford and Haller (1990), or Kalai and Lehrer (1990). If everyone learns, then one should observe the contributions converge to the non-cooperative equilibrium after enough periods. This seems to happen after 10 iterations in small groups. We do not know how long it would take in large groups.[75]

The experimental puzzle is to develop designs which allow separation of these two types of temporal phenomena and help us identify those aspects of the institution which speed learning or channel strategy when that is desirable. Andreoni (1988b) represents a good start on this complicated problem. In a unique design he compared two treatments called Strangers and Partners in an Isaac-Walker environment with p=1, $\frac{a}{N}$=.5, N=5, and z_i=50 all of which were known to everyone. The Partners played repeatedly 10 times just as in Isaac, Walker, and Thomas (1984). The Strangers were 20 subjects randomly reassigned by computer to groups of 5 after each repetition. The idea was to separate strategic play by Partners from no strategic play with Strangers. Thus one should see only learning in the Strangers condition but see learning and strategy in the Partners condition. The data are in Table 13. (Andreoni (1988b)[76])

Table 13. Average Investment in Public Good per Subject

	Round										
	1	2	3	4	5	6	7	8	9	10	All
Partners	24.1	22.9	21.5	18.8	18.4	16.8	12.8	11.2	13.7	5.8	16.6
Strangers	25.4	26.6	24.3	22.2	23.1	21.9	17.8	19.7	14.0	12.2	20.7
Difference	-1.3	-3.7	-2.8	-3.4	-4.7	-5.1	-5.0	-8.5	-0.3	-6.4	-4.1

Surprisingly, contrary to received strategic theory, Partners contribute less than Strangers and the difference increases over time. Andreoni further argues that since there is no reason Strangers should learn slower than Partners, learning alone is not responsible for the observed decay in contributions. But strangers are in a noisier environment and, therefore, may indeed learn more slowly. A strategic hypothesis, that giving occurs early because it generates more later, appears to be inconsistent with the data. A learning hypothesis may be ok. That decay in contributions occurs with repetition in environments with a zero dominant strategy is indisputable. What explains the phenomenon remains to be found. Follow up research is needed.

[75]Isaac, Walker, and Williams are apparently now running some experiments for up to 60 decision rounds which may provide some answers.

[76]I would like to thank Elsevier Science Publishers for permission to quote from this report.

We can also get some indirect evidence on the effect of marginal payoffs from two other sources; experiments with asymmetric payoffs and experiments with rebates. An example of the former can be found in Section 2.4 where Isaac, McCue, and Plott (1985) found (conclusion 7 p. 64) that "individuals in the high payoff condition contribute more than individuals in the low payoff condition." Marwell and Ames (1979) also report more contributions from "high interest" (blue) subjects (see Figure 6 Section 2.3 for the payoffs) than "low interest" (green) subjects. Other confirming evidence with asymmetric payoffs can be found in Brookshire, Coursey, and Redington (1989a), Fisher, Isaac, Schatzberg, and Walker (1988), Palfrey and Rosenthal (1991a), and Rapoport and Suleiman (1993). One of the more powerful sets of supporting data is in Palfrey and Prisbrey (1993) who mimic the Isaac and Walker framework but allow the private value to be asymmetric across subjects. In particular $u^i = P_i(z - c_i) + a\sum c_i$ where P_i is private information, drawn randomly and uniformly from the set $\{1, 2, \ldots, 20\}$. Here it is a dominant strategy to contribute if $P_i < a$ and to not contribute if $P_i > a$. They used a total of 64 subjects in four different experimental sessions involving 4 person groups. A very simple probit model, Probability(contribute)= f(constant + $\alpha(\frac{a}{p})$) is able to correctly predict 83% of the observations.[79]

Clearly, the marginal payoff $\frac{a}{p}$ is an important effect.[80] This is true whether thresholds are present or not. Indeed one other source of confirming data comes from the analysis of rebates in threshold situations. Dawes, Orbell, Simmons, and van de Kragt (1986) study two changes in their simple payoff structure, both of which increase the marginal payoff to contributing *ceteris paribus*. In their baseline condition each subject could contribute or keep \$5. If at least K of N contribute, then all get \$10. In a "no fear" condition all contributors get their \$5 back if less than K contribute. In a "no greed" condition subjects who do not contribute only get \$5 more if at least K contribute. The data are in Table 14. In another study with thresholds Isaac, Schmidtz, and Walker (1988) also find a significant effect for rebates.

Table 14. % Contributing

	K=3 of 7	K=5 of 7
baseline	51%	64%
no fear	61%	65%
no greed	86%	93%

The only report which might cast any doubts on the strong effect of increasing marginal payoffs can be found in Isaac, Walker, and Williams (1990). Here they begin to explore the effect of large numbers (N=40 and 100) without the deception which

[79]The t statistic on the estimated coefficient α is 86.358.

