W09-7 August 10, 2009

Experimental Contributions to Collective Action Theory

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Word Count: 7,514

Acknowledgments

The authors wish to acknowledge financial support from the National Science Foundation and from the Workshop in Political Theory and Policy Analysis at Indiana University. Suggestions and comments from Roy Duch, Arthur Lupia, and Adam Seth Levine are appreciated, as well as the editing assistance of David Price.

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1 Introduction

Collective action problems are difficult problems that pervade all forms of social organization, from within the family, to the organization of production activities within a firm, and to the provision of public goods (PG) and the management of common-pool resources (CPRs) at local, regional, national, and global scales. Collective action problems occur when a group of individuals could achieve a common benefit if most contribute needed resources. Those who would benefit the most, however, are individuals who do *not* contribute to the provision of the joint benefit and free ride on the efforts of others. If all free ride, however, no benefits are provided.

Political scientists trying to analyze collective action problems have been influenced by a narrow, short-term view of human rationality combining an all-powerful computational capacity, on the one hand, with no capability to adapt or acquire norms of trustworthiness and fair contributions to the provision of collective benefits, on the other. To provide public goods, it is thought that governments must devise policies that change incentives to coerce citizens to contribute to collective action.

Formal analysis of collective action problems has been strongly affected by the pathbreaking work of Mancur Olson (1965) on *The Logic of Collective Action* and the use of game theory (e.g., see R. Hardin 1982; Taylor 1987), which improved the analytical approach to these problems. By replacing the naive assumptions of earlier group theorists (Bentley 1949; Truman 1958) that individuals will always pursue common ends, these modes of analysis force analysts to recognize the essential tensions involved in many potential social interactions. Using the same model of individual behavior used to analyze production and consumption processes of private goods to examine collective action problems was an essential first step toward providing a firmer foundation for all types of public policy. The empirical support for these predictions within a competitive market setting gave the enterprise an initial strong impetus.

Homo economicus has turned out to be a special analytical tool rather than the general theory of human behavior. Models of short-term material self-interest have been highly successful in predicting marginal behavior in competitive situations in which selection pressures screen out those who do not maximize external values, such as profits in a competitive market (Alchian 1950; Smith 1991), or the probability of electoral success in party competition (Satz and Ferejohn 1994). Thin models of rational choice have been less *successful* in explaining or predicting behavior in one-shot or finitely repeated social dilemmas in which the theoretical prediction is that no one will cooperate (Poteete, Janssen, and Ostrom forthcoming, chap. 6). Research using observational data also shows that some groups of individuals do engage in collective action to provide local public goods or manage common-pool resources without external authorities offering inducements or imposing sanctions (see NRC 2002).

1.1 Cooperation in the Prisoner's Dilemma

One way to conceptualize a collective action dilemma is the Prisoner's Dilemma (PD) game shown in Figure 1. Imagine two states engage in a nuclear arms race. There are two players (the two states) who choose simultaneously to either cooperate (reduce armaments) or defect (build armaments). If only one state cooperates it will be at a strategic disadvantage. In this case, the outcome (cooperate, defect) has a payout of -1 to the cooperating state. The defecting state receives a payout of 2 because of strategic gains. If both states cooperate, each would receive a payout of 1 because they do not bear the costs of building armaments. If neither cooperates, both receive a payout of zero because neither has gained a strategic advantage. In short, states are best off if they can dupe others into cooperating while they defect, moderately well off if they both cooperate, worse off if they both defect, and in very bad trouble if they are the sole cooperator.

(Figure 1 about here)

Camerer stresses that *games* are not equivalent to *game theory*. Games denote the players, strategies, and rules for making decisions in particular interactions. Game theory, on the other hand, is a "mathematical derivation of what players with different cognitive capabilities are likely to do in games." (Camerer 2003, p.3) Collective action game theory was dominated until recently by the view of short-term rational self interest exposited by Olson (1965).

In the PD game, if there are no possibilities of enforceable, binding contracts, this view predicts defection as it is a strictly dominant strategy (Fudenberg and Tirole 1991, p.10). This prediction can change if repeated play is allowed. A long stream of political science research, led by Robert Axelrod (1984), has examined repeated PD games and found that if play is repeated indefinitely, cooperative strategies are theoretically possible. However, if there is a predetermined end to repetition, the strictly dominant theoretical strategy remains to defect in each round (Axelrod and Hamilton 1981, p.1392).

