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Testing for Effects of Cheap Talk in a Public Goods Game with Private Information*

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We investigate a game where player endowments are private information. If two of the three players contribute their endowments, a "public" benefit is paid to all three players. In one treatment, there is a single move with simultaneous decisions. In a second, cheap talk treatment, players may send binary messages prior to the decision move. Experimental data strongly support the equilibrium model for the first treatment. The results are mixed for the cheap talk treatment. While subjects condition heavily on the messages they receive, message behavior is less systematic. *Journal of Economic Literature* Classification Numbers: 026, 215. © 1991 Academic Press, Inc.

1. INTRODUCTION

Purposive communication by an individual in a strategic setting is directed at influencing the behavior of others. One type of communication that seems particularly interesting involves the sending and receiving of

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language messages that have no direct link to the benefits and costs of decisions and outcomes of the strategic environment. In theory, such "cheap talk" (Farrell, 1986) communication can benefit all parties in settings in which the conflict is not too severe (Crawford and Sobel, 1982) and in which the potential gains to coordinated behavior are substantial. The research reported here represents a first attempt to assess these theoretical ideas by quantitatively measuring the effects of communication in a simple public goods game with cheap talk.[1]

The basic approach is to compare a game in the usual sense with an augmented version of the game in which some additional strategies are added which have no apparent effect on outcomes and payoffs. A simple example involves "replay," whereby a game is played twice—one time where the decisions result in no payoffs but in which players may observe others' "hypothetical" decisions, and then a second time for "real." The question is whether the practice round can have *equilibrium* effects[2] on the subsequent real play.

The equilibrium effects of expanding the strategy space of the players in this seemingly innocuous way can have very significant implications. This basic insight has been exploited recently by Matthews (1989) and Matthews and Postlewaite (1989) in the context of political rhetoric and auction-style bargaining, by Farrell (1986) and Farrell and Saloner (1988) in studying externalities associated with technological adoption and standardization, by Green and Laffont (1985) in their work on posterior implementable incentive contracts, and by Ordeshook and Palfrey (1988) in their model of agendas and straw votes.

In this paper, we look at a particularly simple game in which cheap talk changes the equilibrium set. We both develop a theoretical model of how replay can have real effects on behavior and outcomes and report a series of controlled laboratory experiments to study the empirical significance of this form of cheap talk. The game involves the production of a discrete public good by soliciting voluntary contributions from members of a group who will benefit from the public good. The production technology is such that one unit of input is required from at least some fraction of the group. The minimum number of inputs needed to provide the good is referred to as the threshold. As the inputs are discrete and nonrefundable,

[1] Subsequent experimental evidence on cheap talk in the battle of the sexes game is presented in Cooper *et al.* (1989).

[2] Equilibrium effects refer to effects which consider only fully rational "Nash" equilibrium behavior by the players. Obviously there can be other effects which have to do with the players becoming acquainted with the rules of the game. The fact that we are not considering these "learning" and "practicing" effects does not mean that we consider them trivial or unimportant. We merely wish to try to isolate the strategic equilibrium effects in order to better understand them.

they may be thought of as being a fixed contribution of time or effort to some common goal. We assume that sidepayments are not possible.

The coordination problem in this game is twofold. First, there is a weak incentive to free ride in the sense that each member of the group would prefer the other members to supply the necessary input for the public good. This free riding incentive is reinforced by the no-refund aspect of the public good: if an insufficient amount of input is contributed, those who have contributed will be worse off than they would have been had they done nothing. On the other hand, an individual whose private benefit from the public good exceeds the opportunity cost of his unit of the input has an incentive to contribute when he is pivotal, that is, when his contribution will "make or break" reaching the threshold.

The second aspect of the coordination problem follows from the presence of incomplete information and heterogeneous preferences. The ex ante efficient solution is for just enough individuals to contribute their unit of the input as is needed, and *for the contributors to be the ones with relatively low opportunity costs of the inputs while the free riders are the ones who have relatively high valuations for the input.* Without communication, it is impossible for players to know who has relatively high valuations and who has relatively low ones. Thus the "efficient" outcome can occur only by chance. However, with sufficient communication, it is at least *feasible* to coordinate decisions in a way that produces this desired outcome. The problem of course is that adverse strategic incentives interfere with the achievement of efficiency: perfect coordination is not Bayesian incentive compatible. Members of a group will tend to overstate their opportunity costs hoping that other members will "pay the bill."