[80]For additional work see Carter et al. (1992).

> for changes in payoffs were also kept proportionate, so that, for example, 20 times as many tokens had to be invested by the large group before the payoffs became larger than one cent per token. Thus, the mean contributions were required to be identical for identical effects. (Marwell and Ames (1979), p. 1346.)

I think this means that $\frac{a}{N}$ was held constant as N increased but I cannot really tell from their description. Chamberlin (1978) found a negative effect on contributions as N increased. Bagnoli and McKee (1991) also found a negative effect particularly in early periods. They conjecture "individuals in a larger group may find it more difficult to focus on a particular equilibrium vector of contributions.[82]

I find the Isaac-Walker experiments without thresholds most revealing because they attempt to control for the purely private incentives (measured by MPCR) in order to isolate the effect of numbers and they have tried large numbers without deception. Initially they used groups of 4 and 10 and MPCRs of .3 and .75. Those data were displayed in Section 2.4, Table 8. They found that MPCR mattered and N did not. The only way N mattered was if a were held constant causing a crowding effect where $MPCR = \frac{a}{pN}$ declines as N increases. Believing they had discovered a systemic relation between contribution and numbers, they then designed with Williams an experiment for N=40 and N=100. In doing so they had to overcome several methodological difficulties. To avoid the extremely high cost of such experiments, they developed a new method for rewarding their subjects. In their own words:

> As explained in the class handout, subject i's experimental dollar earnings were converted into the following "performance index" prior to being converted into extra-credit points:
>
> $$\frac{\text{i's Actual Earnings - i's Minimum Possible Earnings}}{\text{i's Maximum Possible Earnings - i's Minimum Possible Earnings}}$$
>
> which can range from 0 to 1 for each individual. At the end of the final round, this fraction was computed for each individual (based on earnings in all rounds), multiplied by 3, and added to the subject's final grade average. Thus, the range of possible extra-credit points was $[0, 3]$. The performance index was used so that the maximum and minimum possible extra-credit earnings did not depend upon the design cell assignment. All classes from which subjects were drawn utilized a 100-point scale and, with minor modifications, used a standard mapping of point totals into letter grades (A=90's, B=80's, etc.).

[82]One other interesting set of experiments with Cournot oligopoly, reported in Morrison and Kamarei (1990), finds no effect from numbers. As with thresholds there is an interior equilibrium but unlike with thresholds it is unique.

> First, the impact from variations in the magnitude of the marginal per-capita return from the public good (MPCR) appears to vanish over the range [0.30, 0.75]. Second, with an MPCR of .30, groups of size 40 and 100 provide the public good at higher levels of efficiency than groups of size 4 and 10. Third, with an MPCR of .75, there is no significant difference in efficiency due to group size. (Isaac, Walker, and Williams (1990) p.13.)

Finally, in an attempt to rescue the "MPCR effect" they ran three single session 40 person experiments with money (at a cost of about $900 each) and an MPCR=.3. They found no deterioration in contributions but, in fact, a slight increase over the "no money" experiments. Continuing their rescue attempt they ran 4 experimental sessions with N=40 but MPCR=.03, three with credit points and multiple sessions and one with money and a single session. Here they finally found contribution rates that looked more like the N=4, MPCR=.3 experiments. Instead of using large numbers to hide one's selfishness, subjects actually seem to become more cooperative in the larger groups. This would be consistent with the existence of the selfish vs. altruistic tradeoff described earlier where holding $\frac{a}{Np}$ constant and increasing N increases contributions. But another possible implication of all this is that voluntary contributions experiments with public goods, as many do them, are yielding data which are not very sensitive to the incentives provided by the experimentalists.

What do we now know and what do we need to find out? Clearly, subjects appear to respond positively to increases in their MPCR although the effect is diluted in large groups. To really pin down the relationship between contributions, MPCR, and N will cost a lot of money and effort since we need to fill in data between N=10, 40, and 100. We also need observations for more values of MPCR than just .03, .3, and .75. There are many other observations on various pairs of MPCR and N in the literature but they need to be extracted and tabulated.[85] This would be, to me, a very interesting subject for a dissertation.

Also, can we now conclude altruism is at work? Rather than running a very large number of experiments, one could try to leap to an understanding by creating a new theory which explains or predicts a relationship $(\Sigma t_i/N) = f[MPCR, N, \alpha]$ where Σt_i are total contributions, N=number of subjects, and α are, perhaps uncontrolled and unobserved, parameters. The development of such a theory would also point to new experiments which might require new theory, and so forth. Let us see how this might work.