This view of human behavior led many to conclude that without external enforcement of binding contracts, or some other change in institutional arrangements, humans will not cooperate in collective action dilemmas (Schelling 1960, pp.213–214; Hardin 1968). Predictions from the PD game generalize to the CPR and PG dilemmas discussed in this chapter—little or no cooperation if they are one-shot or finitely repeated and a possibility of cooperation only in dilemmas with indefinite repetition.

The behavioral revolution in economics and political science led some to question these predictions (see Ostrom 1998). Experimental game theory has been indispensible in challenging the conventional view of human behavior and improving game theoretic predictions in collective action dilemmas. As behaviors inconsistent with predictions from the conventional view are uncovered, analysts change their theories to incorporate the anomalies. For example, some authors have examined preferences for inequality aversion (Fehr and Schmidt 1999), preferences for fairness (Rabin 1993), and emotional states (Cox, Friedman, and Gjerstad 2007) to explain behavior in collective action dilemmas (see also Camerer 2003, pp.101–113).

In the next section we briefly discuss how field evidence suggested that the conventional model did not adequately explain behavior in collective action dilemmas. The careful control possible in experiments is uniquely suited both to uncovering the precise degree of anomalous behavior and for calibrating new models. As these new models of behavior are developed they should then be subjected to the same rigorous experimental and non-experimental testing as the conventional model.

1.2 Evidence from Observational Studies

Much has been written about collective action in observational studies (NRC 1986, 2002). These studies are particularly useful for experimentalists because they provide examples of behaviors and strategies that people employ in real settings to achieve cooperation. Experiments have then made these behaviors possible in laboratory settings in order to assess exactly how effective they can be. For example, much early work indicated that people are willing to sanction non-cooperators at a personal cost to themselves (Ostrom 1990). This literature influenced experimentalists to create the possibility of allowing sanctioning in laboratory environments (Ostrom, Gardner, and Walker 1994; Fehr and Gächter 2000), which led to the development of the inequality aversion model (Fehr and Schmidt 1999). Other variables found in observational research which have influenced experimental work are: group size and heterogeneity (Olson 1965), rewarding cooperators (Dawes et al. 1986), communication and conditionally cooperative strategies (Ostrom 1990), and interpersonal trust (Rothstein 2005). By and large, these same factors appear to be important in experimental studies.

We view field and experimental work as complementary; that is, we have more confidence in experimental results because they are confirmed by field work and vice versa. While observational studies make important contributions to our understanding of what determines if a group acts collectively, they are limited in that there are a host of confounding factors that might account for the effects. Experimental work is uniquely suited to isolate and identify these effects and then to calibrate new models of human behavior.

Let us now turn to experimental contributions to collective action theory. Many experiments exist posing collective action dilemmas, including Trust Game experiments (see Wilson and Eckel this volume), Ultimatum Game experiments, and a host of others (see Camerer 2003 for a review). In this chapter, we will focus on two games that have received much attention in the experimental literature. In Section 2, we review research and contributions from PG experiments and in Section 3, we review CPR experiments. In Section 4, we discuss the emerging role of laboratory experiments in the field, and in Section 5 we conclude.

2 Public Goods Experiments

The PD game discussed in the previous section is a special case of a PG game. Suppose that instead of Cooperate and Defect, we labeled these columns Contribute and Withhold. The game is structured such that a public good is provided to all players in proportion to the number of players that contribute. Suppose that the public good can be monetized as 4 dollars per player contribution. If both players contribute, each receives 4 dollars. If only one player contributes each receives 2 dollars. Suppose further that it costs each player 3 dollar to contribute. This is the same game structure as depicted in the PD game in Figure 1.[1] We can also write the payouts from this PG game in equation form. Let $C_i \in \{0,1\}$ represent the decision to contribute, so that C_i takes the value of 1 if player *i* decides to contribute and C_i takes the value of 0 if player *i* decides to withhold. The payment to player *i* in this one-shot PD game is,

$$\pi_i = \frac{4(C_i + C_j)}{2} - 3C_i. \qquad (1)$$

If player *i* maximizes their own income, then they will select the level of C_i that maximizes Equation 1. In this case the prediction is that $C_i = 0$, because the net effect of one person's contribution is -1. Let us relax some of the assumption from Equation 1 to develop a general PG game. First, we add *n* players to the game and relax the assumption that the decision to contribute is binary. That is, instead of contributing or not contributing, suppose that the subjects can determine a specific amount to contribute, C_i. In general, C_i can be allowed to vary up to some initial endowment, and can take any value between 0 and the endowment, E_i. Let the marginal benefits to unilateral contribution be any value A_i. Let us call the costs of contributing B_i. The general PG game, then, is,