This paper was largely motivated by previous experimental studies. In public goods games similar to the one we study, general, unstructured discussion and communication invariably produce better outcomes than the same games conducted without communication. (See, for example, Van de Kragt *et al.* (1983).) An unfortunate feature of these studies is that they consistently use a form of communication that is too complicated and unwieldy for theoretical modeling.[3] There is an enormous strategy space in these earlier experiments where communication uses the entire English language and speaking order is entirely endogeneous and occurs in continuous time.[4]

[3] Both Crawford and Sobel (1982) and Farrell (1986) have analyzed equilibrium models which allow for arbitrary messages in "sender–receiver" games. It is not clear how to extend their models to environments where everyone may send and/or receive arbitrary messages in arbitrary order.

[4] Exceptions to this include Ferejohn *et al.* (1982) and Smith (1980), both of which examine very structured cheap talk environments with public goods. Unfortunately what is gained in simplicity of the strategy space is lost in added complexity of the production technology (continuous or multiple public goods).

Our approach is to examine the simplest form of communication: each player may communicate exactly one "bit" of information. Of course in the context of contribution games where each decision in the real game is a binary one, this is formally equivalent to a one-replay version of the game.

In the absence of communication, our major experimental finding is that behavior closely approximates the Bayesian equilibrium predictions with one exception. Subjects contribute slightly more often than predicted. This is consistent with findings from others' experiments which we analyzed in Palfrey and Rosenthal (1988). With communication, behavior in the message stage of our experiments was less systematic. Message announcements appear to fit a Bayesian model poorly. Nonetheless, subjects conditioned on the announcements as if the messages were transmitting real information. A related finding from our experiments is that communication failed to provide more efficient outcomes.

The model for threshold public goods with communication and voluntary contribution is presented in Section 2. Section 3 analyzes the special case used in the experimental design. Section 4 gives the details of the experimental design and formally states several hypotheses about individual and group behavior in these experiments. The results are in Section 5.

2. The Model

The group consists of N persons. A group project requires at least w units of input. Each group member is endowed with one indivisible unit of input, which may be either consumed of "contributed" to the production of the project. The project succeeds if and only if at least w units are contributed. The value of the project to any individual is normalized to equal 1. The private value of the endowed unit of input to an individual is denoted c_i. Each person knows his or her own c_i but only knows that the other players' c's are independent random draws from some common probability distribution with CDF $F(\cdot)$. We assume there exist $\underline{c}$, $\bar{c}$, with $0 \leq \underline{c} \leq \bar{c}$, $F(\underline{c}) = 0$, $F(\bar{c}) = 1$, $f = F'$ exists and is continuous and strictly positive on $[\underline{c}, \bar{c}]$. The utility for player i with cost c is given by:

$1 + c$	if i does not contribute and at least w others contribute
c	if i does not contribute and fewer than w others contribute
1	if i contributes and at least $w - 1$ others contribute
0	if i contributes and fewer than $w - 1$ others contribute.

A. *The Voluntary Contribution Game without Communication*

A natural benchmark "mechanism" is one in which individuals either contribute or do not and make their decisions independently and simultaneously. As shown in Palfrey and Rosenthal (1988), the symmetric Bayesian equilibria to this game are of a particular simple form. For any beliefs that player i has about the other players' decision to contribute, there is a unique best response strategy which is a *cutpoint rule*. That is, there is a threshold cost level, call it c^*, such that contribution is optimal if $c_i < c^*$ and noncontribution is optimal if $c_i > c^*$. While there may be more than one equilibrium value of c^*, very mild regularity conditions on $F(\cdot)$ guarantee existence of at least one such value. The set of all such values is the set of all solutions (in c^*) to the equation

$$c^* = \binom{N-1}{w-1} (F(c^*)^{w-1}[1 - F(c^*)]^{N-w}). \tag{1}$$

The interpretation of Eq. (1) is that a person with a private cost of c^* faces an opportunity cost of contributing equal to the expected gain from contributing. At equilibrium, everyone with a cost below c^* is better off contributing, given others are using the c^* decision rule and everyone with private costs greater than c^* is better off not contributing. Individuals with a cost of exactly c^* are indifferent between contributing and not contributing. This implies Eq. (1), which may have multiple solutions for c^*. In fact if $w > 1$, $c^* = \underline{c}$ is always an equilibrium and there usually exists at least one $c^* > \underline{c}$ as well.[5]

There are also some asymmetric equilibria which may occur for some distributions F and some parameters, w and N. An example of such an equilibrium would be one in which a particular subset (say players numbers 1 through w) *always contribute regardless of their cost* (i.e., $c^* = \bar{c}$ for these players) and the other members of the group never contribute ($c^* = \underline{c}$). This is possible as long as $\bar{c} < 1$. We do not consider these (or other) asymmetries.