Standard game theory predicts, for the Isaac-Walker environment that

$$(\Sigma t_i)/N = 0 = f(M, N, \alpha)$$

[85]One of the problems a theorist faces in trying to decide what we know is the fact that many experimentalists make very little effort to relate their results to others.

A third theory, based on the idea that subjects care about fairness or equality, would have $V^i = u^i + \delta\frac{1}{N}(\sum_j (u_j - \overline{u})^2]$ where $\overline{u} = \frac{1}{N}\sum_j u^j$. When $u^i = p(z - t^i) + \frac{a}{N}\sum_j t_j$ then

$$V^i = p(z - t^i) + a\bar{t} + \frac{\delta}{N}\sum_j \left[p^2(\bar{t} - t^i)^2\right]$$

where $\bar{t} = \frac{1}{N}\sum_j t_j$. Differentiate V^i with respect to t_i, set it equal to zero and get

$$-p + \frac{a}{N} - 2\frac{\delta}{N}p^2(\bar{t} - t_i) = 0$$

or

$$t_i = \bar{t} + \frac{N(1-M)}{2\delta p}.$$

The expected % contribution is therefore

$$E[\%C] = E\left(\frac{\sum t_i}{N}\right) = \frac{\sum_j \bar{t}^j}{N} + \frac{N(M-1)}{2p}E\left(\frac{1}{\delta}\right)$$

where $\bar{t}^j$ is j's belief about others expected contributions.[88] Therefore

$$E(\%C) = \sigma\left[E(\bar{t}), \frac{N(M-1)}{P}E\left(\frac{1}{2\delta}\right)\right]$$

where $\partial\sigma/\partial p > 0$ and $\partial\sigma/\partial N < 0$ since $M < 1$. If payoffs are increased then $E(\%C)$ decreases since N, M stay constant but p increases.

We now have three theories based on three different uncontrollable and unobservable parameters. Each is consistent with the finding that increases in M increase contributions. Each yields different predictions for the comparative statics of N, P, and z and they can, therefore in principle, be separated in the lab even if full control is not possible. At least two should be demonstrably incorrect based on data. Maybe the third is also.[89] The next round belongs to the experimentalists.

---

[88]Since $M < 1$, $t_i < \bar{t}^i$ so, strictly speaking, $t_i = 0$ if $\bar{t}^i + \frac{N(M-1)}{2\delta \cdot p} \leq 0$. Thus $E(\%C)$ is an overestimate of the correct number. This does not affect the comparative statics below. Also it does provide a somewhat *ad hoc* explanation for a decline in contributions with repetition since if subjects use last periods contributions to estimate this periods $\bar{t}^i$ then contributions will follow the time path given by

$$\%C_\tau = \%C_{\tau-1} + k\left(\frac{N(M-1)}{P}\right)$$

where k is a subject pool specific constant.

[89]Since none predicts splitting of tokens, a well-known fact, all are technically deficient. See Chen (1993) for a theory which might explain splitting.

Table 15. Duncan's Multiple Range Test for Contibutions

Treatment	Mean	N	Duncan Grouping
No Information, No Announcement	60.3	72	A
Information, Announcement	59.3	72	A
Information Only	46.0	72	B
Announcement Only	34.0	72	C

Note: Means with the same letter are not significantly different at .05
Sell and Wilson (1990) p.23. (I would like to thank J. Sell for permission to quote from this report.)

to reduce contributions. With verification it helps (59.3% vs. 46.0%). But no information or communication, the one-shot equivalent, yields the same rate of contribution as information and communication, the repetition and communication equivalent. Sell and Wilson state:

> Our results are consistent with other reported results using a voluntary contribution mechanism. Everywhere we observe a consistent decay in provisioning that extends over the periods ... Where individuals are able to make announcements and check on one another's behavior, they are somewhat less likely to lie in their announcements (the Pearson's correlation coefficient between one's announcement and contribution is .34, compared with .10 under the *Announcement Only* condition).

But they also admit that they are "far from capturing the essence of communication."