$$\pi_i = \frac{A_i}{n}\sum_{j=1}^{n} C_j - B_i C_i,$$

where

$$C_i \in [0, E_i]. \qquad (2)$$

The parameter B_i is often set to 1 and A_i is set to be the same for every player. In this case the primary characteristic used to describe the game is the ratio of marginal benefits to the number of subjects, $\frac{A}{n}$. This ratio is known as the Marginal Per Capita Return (MPCR). If $B = 1$ then A must be less than n for this to be a PG game and for this to remain a collective action dilemma. The closer the MPCR is to 1, the higher the benefits from cooperation.[2]

The experimental protocols for PG games are typically abstract, instructing subjects to allocate their endowment to a group or an individual fund. The individual fund has a rate of return equal to B. The rate of return on the group fund is equal to the MPCR. The PG game is also often referred to as a Voluntary Contribution Mechanism (VCM) because subjects make voluntary contributions to this group fund.

2.1 Baseline Public Goods Experiments

In the baseline PG experiment subjects are each endowed with the same number of tokens, receive the same MPCR, and the same rate of return to their private accounts. If one assumes that subjects behave according to narrow self-interest, then one would expect no contributions in any round of the game.

In one of the first PG experiments, Isaac and Walker (1988a) endowed each subject, in groups of four, with 62 tokens and repeated play for 20 rounds. The MPCR was $0.003 per token while the return to the private account was $0.01. The dotted line (NC-NC) at the bottom of Figure 2 shows that in the first round subjects contributed about 50 percent of their endowment to the public good. Over time these contributions steadily fall towards 0. This result is fairly robust, having been replicated in a number of studies (Isaac and Walker 1988b, p.184). This result appears to confirm the traditional model of narrow self-interest, especially if the anomalies at the beginning rounds can be attributed to learning (see Muller et al. 2008).

2.2 Communication

When participants in the CPR experiment cannot communicate, their behavior approaches zero contributions over time. Participants in most field settings, however, are able to communicate with one another at least from time to time, either in formally constituted meetings or at social gatherings. In an effort to take one step at a time toward the fuller situations faced by groups providing public goods, researchers have tried to assess the effects of communication.

Figure 2 shows the results from two additional treatments in Isaac and Walker (1988b). In the treatment C-NC, subjects were allowed to communicate at the beginning of each of the first 10 rounds but were not allowed to communicate thereafter. In the treatment NC-C, subjects were not allowed to communicate for the first 10 rounds, but starting in round 11 were allowed to communicate in every round thereafter. In the baseline treatment (NC-NC), described in the previous section, no communication was allowed in any round. Nonbinding communication is referred to as *cheap talk* and is predicted to have no effect on outcomes in the PG game (Harsanyi and Selten 1988, p.3).

(Figure 2 about here)

Figure 2 clearly shows, however, that communication has a profound effect. Take the case where communication was allowed in the last ten rounds of play, the dashed line in the left panel of Figure 2. It appears for the first 10 rounds that the subjects are on a similar trajectory as those who are never allowed to communicate; that is, mean contributions to the public good are steadily falling. After communication in round 10, however, mean contributions increase substantially. In the second half of the game, contributions are near 100 percent of the total endowments. The right panel of Figure 2 shows that the mean contributions are significantly higher in the second ten rounds. Perhaps more astonishing is that when communication is allowed in the first ten rounds, contributions continue to remain high in the second ten rounds (the solid line in the left panel of Figure 2), although this tends to taper off in the last three rounds. Still, mean contributions remain essentially the same across 10 round increments, as indicated in the right panel of Figure 2. Such strong effects of communication have been found in many studies (Sally 1995). In fact, Miettinen and Suetens (2008, p.945) have argued that, "Researchers have reached a rather undisputed consensus about the prime driving force of the beneficial effect of communication on cooperation."