B. *The Game with Communication*

With communication, additional equilibria arise. Outcomes which would correspond to "correlated" equilibria to the game without communication become feasible because players can use their joint messages as a correlating device.[6] To analyze this communication game, one first

[5] In our experiment, there is a unique solution for $c^* > \underline{c}$.

[6] See Forges (1986) and Myerson (1986) for a formal statement of the relationship between correlated equilibria and equilibria with communication.

must specify the communication technology. Second, one must model an equilibrium in communication in addition to an equilibrium in the contribution subgame. Third, one must specify how players make inferences from the messages communicated by others. Finally, to solve for an equilibrium, these inferencing rules used by the players must be consistent with the communication equilibrium, and the equilibrium to the contribution subgames must be appropriately conditioned on these inferences.

The complicated aspect of strategic play is that the inferencing rules used by players and their optimal communication strategies interact. Moreover, rather than this interaction being tied down somehow by costs of communication which differ across types, the relative costs and benefits of communicating different messages are entirely endogenous and are tied down only on beliefs and expectations.

First, we specify a communications technology as follows. Individuals may costlessly make a *single* announcement from a two-element set. All announcements are made simultaneously.[7] We call the first announcement (message) **I** and the second announcement **NI**.

The communication game has two stages. In the first stage, each player announces either "**I**" or "**NI**." In the second stage, with everyone having observed the announcements of all other players in the first stage, the voluntary contributions game is played out.

This models a particular communication technology, but it is meant as a *model* of communication not a completely accurate description of all details of what is normally thought of as a conversation or a group discussion. Allowing persons to communicate a second message after observing others' communications, or modeling the messages as being sequential instead of simultaneous, are obvious possibilities for a more detailed model of communication. However, we abstract from these possibilities in order to present a simple model which captures the following essential features of pure communication:

(1) Communication is *costless*. Players' payoffs are not directly a function of their communication decisions. Their messages may indirectly affect their payoffs by influencing other players' contribution decisions, but there is no *direct* effect.

(2) Communication *precedes* decisions. If this were not the case, subjects could not condition on other players' messages.

(3) Individual announcements (or some statistic of these announce-

[7] The concept of a communication technology always implicity assumes that there is some way to limit the messages that can be sent between players. Ideally, we would want to define an equilibrium relative to an arbitrary communication technology, in the sense that the equilibrium did not depend heavily on the particular choice of technology. This much harder problem is not addressed in this paper.

ments) are *transmitted* to other members of the group. Depending on the specific communication technology, different members of the group may receive different transmissions.

Equilibrium. The equilibrium concept we use is sequential equilibrium. While the set of sequential equilibria to the communication variant of a game may generally be quite large relative to the set of sequential equilibria to the noncommunication game from which it was derived, we will be focusing on a particular type of equilibrium to compare with the noncommunication equilibria to the voluntary contribution game. The problem of multiple equilibria in these communication games is serious, and we address this problem later in the paper.

We make two further assumptions about the equilibrium with communication. First, we consider only symmetric equilibria, in which all players adopt the same equilibrium strategy. Second, the wording used for these messages in the experimental instructions was "intend to spend (**I**)" and "intend not to spend (**NI**)." We will assume that players adopt a language[8] convention according to which, in any equilibrium, the probability that any type of player will contribute in the final stage, conditional on any outcome in the communication stage, is (weakly) greater if he sent the message "**I**" than if he sent "**NI**."

Consider the following strategy in the communication game. Let $\{c_c, c_0, \ldots, c_N\}$ be a set of costs. In the communication round, each player communicates the message "**I**" if and only if his private cost $c_i \leq c_c$. In the second round, if exactly $0 \leq k \leq N$ said "**I**" in the first round, then in the real contribution round each player contributes if and only if $c_i \leq c_k$. An equilibrium is a set of these costs such that the associated strategies are a sequential equilibrium of the communication game.