Dawes and Orbell have been systematically studying communication in dilemmas, trying to identify that essence. Experiments without thresholds are reported in Dawes, van de Kragt, and Orbell (1987), Orbell, van de Kragt, and Dawes (1988), and Orbell, Dawes, and van de Kragt (1990). Their present position seems to be that communication "works either because it provides an occasion for (multilateral) promises or because it generates group identity – or, possibly some combination of those two hypotheses." (Orbell, Dawes, and van de Kragt (1990) p.619, footnote 7.) They also note that multilateral promising only goes so far. In their words

> Perhaps the psychology of multilateral promising reduces to the psychology of a set of bilateral promises – perhaps, that is, people in our experiment felt they were making promises, as Hobbes put it, "every one apart, and Man by Man." But the straightforward interpretation of our data is that people do revert to what we have called multilateral promising and that, when they do,

3.4.1 Environment

Homogeneity and Information In many of the early experiments with voluntary contributions, all subjects were given the same preferences and endowments.[94] There is now reason to believe that such homogeneity in the environment has a positive effect on contributions. Isaac, McCue, and Plott (1985) conjectured this in their attempt to reduce contributions, and included asymmetries in payoffs. But they did not control for the effect by also studying their environment without asymmetries.

We have already seen that contribution rates are responsive to marginal payoffs. See Section 3.2. What is at issue here is whether there is an additional effect due to heterogeneity in payoffs or endowments. For example, suppose if everyone is the same, contributions are 60% with MPCR=.75 and 30% with MPCR=.3. Now suppose we have an environment with half MPCR's=.75 and half=.3. Is the aggregate contribution rate 45%? Or are the contribution rates of the high MPCR types now less than 60% since they can safely mimic the behavior of the low MPCR types? Theory is no help since it predicts contributions of 0 no matter what. What do the data say?

In Table 16, I provide a summary of five papers which compare *ceteris paribus* contributions in homogeneous environments to contributions in heterogeneous environments. Looking only at the last column would lead one to conclude that heterogeneity lowers contributions. But the effect can clearly be dampened by a lack of information and/or a lack of repetition (or repetition without reports of previous outcomes). Can we separate these effects? Let us look at the role and impact of alternative information structures.

Table 16.

	Threshold	Repetition	Complete[c] Information	More Heterogeneity Implies % Contribution
Bagnoli and McKee (1991)	Y	Y	Y	decrease
Brookshire et al. (1989a)	N	Y	Y and N	decrease
Fisher et al. (1988)	N	Y	N[a]	decrease in first 10 periods
Marwell and Ames (1979, 1980)	Y	N	N	no effect
Rapoport and Suleiman (1993)	Y	Y[b]	Y	decrease only at high threshold

[a] Subjects were told values "might not be the same" and all values were changed at period 10.

[b] Repetition occurred but no information about previous contributions of others was provided.

[c] Complete information means subjects know the *ex ante* distribution of possible types.

[94] This is true of Dawes, McTavish, and Shaklee (1977), Isaac, Walker, and Thomas (1984), and some of Marwell and Ames (1979).

of experiments women tend to be more cooperative than men and have a higher variance of choices." But they also note that "after 25 periods these differences vanish." In the middle, finding no effect, are Isaac, McCue, and Plott (1985), Poppe and Utens (1986), and Orbell, Schwartz-Shea, Dawes, and Elvin (1992). On the other side, there is the only experiment designed specifically to isolate and identify a gender effect in a public goods experiment with more than two players. Brown-Kruse and Hummels (1992) used an Isaac-Walker design with N=4 and MPCR's of .3 and .5. They also varied a condition they called "community", a group identity phenomena discussed further in Section 3.4.2. They found first that there were no significant differences in the way that men and women responded to the community or multiplier (MPCR) treatments, nor in the way they contributed by period. But they also found significant gender differences in contribution rates. "...males contributed at higher rates than did women" (p.12). Men's initial contributions rates are higher but their comparative statics are the same. So are there gender differences? I think the question remains open.[98]

3.4.2 Systemic

In this section we consider three explanatory variables that may be important determinants of cooperative behavior but which are difficult to measure and control.

Economics Training In Marwell and Ames (1981) a tongue-in-cheek, but still provocative, question was raised: are economists the only free riders? They reported finding that contributions were significantly lower if and only if the subjects were graduate students in economics at Wisconsin. Isaac, McCue, and Plott (1985) took exception to this and used students in an undergraduate sociology course at Pasadena City College and students from undergraduate economics courses at Caltech. They found, under repetition, that "the tendency for erosion of contributions is not unique to societies populated by economists ... Our single experiment with sociology subjects yielded substantially the same results as other subject pools, including economists." I find neither set of data particularly convincing. It is not obvious what is being measured by participation in a class: Experience, training, self-selection, or propensity to contribute? Are high school, 2-year college, 4-year college, and graduate classes different? Is the effect large enough (if it exists at all) to be found across a large number of very sensitive environments? The effect of training and/or self-selection on cooperation remains a wide-open problem.[99]

[98]Robyn Dawes has suggested to me that a "wild speculation would be that men cooperate more when the experimenter is female" and vice versa. This can be tested.