Subject communication tends to focus around strategies for the game. Often subjects will agree on some predetermined behavior. While they frequently do what they promise, some defections do occur. If promises were not kept, subjects use the aggregated information on the outcomes from the previous round to castigate the unknown participant(s) who does not keep to their agreement. Subjects can be indignant about evidence of defection and express their anger openly. Not only does the content of communication matter, but the medium of communication is also important. Frohlich and Oppenheimer (1998) found that those allowed to communicate face-to-face reach nearly 100 percent contribution to the public good, while communication via email improves contributions to about 75 percent (Frohlich and Oppenheimer 1998, p.394).[3]

While the findings that communication makes a major difference in outcomes, some debate exists as to *why* communication alone leads to better results (Buchan, Johnson, and Croson 2006). A review by Shankar and Pavitt (2002) suggest that voicing of commitments and development of group identity and norms seem to be the best explanation for why communication makes a difference. Another reason may be the revelation of a participant's type which is one source of incomplete information in experimental games. For example, face-to-face communication and verbal commitments may change participants' expectations of other participants' responses. In particular, if a participant believes that other participants are of a cooperative type (that is, will cooperate in response to cooperative play) that participant may play cooperatively to induce cooperation from others. In this case, cooperating can be sustained as rational play in the framework of incomplete information regarding participant types.

2.3 Leadership

Political leadership has long been linked to the provision of public goods (Frohlich, Oppenheimer, and Young 1971). Economists have traditionally modeled leadership in PG settings as "leading by example" (Levati, Sutter, and Van Der Heijden 2007). That is, one subject is randomly selected from the experiment to be a first mover in the PG game. Other subjects are then hypothesized to take cues from the first mover. Evidence suggests this type of leadership increases cooperation in PG games (Levati et al. 2007).

Leading by example, however, seems to be a coarse operationalization of leadership. Experimental research would benefit from more thoughtful insights from political leadership theory, and these theories could be carefully examined by endowing leaders with different capabilities. For example, characteristics of Machiavelli's prince might be manipulated in the laboratory. Is it truly better to be feared than loved? Do leaders who devise punishments for those who do not contribute to the public good fare better than those that offer rewards?[4]

In addition, while much research has been conducted on the election process of political leadership (see Morton and Williams this volume), the effects of such institutions on PG provision have not been thoroughly explored. One notable exception is a recent paper by Hamman, Woon, and Weber (2008). The authors investigate the effects of political leadership by forcing groups to delegate authority to one elected (majority-rule) leader who then determines the contributions of each member to the public good. The delegate is then reelected in subsequent periods, ensuring some accountability to other group members. The authors find that under delegated PG provision, groups elect delegates that ensure the group optimum is almost always met.

3 Common-Pool Resource Experiments

Common-pool resources such as lakes, forests, fishing grounds, and irrigation systems are resources from which one person's use subtracts units that are then not available to others, and it is difficult to exclude or limit users once the resource is provided by nature or produced by humans (Ostrom et al. 1994). When access to a common-pool resource is open to all, anyone who would like to use the resource has an incentive to appropriate more resource units when acting independently than if they could find some way of coordinating their appropriation activities with others.

CPR games are different from PG games in two ways: first, the decision task of a CPR is removing resources from a joint fund instead of contributing; second, appropriation is rivalrous. This rivalry can be thought of as an externality that occurs because the payout rate from the common-pool resource depends on total group appropriation nonlinearly. Initially, it pays to withdraw resources from the common-pool resource, but subjects maximize group earnings when they invest some, but not all, of their effort to appropriate from the CPR.

The first series of CPR experiments were initiated at Indiana University to compliment ongoing field work. The series started with a static, baseline situation that was as simple as possible while keeping crucial aspects of the problems that real harvesters face. The payoff function used in these experiments was a quadratic function similar to the theoretical function specified by Gordon (1954). The initial resource endowment of each participant consisted of a set of tokens that the participants could allocate between two situations: Market 1, which had a fixed return and Market 2, which functioned like a common-pool resource so that the return was determined in part by the actions of all participants in the experiment.

Each participant *i* could choose to invest a portion x_i of his/her endowment of ω in the common resource Market 2, and the remaining portion ω - x_i is then invested in Market 1. The payoff function used in Ostrom et al. (1994) is:

$$u_i(\mathbf{x}) = \begin{array}{ll} 0.05 \cdot e & if\ x_i = 0 \\ 0.05 \cdot (\omega - x_i) + (x_i / \sum x_i) \cdot F(\sum x_i) & if\ x_i > 0 \end{array} \quad (3)$$

where,

$$F(\sum x_i) = (23 \cdot \sum_{i=1}^{8} x_i - 0.25 \cdot (\sum_{i=1}^{8} x_i)^2)/100 \quad (4)$$

According to this formula, the payoff of someone investing all ω tokens in Market 1 (x_i = 0) is 0.05*****ω. The payoff from Market 1 is like a fixed wage paid according to the hours devoted to working. Investing part or all of the tokens in Market 2 (x_i > 0) yields an outcome that depends on the investments of the other participants.