The rest of this section characterizes these equilibria, as a function of N, w, and $F(\cdot)$. First, let $G(c, c_c) = F(c)/F(c_c)$. Following the same argument used in the noncommunication section, given c_c, and given that exactly $k > w$ persons said "**I**" in the communication stage, a sufficient condition for $c_k \in (\underline{c}, c_c)$ to be an equilibrium in the continuation game is

$$c_k = \binom{k-1}{w-1} [G(c_k, c_c)]^{w-1}[1 - G(c_k, c_c)]^{k-w}. \tag{2}$$

If exactly $0 \leq k < w$ said "**I**" in the first round then, letting $\overline{G}(c_k, c_c) = (F(c_k) - F(c_c))/(1 - F(c_c))$, $c_k \in (c_c, \bar{c})$ is characterized by

[8] This requirement that equilibria follow a language convention is in the spirit of Farrell's (1986) notion of a neologism.

$$c_k = \binom{N-1-k}{w-1-k} (\overline{G}(c_k, c_c))^{w-k-1}[1 - \overline{G}(c_k, c_c)]^{N-w}. \tag{3}$$

If $k = w$, then $c_w \in [0, c_c\}$ must satisfy either $c_w = c_c$ or Eq. (2).

To complete the characterization of the communication equilibrium requires an equilibrium condition for the cutpoint, c_c. As before, the intuition for the cutpoint is that a c_c type must be indifferent between the consequences of saying "**I**" and the consequences of saying "**NI**." However, the source of benefits from saying one thing as opposed to the other is quite different. Rather than directly affecting the outcome, one's message affects the equilibrium behavior of the other players which in turn affects the outcomes. First, suppose that if $k = w$, then an equilibrium in the continuation game will have $c_w = c_c$.[9] In other words, if the players stumble onto an ex post efficient set of promises in the communication round, these promises are carried out in the final round. Furthermore, the marginal announcer of the message "**I**" (i.e., a c_c type) will contribute in the second stage only if $k \leq w$. The reason for this is that in the second round, the game has been transformed into a (k, w)-*noncommunication* game with $F(\cdot)$ truncated from above at c_c. For $k > w$, these games only have equilibria with $c_k < c_c$ or else $c_k = 0$. For $k < w$ equilibrium requires that $c_k > c_c$ or $c_k = 0$. With this in mind, the expected payoff of saying "**I**" for the (marginal) c_c type, if everyone else is using c_c in the communication stage and everyone (including this player) is using $(c_0, c_1, \ldots c_N)$ in the final stage, is

$$V_{\mathbf{I}}(c_c, c_0, c_1, \ldots c_N) = \sum_{k=1}^{N} \{B(k-1, N-1, F(c_c)) \cdot \Pi_{\mathbf{I}}(k; c_c, c_0, c_1, \ldots, c_N)\},$$

where

$B(a, b, c) \equiv$ binomial probability of a successes in b trials if the probability of success is c

and

$$\Pi_{\mathbf{I}}(k; \cdot) = 0 \qquad \text{if } c_k = 0$$

$$= -c + \sum_{t=w-k}^{N-k} B(t, N-k, \overline{G}(c_k, c_c)) \qquad \text{if } c_k \neq 0 \text{ and } k \leq w$$

[9] There may be other equilibria in the continuation game. See Appendix.

$$= \sum_{t=0}^{k-1-w} B(w + t, k - 1, G(c_k, c_c)) \qquad \text{if } c_k \neq 0 \text{ and } k > w.$$

Similarly, the expected value of saying "**NI**" for a c_c type is

$$V_{\mathbf{NI}}(c_c, c_0, c_1, \ldots c_N) = \sum_{k=0}^{N-1} \{B(k, N - 1, F(c_c)) \cdot \Pi_{\mathbf{NI}}(k; c_c, c_0, c_1, \ldots, c_N)\},$$

where

$$\Pi_{\mathbf{NI}}(k; \cdot) = 0 \qquad \text{if } c_k = 0$$

$$= -c + \sum_{t=w-k-1}^{N-k-1} B(t, N - k - 1, \overline{G}(c_k, c_c)) \qquad \text{if } c_k \neq 0 \text{ and } k < w$$

$$= \sum_{t=0}^{k-w} B(w + t, k, G(c_k, c_c)) \qquad \text{if } c_k \neq 0 \text{ and } k \geq w.$$

A nontrivial communication equilibrium requires that $V_{\mathbf{I}}(\cdot) = V_{\mathbf{NI}}(\cdot)$.