[99]In research on ultimatum games, a 2-person situation, Carter and Irons (1991) find that economists are more selfish. Frank, Gilovich, and Regan (1993) have a similar finding for 2-person prisoner dilemmas. Kagel, Kim, and Moser (1992) do not support the Carter and Irons result. I know of no other work specifically designed to isolate an "economist" effect than these three, but see Schram and Sonnemans (1992) for additional work in this area.

subjects (for which they were paid). By changing the experiment so that (1) contributions are all or none and (2) the public good is provided if at least K of N contribute, it is easy to show that a subject contributes if and only if $rP_j^{K-1} \geq z^j$ where P_j^{K-1} is j's belief (probability) that exactly $K-1$ others will contribute. If z^j is randomly chosen from a *cdf* $G(\cdot)$ then at a Bayes Equilibrium[102] each expected payoff maximizing subject contributes if and only if $z_i \leq z^*$, the probability any one subject contributes is $G(z^*)$, and z^* satisfies

$$\frac{1}{r}z^* = \begin{pmatrix} N-1 \\ k-1 \end{pmatrix} G(z^*)^{K-1}(1-G(z^*))^{N-K}.$$

Palfrey and Rosenthal carefully induce the payoffs and G. In their words:

> At the beginning of each experiment, subjects were told K, N, r in "francs," ...and all other relevant information about the experimental procedures. They were also told how many cents per franc they would receive at the conclusion of the session. These values were held constant throughout an experiment. Subjects earned between \$10 and \$20 during each session. Sessions lasted between forty-five minutes and an hour and a half.
>
> In each round, subjects were each given a single indivisible "token" (endowment). Token values in franc increments between 1 and either 90 or 204 were independently drawn with replacement from identical uniform distributions and randomly assigned to subjects, and this was carefully explained to the subjects in the instructions. ...Then each subject was told the value of his or her token, but not told the values of the tokens of other subjects. Subjects were then asked to enter their decisions (spend or not spend the token).

The results were very striking. First using the predicted $z^*(K,N)$ and varying K and N, (N=3 and 4, K=1,2, and 3), one can get a prediction of subjects earnings in the Bayes equilibrium. The regression of predicted on actual yields

$$\text{actual earnings}=-.054+1.045z^* \text{ predicted with } n=33 \text{ and } R^2=.95.$$

The intercept is not statistically different from 0 and the slope is not different from 1. But individual behavior differs substantially from that predicted by the model: contribute when $z^i \leq z^*(K,N)$. Palfrey and Rosenthal consider four alternative models: biased probabilities, risk aversion, other non-linear utility forms including altruism and the Rapoport model, and cooperation. They show that these yield different predictions about how contributions change with K and N. They then proceed to show that the data support only the hypothesis that subjects' priors about $G(z^*(K,N))$ are biased

[102]See Palfrey and Rosenthal (1991a) for the details.

Dawes (1991) they use this as one assumption in a model which purports to explain the evolution of cooperation and, presumably therefore, the tendency to cooperate in the one-shot experiments. I think these ideas deserve to be explored further especially in a way that provides more reliability in the responses to questions about beliefs. Scoring rules or payments to the subject whose predicted percentage cooperation is closest to the actual percentage might tighten up the data. It would also be interesting to see how repetition affects predictions and how prediction affects behavior.[107]

Friends, Group Solidarity Two experimentalists have tried to discover whether some form of group identity might cause contributions to increase. Both have indicated the answer is yes. Orbell, van de Kragt, and Dawes (1988) report the results of an experiment similar to the Dawes, McTavish, and Shaklee (1977) experiments described in Section 2.2. One difference was that some groups were told their contributions would provide a public good, not for those in their own room, but for a similar group in another room. Although the payoff structure is identical in both treatments, cooperation is significantly higher (almost twice as high) when the public good accrues to subjects in one's own room. The data are in Table 17. The effect is magnified by discussion although, somewhat surprisingly to me, discussion increases contributions even when the benefits go to others.[108]

Table 17. % Contributions

	Give to	
	Own Group	Other Group
No Discussion	37.5	19.6
Discussion	78.6	30.4

Brown-Kruse and Hummels (1992) also try to control for group identity by using a community versus non-community treatment. In their words:

> In the community v. noncommunity treatment, we controlled the nature of pre-experiment communication. By filling out a required questionnaire, subjects in the community setting were encouraged to meet, talk, and learn something about each other. Our goal was to arouse a sense of membership in a group. (Brown-Kruse and Hummels (1992) p.6.[109])

[107]That is, does the mere act of asking for predictions affect the rate of contribution?

[108]They provide a second set of data, which shows that the opportunity of promising may be an important part in explaining the effect of discussion. This is further discussed in Orbell, Dawes, and van de Kragt (1990).