Basically, if appropriators put all of the assets into their outside option, (working for a wage rather than fishing) they are certain to receive a fixed return equal to the amount of their endowment times an unchanging rate. If appropriators put some of their endowed assets into the CPR, they received part of their payoff from the outside option and the rest from their proportional investment in the CPR. The participants received aggregated information after each round so they did not know individual actions. Each participant was endowed with a new set of tokens in every round of play. Their outside opportunity was valued at $.05 per token. They earned $.01 on each outcome unit they received from investing tokens in the common-pool resource. The number of rounds in each experiment varied between 20 and 30 rounds but participants were informed that they were in an experiment that would last no more than two hours.

The solid line of Figure 3 shows the relationship between total group investments in Market 1, the fixed wage rate, and group earnings from that market. The dashed line shows the relationship between group investments in Market 2, the CPR, and its group earnings. Wage earnings are interpreted as the opportunity costs of investing in the CPR. Total earnings, represented by Equation 3, are maximized when the CPR earnings minus wage earnings is maximized. Given the parameterization of Equation 4, this occurs at total investment in the CPR of 36 tokens.

(Figure 3 about here)

The symmetric Nash equilibrium for this finitely-repeated game (if subjects are not discounting the future and each participant is assumed to be maximizing own-monetary returns) is for each participant to invest 8 tokens in the common-pool resource for a total of 64 tokens (see Ostrom et al. 1994, pp.111–112). They could, however, earn considerably more if the total number of tokens invested were 36 tokens (rather than 64 tokens). The baseline experiment is an example of a commons dilemma in which the Nash equilibrium outcome involves substantial overuse of a common-pool resource, while a much better outcome could be reached if participants were to lower their joint use relative to the Nash equilibrium.

3.1 Baseline CPR Experiments

Participants interacting in baseline experiments substantially overinvested as predicted. At the individual level participants rarely invested 8 tokens—the predicted level of investment at the symmetric Nash equilibrium. Instead, all experiments provided evidence of an unpredicted and strong pulsing pattern in which individuals appear to increase their investments in the common-pool resource until there is a strong reduction in yield, at which time they tend to reduce their investments leading to an increase in yields. At an aggregate level, behavior begins to approach the symmetric Nash equilibrium level in later rounds.

3.2 Voting

If subjects are allowed to make binding agreements about their behavior in the CPR game they might overcome the free rider problem. People may be willing to voluntary precommit to limit the choices available to themselves in the future in order to achieve a more preferred group outcome (Elster 1977).

Ostrom et al. (1994) investigated if subjects were willing to precommit to binding contracts in the CPR game and if those contracts would produce efficient results. The authors gave groups of 7 subjects an opportunity to use simple majority rule to develop an appropriation system for themselves. In the lab, they found people moving toward a minimum winning coalition. Subjects knew the computer numbers and began to make proposals like “Let’s give all the optimal resources to computer number one, two, three, and four.” Of course, this came from somebody who was using computer number one, two, three, and four; and they zeroed out five, six, and seven. When the voting rule was changed to require unanimity, the subjects also went to the optimum, but they allocated it across the entire group.

3.3 Sanctioning

In the field, many users of CPRs do monitor and sanction one another (Coleman and Steed 2009). Engaging in costly monitoring and sanctioning behavior is not consistent with the theory of norm-free, full rationality (Elster 1989, pp.40–41).

To test if participants would use their own resources to sanction others Ostrom, Walker, and Gardner (1992) conducted a modified CPR game. Individual investments in each round were reported as well as the total outcomes.[5] Participants were then told that in the subsequent rounds they would have an opportunity to pay a fee in order to impose a fine on the payoffs received by another participant. In brief, the finding from this series of experiments was that much sanctioning occurs. Most of the sanctions were directed at subjects who had ordered high levels of tokens. Participants react both to the cost of sanctioning and to the fee/fine relationships. They sanction more when the cost of sanctioning is less and when the ratio of the fine to the fee is higher (Ostrom et al. 1992). Participants did increase benefits through their sanctioning but reduced their net returns substantially due to the high use of costly sanctions.

4 Bringing the Lab to the Field

We think laboratory experiments in field settings hold a challenging, yet potentially fruitful avenue for political scientists to investigate collective action theory. Many collective action dilemma experiments have been conducted in developed countries with undergraduate students from university settings. The initial reasons for this selected sample of participants were their accessibility, control for the experimenters, and lower overall costs.[6] Experiments have now been conducted with non-student populations and with more salient frames of the decision tasks, and there are often striking differences in behavior across these populations (Henrich et al. 2004).