3. Equilibrium Using the Experimental Parameters

We now turn to the special case of $N = 3$, $w = 2$, F Uniform on [0, 1.50]. This is the parameterization of the communication game used in the experiments. It combines features of a n-prisoners' dilemma game and the n-person generalization of the game of chicken, since some players have dominant strategies not to contribute ($c > 1$), but other players would strictly prefer to contribute if exactly one other person contributed. Noting that for any $k = 0, 1, 2, 3$ there is always an equilibrium in the second stage with $c_k = 0$, we focus on the simple type of equilibrium in which $c_0 = c_1 = 0 < c_3 < c_2 = c_c$. For this equilibrium, we need only solve for two cost levels, c_c and c_3. From the equation above, after simplifying, the two equations which jointly determine c_c and c_3 are

$$c_c^2 = c_3^2 + 2(1.5 - c_c)c_c(1 - c_c)$$
$$c_3 = 2c_3(c_c - c_3)/c_c^2.$$

The unique solution is $c_c = .724$ and $c_3 = .462$.

A. *The Multiple Equilibrium Problem*

In general, and in this specific example, there are many equilibria when preplay communication is allowed, a problem we alluded to earlier in this paper. In the Appendix, for the three-person example used in the experiments we exhaustively analyze all of the *symmetric* equilibria in which players condition their contribution decisions only on the aggregate message generated in the communication stage. Thus we characterize the symmetric pure strategy equilibria as a set of five cutpoints, $\{c_c, c_0, c_1, c_2, c_3\}$. While the entire derivation of these equilibria is too long to go through here, we state a few of the main results.[10]

1. $c_k = 0 \; \forall \; k$ is an equilibrium. This is exactly the same as the "bad" equilibrium in the noncommunication game.

2. $c_c = 1.5$, $c_0 = c_1 = c_2 = 0$, $c_3 = .375$ is an equilibrium. Similarly, $c_c = c_1 = c_2 = c_3 = 0$, $c_0 = .375$ is an equilibrium. These are exactly the same as the "good" equilibrium in the voluntary contributions game, and therefore involve no meaningful communication.

3. If there is an equilibrium for some value of c_c, then there is an "essentially equivalent" equilibrium in which the literal interpretation of the signal is reversed. These "unconventional" equilibria are ruled out.

4. $c_2 > 0$ implies $c_c < 1$.

5. With the exceptions of (2) and (3) above, every equilibrium has $c_k \leq 1 \; \forall \; k$ and $c_c \leq 1$.

6. $0 < c_c < 1$ implies $c_3 < c_c$.

7. $c_0 = 0$, with the exception of noncommunication equilibria.

8. $c_2 \neq 0$ implies $c_2 = c_c$.

9. $c_1 \neq 0$ implies $c_1 > c_c$.

Summarizing (1) through (9), we get the following intuition. First of all, there exist "babble" equilibria (Matthews, 1989), in which no information is communicated. Equilibria which are not babbling will be called "nontrivial" communication equilibria. Second, for any message space, there will be several strategies of communication in which the literal meaning of messages are permuted. In the case of this particular game, we could have equilibria in which "**I**" was translated to mean "Intend NOT to contribute" and "**NI**" was translated to mean "Intend to Contribute." In other words, in addition to a message space we must define a *language* which specifies a particular translation of the messages. This confronts game theory with a dilemma, since the notion of equilibrium is normally defined

[10] We describe the equilibria by the strategies played along the equilibrium path. The description of "off the equilibrium path" behavior is straightforward.

independently of the *labeling* of strategies.[11] However, one cannot help but conjecture that observed outcomes of these games must depend on the labeling of the strategies, since that is exactly what communicating in a language means. Third, the cutpoints in the second stage, at least if they are positive, are ordered in a natural way, with $c_3 < c_2 = c_c < c_1$. Fourth, in nontrivial equilibria, if no one promises in the first stage, then there is never any contribution in the subsequent stage.

The multiple equilibria problem presents some further difficulties. First, while the question of refinements in games with costless communication is clearly important and relevant, it is by no means resolved. Treating that issue in complete detail is beyond the scope of this paper, but is clearly an interesting and important direction for future study. Second, in our game, "refinements" such as stability (Kohlberg and Mertens, 1986) and divinity (Banks and Sobel, 1987) are not strong enough to produce a significant reduction of the equilibrium set. Consequently, we have focused on a nontrivial communication equilibrium that is both analytically tractable and superior in a welfare sense to the noncommunication equilibrium.