[109]I would like to thank J. Brown-Kruse for permission to quote from this report.

the data in Table 19 we see that unanimity does increase contributions if there are no vetos but there are so few success periods (13%) that the gain in potential contributions is outweighed by the failures. This effect is very similar to the effect of increases in thresholds observed in Section 3.1. Since there is only this one study[111], one must be careful about leaping to conclusions but it seems likely that unanimity is not desirable as an institutional device to increase contributions, a fact that would have been impossible to discover with theory or field data.

Table 19.

	Efficiencies in Success Periods[a]	% Success Periods
with unanimity	57.5	13
without unanimity	32	100

[a] A success period is one in which no veto occurs.

Revision and Sequence Two other institutional variations may have a more positive effect on cooperation than unanimity. One, sequencing,[112] has been tested in a threshold environment and one, revision, has been tested across different environments including an Isaac-Walker environment and a threshold environment. They each deserve further exploration.

The idea of sequencing is not new[113] but one of the first studies of its properties in public goods environments seems to be in Erev and Rapoport (1990). Sequencing allows or requires participants to make their decisions sequentially with complete information about previous decisions in the sequence. When there is a threshold this significantly changes the theoretical properties of the game. If one applies the modern notions of subgame perfection to a game in which the monetary public good is provided if and only if K of N contribute then the theory predicts the last K in the sequence will contribute and the good will always be provided efficiently. The data lend limited support to this conclusion. Using an environment similar to van de Kragt, Orbell, and Dawes (1983) requiring 3 of 5 contributors, Erev and Rapoport found that the percentage of cooperation was essentially the same whether decisions were sequential (45.3%) or simultaneous (42.9%). However, under the sequential protocol the public good was provided 66.7% of the time

[111]There have been other mechanisms tested with unanimity. Banks et al.. (1988) also test Smith's auction process and obtain data similar to that in Tables 18 and 19. Smith et al. (1982) tested Oral Double Auctions with unanimity and found that the extramarginal units which were rationed out by the price system – as they should be – tended to veto the allocations and significantly reduce efficiencies.

[112]This variation is clearly related to sequential protocols in bargaining such as ultimatum games. See Chapter (Roth).

[113]See, for example, the work of Harstad and Marrese (1978, 1979, 1982), or Cremer and Riordan (1982).

Decision Costs Decision costs are related to bounded rationality and computational and informational complexity. Generally the idea is that precise optimization carries cognitive processing costs which are traded off by subjects against rewards: The lower the rewards the more errors in computation. While Smith and Walker (1992) address some of the issues in the context of private goods, it is difficult to identify any systematic study in the context of public goods. Two papers are vaguely related. Dawes and Orbell (1982) report the results of an experiment using one of their standard dilemma designs with no threshold, with no communication and with losses truncated at zero in which they tried to check whether communication causes increases in contributions because it facilitates thinking. They allowed some subjects only 5 minutes to think about their choice and allowed others 24 hours. The results were clear and unequivocal: cooperation rates were 35.6% for 5 minutes and 35.9% for 24 hours. "Thinking time per se does not help." (p.172.) In a second study related to decision costs, Saijo and Nakamura (1992) compare rates of contribution in an Isaac and Walker type design with MPCR=.7 and 1/.7 and with N=7. They provide two different payoff tables to different subjects. One they call *rough*, similar to that provided by Isaac and Walker, provides two columns of data: "Total contributions" in increments of 10 and "Your (public good) Payoff." In the format they call *detailed* they provide a 61×11 matrix whose rows are "sum of others contributions" including all integers ranging from 0 to 60 and "your contribution" ranging from 0 to 10. The entries are "your (total) payoff." They obtain considerably different results with the detailed table than with the rough. Using the rough table and MPCR=.7, the rates of contribution and the decline with repetition mimic those in Isaac, Walker, and Thomas (1984) (see Section 2.5): more than 30% contribution early with decay towards 10%. With the detailed table "the mean investment for all ten periods is significantly less (19.6% vs. 34.1%) than the previous experiments and no specific decay toward period 10 is observed." (p.10.) It seems from Saijo and Nakamura (1992) that reducing cognitive processing costs by providing the detailed table[118] reduces contributions and eliminates the decline with repetition. This is consistent with a hypothesis that some subjects make errors (which are one-sided at 0) that they correct with repetition or with detail. This is a wide open area of research at the edge between psychology and economics. It is related to the issue of presentation raised in footnote 16. It certainly seems to me to be worth a lot more careful research.