Because of the increased costs and logistical problems associated with these types of experiments, researchers should think carefully about the reasons for extending their research to field settings. Harrison and List (2004) argue that key characteristics of subjects from the experimental sample need to match the population for which inferences will be generalized. That is, if age, education, or some political or cultural phenomenon unique to students are not key characteristics of the theory being tested, then a student sample may be appropriate to test the theory. On the other hand, if one wishes to investigate the effects of Communism, for example, then a sample of U.S. students would not be appropriate; an older age sample from a post-Communist country would be needed for the experiment (Bahry et al. 2005).

4.1 Ethnic Diversity and the Mechanisms of Public Goods Provision

Habyarimana et al. (2007) were interested in why ethnic heterogeneity leads to decreased investments in public goods. In order to test a number of possible mechanisms, the authors conducted a set of surveys and experiments using 300 randomly selected subjects recruited from a slum in Kampala, Uganda. It was necessary to use such subjects because "ethnicity is highly salient in everyday social interactions" and the subjects had almost exclusive responsibility for supplying local public goods (p.712).

The authors identified three different potential mechanisms. First, they tested the effects of ethnic heterogeneity of tastes—the extent to which different ethnic groups care about different types of public goods and their preferences that public goods are provided to their own ethnic group and not others. Using a survey instrument the authors found that there was little difference in tastes both as to which types of public goods subjects preferred (drainage, garbage collection, or security) or to the means of their provision (government versus local). The authors then had subjects play an anonymous dictator game to test if non-co-ethnic pairs have different tastes for income distribution than co-ethnic pairs and found that this was not the case. In the dictator game a subject is given some sum of money and is simply asked to divide the money with a partner. The subject can give all, none, or anything in-between.

Second, they tested the effects of technological advantages of homogeneous groups. Such groups can draw on common language and culture to produce public goods and are better able identify non-cooperative members. To test the first proposition, that co-ethnics work well together, the authors had subject pairs solve puzzles. They found that while co-ethnic pairs were more likely to solve the puzzle than non-co-ethnic pairs the difference was not significant. The second sub-mechanism is that members of homogenous groups can find and identify non-cooperative members through social networks. The authors had subjects locate randomly selected non-experimental subjects as “targets” in Kampala. Those of the same ethnicity as their target found the target 43 percent of the time while those of different ethnicities found the target only 28 percent of the time.

Third, ethnicity might serve to coordinate strategies through social sanctioning. To test this mechanism the authors had subjects play a non-anonymous dictator game. The authors reason that in such a game if subjects give nothing to their partner they might still be subject to social shame for acting non-cooperatively. The authors found that certain types of subjects discriminate their giving based on ethnicity when play is not anonymous. That is, they give less to non-co-ethnics than they do to co-ethnics.

In this study experimental methods allowed the researchers to carefully parse out and test different causal mechanisms which explain why ethnically heterogeneous groups provide fewer public goods than homogenous groups. Experimental methodology was needed to explore these mechanisms, as all three seem equally plausible when analyzing observation data. In addition, the field setting allowed the authors to test these theories with samples from a population where ethnic diversity was a major factor in public goods provision.

4.2 Social Norms and Cultural Variability in Common-Pool Resources

A very interesting series of replications and extensions of CPR experiments has been conducted by Juan-Camilo Cardenas (2000) and colleagues using field laboratories set up in villages in rural Colombia. The villagers that Cardenas invited were actual users of local forests. Cardenas wanted to assess whether experienced villagers, who were heavily dependent on local forests for wood products, would behave in a manner broadly consistent with that of undergraduate students in a developed country.

The answer to this first question turned out to be positive.[7] Cardenas asked villagers to decide on how many months a year they would spend in the forest gathering wood products as contrasted to using their time otherwise. Each villager had a copy of an identical payoff table. In the baseline, no-communication experiments, Cardenas found a pattern similar to earlier findings from the baseline CPR experiments. Villagers substantially overinvested in appropriation from the resource.