4. Experimental Design and Hypotheses

The experiments were conducted using 54 undergraduates at Carnegie–Mellon University and California Institute of Technology. None of the participants had prior experience in the tasks required in these experiments. Six separate sessions were run, with nine subjects participating in each session. The subjects played the noncommunication game in three of the sessions (all at Caltech) and the communication game in the other three sessions (one at CMU and two at Caltech).[12] Instructions are available from either author on request.[13]

A. *Noncommunication Sessions*

All sessions had 20 rounds. In each round, subjects were each given a single indivisible "token." Token values in penny increments between 1 to 90¢ (also referred to as costs) were independently drawn with replacement from identical uniform distributions and randomly assigned to sub-

[11] A recent attempt to endogenize the labeling of strategies has been made by Crawford and Haller (1987). This idea is also closely related to the idea of focal points introduced by Schelling (1958).

[12] The experiments reported here followed two pilot sessions conducted at CMU. The pilots ran for only 10 rounds instead of 20. In the final design payoffs and costs were scaled by a factor of 0.6 and the wording attached to the intent messages was modified slightly.

[13] The data can be obtained by mailing a request and an IBM type diskette to either author.

jects. Each subject was told the value of his or her token but told only the probability distribution of values of the tokens of other subjects. Subjects were then asked to enter their decisions (spend or not spend the token). If at least two of the three subjects spent, each subject received 60¢ if he was a "contributor." If he or she was not a contributor, the payoff was 60¢ plus the token value. The computed equilibrium values in the preceding section must be multiplied by this factor of 60 to reflect rescaling.[14] Each round subjects were assigned to a new group in a rotation sequence which minimized the number of times any two subjects were paired together in the same group and which guaranteed that no two subjects were paired in adjacent rounds.[15] The reason for doing this was to limit reputation and supergame effects which can occur with repeated play.

B. *Communication Sessions*

These sessions also had 20 rounds, but now each round was further broken down into two stages: a communication stage and a contribution stage. In the communication stage, subjects chose one of two messages: "I intend to contribute"; "I do not intend to contribute." They were advised that these messages were not binding, and they could make either contribution decision regardless of which message they sent in the communication round. After these simple messages were sent, each person was told how many members in their group sent each message, and was reminded which message he or she had sent. This was directly followed by the contribution stage, where individuals made *binding* contribution decisions.

At the beginning of each session, the experimenter read the instructions to the subjects and answered any questions about the procedures. In the session at Carnegie–Mellon, each of the 20 rounds was run by handing out private costs and collecting slips of paper on which subjects wrote their

[14] These costs were generated in advance by a standard computerized pseudo-random number generator.

[15] Subjects were simply told that they were being assigned randomly to a different group each period. A referee has correctly argued that it might have been more appropriate to communicate the fact that it was impossible for one subject to meet another in two consecutive rounds. In other experiments run prior to receiving the referee's report, we had in fact made this change in instructions. These experiments involved a number of different design changes, and are reported in Palfrey and Rosenthal (1991). The results for the games with the same structure as those of this paper were very similar to those reported here. Because of length considerations and because these experiments cannot be matched with communications experiments, these additional results are not reported in this paper. Suffice it to say that the results were robust to the combined effects of (a) sequencing with different games, (b) scaling back the dollar values of payoffs, (c) playing with 12 subjects rather than 9, (d) the change in instructions on subject pairings, and (e) using a new computer program with a different screen display.

messages (and subsequently contribution decisions) and returning slips of paper to privately inform each subject of the messages (contribution decisions) by the rest of his group. In the Caltech sessions, the procedures for distributing costs, collecting contribution decisions, and reporting outcomes were entirely computerized.[16] The communication sessions are designated $C1$ (Carnegie Mellon), $C2$, $C3$, and the noncommunication sessions $N1$, $N2$, and $N3$.

The costs assigned to subjects and the rotation of group assignments in each of the communication sessions matched exactly the costs and group assignments in one corresponding noncommunication session. This allowed us to control (at least to a limited extent) for random variation in the exact costs that were drawn.

C. *Specific Hypotheses*

The theoretical model presented in the previous section produces a number of hypotheses about (a) individual behavior, (b) aggregated outcomes, and (c) comparisons across the two treatments. We first consider individual behavior.