Fairness It is often claimed that non-reward maximizing behavior arises because of subjects' concerns for fairness. There has been a lot of study or at least claims of this in bargaining experiments (see Chapter (Roth)) but very little has been done in the context of public goods. Marwell and Ames (1979) administered a survey as part of their experiment (see Section 2.3) and they suggest that the answers to that "suggests one major theme – the consideration of 'fairness' as a mediating factor in investment

[118]The detail table eliminates computation and interpolation but increases informational size from 2×11 entries to 61×11 entries. Does this increase or decrease decision costs?

example, doubling the value of each unit of endowment and doubling the value of each unit of the public good) reduces the contribution rate in dilemmas.[121] This is a matter of control.

It is obvious that subjects bring motivations beliefs and capabilities to the lab that may be vastly different from those assumed in standard game-theoretic models. Some experimental situations such as Double Oral Auctions appear to be very robust against such variations. No control is needed. Some experimental situations such as voluntary contributions mechanisms with public goods are very sensitive to such variations. That sensitivity can be controlled with high payoffs but little is learned. The hard problem is to isolate and measure the effects of the variations. This will keep experimentalists busy for a long time.

4 Final Thoughts

What do we know about behavior in public goods environments? In particular, are *subjects naturally cooperative, contributors, and altruistic?* Conventional wisdom is based on the data generated by Marwell and Ames, Dawes and Orbell, Isaac and Walker, and others in environments without thresholds. These suggest that in public goods experiments where the dominant payoff maximizing strategy is to give nothing and where the group optimum is to give everything, in one-shot decisions or in the early rounds of repetitive decisions contributions from 30% to 70% occur.[122] There are at least two explanations for the data: (a) subjects trade off altruistic and cooperative responses against personal payoffs or (b) subjects make mistakes, do not care, are bored, and choose their allocations randomly. How can we tell the difference? Let us look at four recent papers which, I think, provide a clue. Two use environments which retain a dominant strategy feature but test the hypothesis of natural cooperation by eliminating the conflict between group and self-interest.[123] Two others study an environment with an interior Nash and interior social optimum so mistakes can be made by both contributing too much and contributing too little.[124]

In Palfrey and Prisbrey (1993) and Saijo and Nakamura (1992) each subject faces an Isaac-Walker type payoff of $u^i = p^i(z - t_i) + b^i(\sum_j t_j)$. Sometimes $b^i < p^i < Nb^i$, so self-interest suggests $t_i = 0$ and group interest suggests $t_i = z$. But sometimes $p^i < b^i$ so both group and self regarding behavior would suggest $t_i = z$. Palfrey and Prisbrey use an asymmetric information environment in which each subject has a different value

[121]See, for example, Marwell and Ames (1980), Palfrey and Prisbrey (1993), and McKelvey and Palfrey (1992), who test this hypothesis directly.

[122]See, for example, Table 9.

[123]See Andreoni (1993a) and (1993b) for additional work like this.

[124]In Dawes et al., Marwell et al., and Isaac et al., etc. the dominant strategy was $t = 0$. Only mistakes such that $t > 0$ are possible.

Another approach to separating errors from altruism places the non-cooperative equilibrium in the interior of $[0, z]$ and separates that equilibrium from the group optimum. Both Andreoni (1993b) and Walker, Gardner, and Ostrom (1990) do this by introducing income effects.[128] Andreoni (1993b) wanted to study whether government funding of the public good would crowd-out private contributions. He recognized that to do so required an environment with an interior non-cooperative equilibrium. He created an environment in which an individual's payoff is $u = \alpha ln(z - t_i) + (1 - \alpha) ln(y)$, $y = \Sigma_i t_i$, and $0 \leq t_i \leq z_i$. The first thing to note about this world is that the non-cooperative equilibrium (that generated by perfectly selfish game-theoretic behavior) is

$$t^* = \frac{(1-\alpha)}{1+\alpha(N-1)} z$$

so that for $0 < \alpha < 1$, $0 < t^* < z$. The second thing to notice is that the marginal per capita return (MPCR) to contributing is

$$(u_y^i / u_x^i) = \frac{1-\alpha}{\alpha} \cdot \frac{x}{y} = \frac{1-\alpha}{\alpha} \frac{z-t}{y}$$