Face-to-face communication enabled the villagers to increase total earnings on average from 57.7 percent to 76.1 percent of optimal. Subjects filled in surveys after completing the experiments; Cardenas used these to explain the considerable variation among groups. He found, for example, that when most members of the group were already familiar with resources they used the communication rounds more effectively than when most members of the group were dependent primarily on individual assets. Cardenas also found that "social distance and group inequality based on the economic wealth of the people in the group seemed to constrain the effectiveness of communication for this same sample of groups" (Cardenas 2000, p.317; see also Cardenas 2003). In five other experiments, they were told that a new regulation would go into force mandating that they should spend no more than the optimal level of time in the forest each round (Cardenas, Stranlund, and Willis 2000). Subjects were told that there would be a 50 percent chance that someone would be monitored each round. The experimenter rolled a dice in front of the participants each round to determine whether the contributions of any participant would be monitored. If an even number appeared, someone would be inspected. The experimenter then drew a number from chits numbered between 1 and 8 placed in a hat to determine who would be inspected. Thus, the probability that anyone would be inspected was 1/16 per round—a low but realistic probability for monitoring forest harvesting in rural areas. The monitor checked the investment of the person whose number was drawn. A penalty was subtracted from the payoff of anyone over the limit and no statement was made to others as to whether the appropriator was complying with regulations or not.

The participants in this experiment with a rule to withdraw the "optimal" amount imposed on them actually *increased* their withdrawal levels in contrast to behavior when no rule was imposed and face-to-face communication was allowed. Thus, participants who were simply allowed to communicate with one another on a face-to-face basis were able to achieve a higher joint return than those who had an optimal but imperfectly enforced external rule imposed on them.

4.3 Some Considerations

While investigating the differences in experimental behavior across societies holds the potential for important new insights into collective action theory, one would be remiss without mentioning some of the ethical concerns attendant to such research. Generally payments for participation in these experiments are large compared to local wage rates. Average payments in these games generally range from one-half days wage to a week's wage, although in some instances the stakes are even much greater. Researchers should consider both benefits that subjects receive from participating, but also the potential for conflict if some subjects are dissatisfied with the results. Every effort should be taken to ensure that earnings remain anonymous.

5 Conclusion

While much important work has already been done in collective action experiments, interesting questions remain. It is perhaps not surprising that considerable variation in behavior is recorded in these experiments across different societies. However, what is unclear is explaining the cultural and political dimensions driving these differences. Political scientists can make important contributions to understanding such behavior by reference to variation in political phenomenon at the local and national level. Political corruption, for example, may be very important for determining why subjects in some societies are more cooperative than subjects in others (Rothstein 2005).

Important advances can also be made in understanding the role of different political structures and the incentives they provide in CPR and PG games. We do not understand, for example, what effect different voting rules have on the propensity to delegate punishment authority or allocative authority and what effects this may have on cooperation. Research has yet to be done that examines the effects of oversight, a third-order collective action dilemma, on the propensity to sanction and the subsequent collective action outcome. There is still much work to be done examining the role of different institutional arrangements on collective action.

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		Player 2	
		Cooperate	Defect
Player 1	Cooperate	(1,1)	(-1,2)
	Defect	(2,-1)	(0,0)

Figure 1: A Prisoner's Dilemma Game

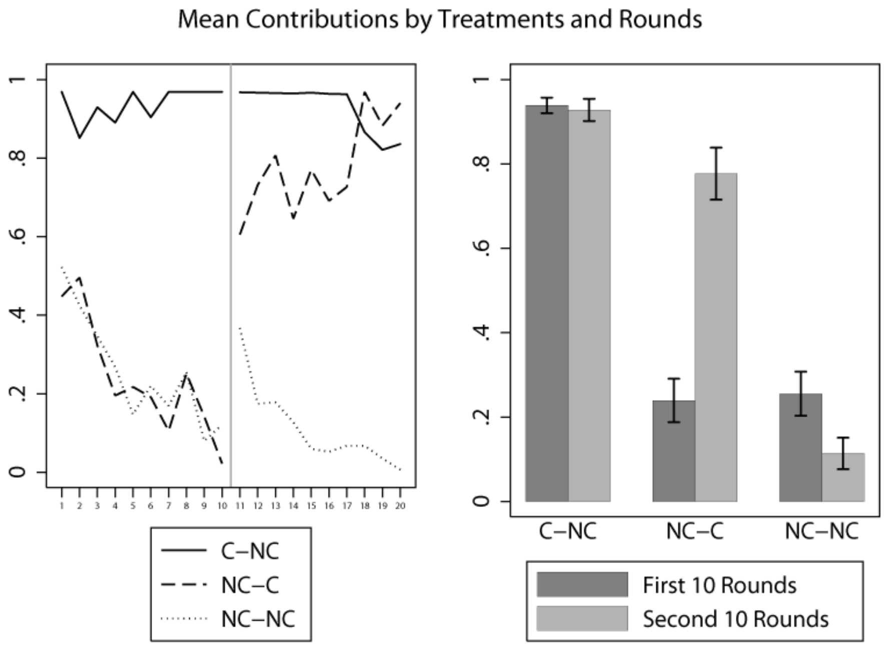

Figure 2. Contributions in a Public Goods Game

Source: Data from Issac and Walker (1988a).