The task each subject is faced with is a very complicated strategic situation. This is especially so in the communication sessions, but there is also a great deal of complexity in the more basic noncommunication treatment. Therefore, we begin with a very weak hypothesis about individual behavior which does not theoretically depend upon the strategic aspect of the situation, namely that individuals do not use dominated strategies. In the context of our experiments, this reduces to:

HYPOTHESIS 1. *A subject with a cost which exceeds the benefit of* 60 *does not contribute.*

The next hypotheses address the strategic, equilibrium predictions of the theory. First, given any beliefs or expectations about the likely behavior of the other members of one's group, the optimal decision rule is *always* a cutpoint rule. For example, in the noncommunication sessions, this means a decision rule which divides costs into a low range and a high range, with contributing being the optimal response in the low range and not contributing being the optimal response in the high range. In the communication sessions, this means a set of cutpoints, $\{c_c, c_0, c_1, c_2, c_3\}$. In order to test this hypothesis, it will be necessary to jointly test the hypothesis that these cutpoints are stationary.

HYPOTHESIS 2. *Every subject uses a constant cutpoint decision rule.*

[16] There were no apparent differences between the sessions at Caltech and Carnegie Mellon.

Second, we have been assuming throughout that individuals use symmetric strategies: that is, in equilibrium, every subject uses the same decision rule to decide whether or not to contribute.

HYPOTHESIS 3. *All subjects use the same cutpoint.*

We also have a number of predictions about the numerical values of the cutpoints.

HYPOTHESIS 4. *In the noncommunication sessions,* $c^* = 22.5$.

HYPOTHESIS 5. *In the communication sessions,* $c_c = 43.4$.

HYPOTHESIS 6. *In the communication sessions,* $c_0 = 0$.

HYPOTHESIS 7. *In the communication sessions,* $c_1 = 0$.

HYPOTHESIS 8. *In the communication sessions,* $c_2 = c_c$.

HYPOTHESIS 9. *In the communication sessions,* $c_2 = 43.4$.

HYPOTHESIS 10. *In the communication sessions,* $0 < c_3 < c_2$.

HYPOTHESIS 11. *In the communication sessions,* $c_3 = 27.7$.

The next two hypotheses are weaker ones that predict certain qualitative features of behavior.

HYPOTHESIS 12. *In the communication sessions, when* $k = 2$, *the two individuals who used the message "**I**" contribute, and the other individual does not contribute.*

HYPOTHESIS 13. *In the communication sessions, the frequency of contributions among individuals who use the message "**I**" is higher when* $k = 2$ *then when* $k = 3$ *and higher when* $k = 3$ *than when* $k = 1$.

The theoretical difference in efficiency of the communication equilibrium and the no communication equilibrium is large and unambiguous. Using either interim or ex ante welfare comparisons, the communication equilibrium is far superior. Net of token values, the ex ante surplus from the communication equilibrium, is roughly three times that of the noncommunication equilibrium. In fact, the net of token value interim surplus for every type is at least 50% higher in the communication equilibrium. In fact, it is almost the case that the communication equilibrium dominates the no communication equilibrium ex post! The outcome with the no communication equilibrium is superior only for rare configurations of token values: where a single player has a token value between 23¢ and 27¢ and both other players have a token value between 28¢ and 43¢. Such configuration arise with probability less than .01.

HYPOTHESIS 14. *On average, subjects earn more in the communication game than in the noncommunication game.*[17]

There is a difficult question when it comes to testing the above hypotheses, particularly the more specific ones (Hypotheses 4–11), because there are so many possible alternative hypotheses. For this reason, we will consider a number of alternative null models. Our analysis of the data is partly descriptive and partly based on formal tests of these hypotheses.

5. RESULTS

A. *Probit Estimation*

Most of the theoretical predictions outlined in the previous section suggest that we estimate the effect of cost on the spending or message decisions, using individual observations. We do this using a probit model. For the no communication sessions, the probit equation is:

$$\text{Prob}\,\{(s_i = 1)\} = \Phi(\beta_0 + \beta_1 \ln(\tilde{c}_i/(61 - \tilde{c}_i))),$$

where

$$\tilde{c}_i = c_i, \qquad c_i \leq 60$$
$$= 60, \qquad c_i > 60,$$

β_0 and β_1 are estimated coefficients,

and Φ denotes the unit normal cumulative.

The transformation of cost used above has the desirable property of producing estimates of the contribution probability near to 1.0 as c approaches zero and near to zero when contribution is a dominated strategy, that is when $c \geq 60$. (We normalized with 61 to avoid division by zero.)[18] The underlying error structure is assumed to be cross-sectionally and intertemporally independent.

[17] The qualifier on average is needed because certain specific realization of costs can actually lead to smaller gains when the communication equilibrium strategies are followed. For example, if the costs are (26, 40, 40), no one contributes in the no communication setting. In contrast, in the communication setting, $k = 3$ and the endowment of 26 is contributed and wasted.