which is not constant in z. This is what is meant by income effects. At the non-cooperative equilibrium,[129] t^*, $MPCR = 1$, so if the subjects' cooperative nature is similar to that in the linear world of Isaac and Walker, we should expect to see contributions greater than t^*. If everyone is symmetric, we can identify a group optimum[130] as that $\hat{t}$ which maximizes $\alpha ln(z - t) + (1 - \alpha) ln Nt$. Thus $\hat{t} = (1 - \alpha)z$. Notice that, for $0 < \alpha < 1$ and $N > 1$, $0 < t^* < \hat{t} < z$, and the MPCR at $\hat{t}$ is $\frac{1}{N}$ for all subjects.[131] With this design it is possible for an experimentor to manipulate t^* and $\hat{t}$ to see whether subjects respond or not. Andreoni's data suggest that they do. Although he only used one set of parameters with $z = 7$, $t* = 3$, and $\hat{t} = 6$, contributions averaged 2.84 over a number of periods and were bounded between 2.11 and 3.33 in each period. This is clearly near the non-cooperative equilibrium, is less than altruism would suggest, and is nowhere near the optimum. Although I have not analyzed these data to separate out the percentage of Nash players, this is certainly additional evidence supporting the conventional wisdom that average rates of contribution are 50% may be the unintended result

[128]A simple theoretical exercise which would provide an interesting environment for an experiment is to determine an environment where every subject has a dominant strategy to contribute t_i^* where $0 < t_i^* < z$ and where the group optimum t^o is such that $t^o \neq \sum_{i=N}^{N} t_i^*$.

[129]For any utility/payoff functions the MPCR, for all players, will equal 1 at an interior Nash equilibrium.

[130]If there are asymmetries and either $x^i \neq x^j$ or $z^i \neq z^j$ then it is not clear what a group optimum is. Instead, there are many Pareto-optima. If the subjects are maximizing their total take then the best function to maximize is $\Sigma_i u^i$. But this may leave some subjects very badly off.

[131]As N grows, t goes to zero while $\hat{t}$ stays constant and the MPCR at $\hat{t}$ goes to 0. To get some idea of the strength of the incentives consider $\alpha = \frac{2}{3}$ and $N = 10$. Then, $\frac{t^*}{z} \cong 5\%$, $\frac{\hat{t}}{z} = 33\%$, and $MPCR(\hat{t}) = .10$. To make it possible to keep MPCR at $\hat{t}$ constant in N one must use a CES utility function.

each other. (3) *Altruism or group-regarding preferences cannot explain the data.* When the conflict between group interest and self-interest is removed, subjects still contribute in ways that are counter to both their self interest and their group interest. (See Saijo and Nakamura (1992).) Up to 50% of the subjects appear to be solely self-interested when they understand the experimental situation.[134] (See Palfrey and Prisbrey (1993).) Further, experience, repetition, better detail in payoffs, and information about heterogeneity reduce the apparent altruistic instinct of 30-40% of other subjects. (4) *It is possible to provide an environment in which at least 90% of subjects will become selfish Nash-players.* Heterogeneous payoffs and resources, complete and detailed information particularly about the heterogeneity, anonymity from others and the experimenter, repetition and experience, and low marginal payoffs will all cause a reduction in rates of contribution especially with small numbers. Add unanimity to the mechanism and rates will go to zero. (See Banks, Plott and Porter (1988).) It is possible to extinguish any trace of "altruism" in the lab. (5) *It is possible to provide an environment in which almost all of the subjects contribute towards the group interest.* Homogeneous interest, little or rough information, face to face discussions in small groups,[135] no experience, small numbers and high marginal payoffs from contributing will all cause an increase in contributions. Why and how often this all works remains a mystery. (6) *There appear to be three types of players:* dedicated Nash players who act pretty much as predicted by game theory with possibly a small number of mistakes, a group of subjects who will respond to self-interest as will Nash players if the incentives are high enough but who also make mistakes and respond to decision costs, fairness, altruism, etc., and a group of subjects who behave in an inexplicable (irrational?) manner. Casual observation suggests the proportions are 50%, 40%, 10% in many subject pools. We, of course, need a lot more data before my outrageous conjectures can be tested.

Let me finish with one pessimistic and one optimistic observation from the point of view of the mechanism designer. My pessimistic remark is that although inexperienced subjects can be led to provide large contributions in one-time decisions with the use of relevant discussions, one cannot rely on these approaches as a permanent organizing feature without expecting an eventual decline to self-interested behavior. Thus, for example, techniques such as TQM (Total Quality Management), political orations, and half-time speeches can have at best a transitory effect in calling upon the altruistic impulses of some. Ultimately self-interest takes over. My optimistic remark is that since 90% of subjects seem to be responsive to private incentives, it will be possible to create new mechanisms which focus that self-interest towards the group interest. We need not rely on voluntary contributions approaches but can instead use new organizations such as those found in Smith (1979a), Groves and Ledyard (1977), or Ledyard and Palfrey (1992). Experiments will provide the basic empirical description of behavior which must

[134]Even Isaac, Walker, and Williams find 38% Nash-behavior in their large group – no money experiments by round 10.

[135]One unanswered question is how or whether this works in large ($N \geq 40$) groups.

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