The left panel shows the proportion of contributions to the group fund in a PG experiment, by round, for three treatments. Rounds were broken into two halves of 10 rounds each. In the C-NC treatment, communication was allowed in the each of the first 10 rounds and no communication in the last ten rounds. In the NC-C treatment, no communication was allowed in any of the first ten rounds, but communication was allowed in each of the last ten rounds. In the NC-NC treatment no communication was allowed in any round. The right panel shows the proportion of contributions to the group fund by halves of the experiment for each treatment, as well as 95 percent confidence intervals for those means.

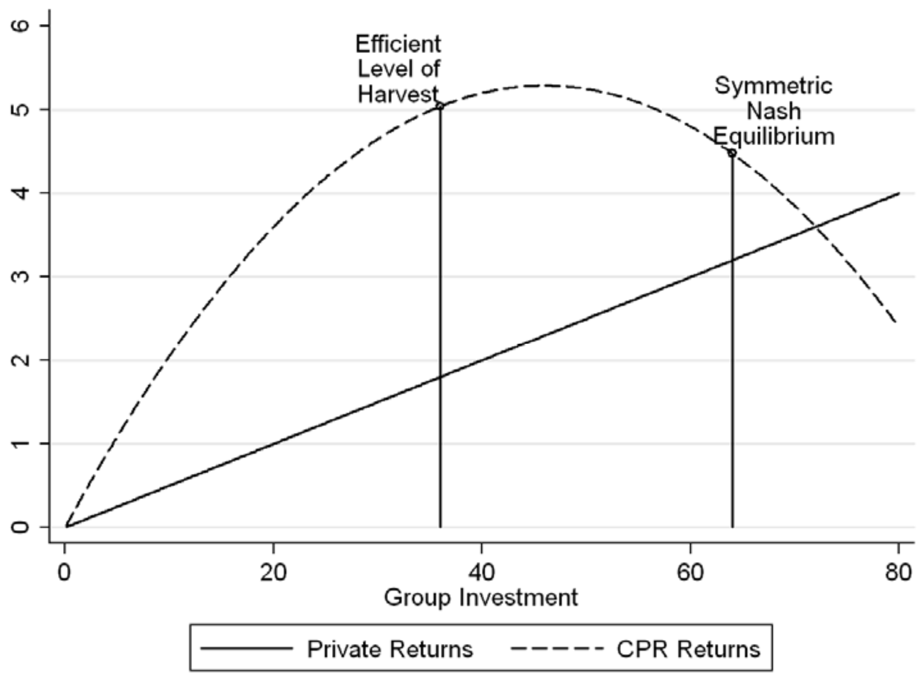

Figure 3. A Common-Pool Resource Game

The dashed black line represents total group earnings from the CPR for all levels of group investment and the solid black line represents earnings from the private fund. Earnings from the private fund are the opportunity cost of earning from the CPR. The efficient level of investment in the CPR, i.e., that maximizes group earnings from the CPR (CPR earnings minus the opportunity costs from the private fund) is a group investment of 36 tokens (see Ostrom et al. 1994). Note that net earnings from the CPR are negative when group investment equals 72 tokens. The symmetric Nash equilibrium is to invest 8 tokens each, or 64 total tokens, in the CPR.

Notes

[1] If both players contribute they each receive 1 dollar (4 dollars from the public good minus 3 dollars of contribution costs). If only one player contributes, that player receives -1 dollars (2 dollars from the public good minus 3 dollars of contribution costs), but the noncontributing player receives 2 dollars (2 dollars from the public good and no expense incurred from contributing). If neither player contributes, both receive 0 dollars.

[2] See Isaac and Walker (1988b) for results related to changing the relative size of the MPCR.

[3] See Sally (1995) for meta-analysis relating communication treatments to cooperation in experimental games.

[4] For the PG game, see Sefton, Shupp, and Walker (2007) and for the CPR game see van Soest and Vyrastekova (2006).

[5] See Fehr and Gächter (2000) for an application in PG games.

[6] While laboratory experiments conducted in a university setting usually pay participants more than they would earn in a local hourly position, the costs of the experiment itself are substantially less than experiments conducted in field settings.

[7] Although, see Henrich and Smith (2004) for a counterexample.