[18] We also tried probits using the cost variable in linear and log forms. As might be expected, fits were similar to those reported here. Our choice of the actual transformation used is largely on theoretical grounds, although log-likelihoods and classification percentages were generally slightly better for the transformation than for the other probit models.

In the communication experiments, we used a conditional probit version of the above model to estimate coefficients β_0^m, β_1^m, β_{0k}^s, β_{1k}^s, $k = 0, 1, 2, 3$, for the message decision and for the spending decision conditional on the number of intent messages produced by the subject's group. Since the message and spending errors were assumed to be independent, each pair of β's could have been estimated independently; we carried out a joint estimation of the full model to facilitate likelihood ratio testing.

For each of the three experiments, we had only nine $k = 0$ observations. In $C2$, no one contributed (as predicted theoretically). In $C3$, for $k = 0$, everyone with a cost below 40¢ contributed and no one with a cost 41 or above contributed. These facts imply that probits cannot be estimated for $k = 0$ in $C2$ and $C3$. Consequently, we deleted the spending (but not the message) decisions for these observations. Results presented for $C1$ contain probit estimates for $k = 0$, but comparisons across the three experiments are based on probits without these nine observations.[19]

Our probit specification can capture the extent to which the data are consistent with our theoretical model. First note that, as individuals tend to use a common cutpoint, the magnitude of β_1 will increase. Indeed, when an empirical cutpoint that achieves perfect classification can be found, the probit will "blow up." Second, if individuals tend to use a common cutpoint, c^*, the estimated probability of contribution at c^* should be 0.5, implying that $\beta + \beta_1 \ln(c^*/(61 - c^*)) = 0$. If this condition is approximately satisfied for the appropriate equilibrium cutpoint and if β_1 is "large" in magnitude, the relationship between decision probabilities and costs will approximate the step function called for in our theory.

The assumption of independence avoids some challenging econometric problems that are pervasive in experimental economics. The structure of the experiment suggests immediately that intertemporal and cross-sectional dependence may be present. Even though the experiment is structured so that each round is an independent game, "error" behavior by one subject may be present, systematically, across all rounds. In the communication setting, an "erroneous" message by one subject will influence the contribution decisions of two other subjects.

As a rough guide, however, we note that we have 54 subjects, 540 message decisions, and 1080 contribution decisions. The "correct" N for

[19] Except for the $k = 0$ observations, all results presented here are for the full set of observations. While there might be some concern that the "significance" of some results reflects mainly the behavior of individuals with dominant strategies not to spend, the transformation we have applied in effect gives these high end observations little weight in the estimation. In fact, the estimates of the spending parameters are virtually identical when estimation is restricted to the subsample with costs below 60; there are some slight, insignificant differences in the estimated coefficients for the message decision. (The last line of Table I shows the "below 60 only" estimates for the no communication experiments.)

TABLE I
PROBIT ESTIMATES FOR THE NO COMMUNICATIONS SESSIONS

Dataset	Constant	Cost	% Correctly classified	N	−2 ln-likelihood
Last 5 rounds	0.200	−0.893	90	135	66.493
	(1.021)	(−5.210)			
Last 10 rounds	0.176	−0.745	89	270	154.489
	(1.529)	(−9.240)			
Last 15 rounds	0.236	−0.690	87	405	255.083
	(2.534)	(−12.304)			
All 20 rounds	0.245	−0.555	84	540	409.328
	(3.181)	(−14.195)			
All 20 rounds	0.244	−0.561	77	360	370.946
costs $\leq$ 60	(3.159)	(−10.758)			

Note. t-statistics in parentheses.

statistical purposes is undoubtedly less than the number of decisions but greater than the number of subjects.

Several key observations emerge from the data analysis, and we summarize these below before addressing the specific hypotheses.

- *Subjects are cost sensitive.* Coefficients on all cost variables are negative in all six experiments. When data are pooled across the three experiments in each condition, the asymptotic t-tests on all cost coefficients are highly "significant." (See Tables I and II). Subjects do not contribute at random. Nor is any subject a pure "altruist" who always contributes or even always contributes when she has a cost less than 60.

The sensitivity of contribution decisions to cost is evident even when we control for subjects' own message decisions. For $k = 3$ and $k = 0$, all messages are identical. For $k = 1$ and $k = 2$, we added a dummy variable for the message to the probits in Table II. Coefficient estimates on the cost terms were essentially unchanged by this exercise. Although the t-statistics corresponding to the introduced dummy variables were never significant at the 0.01 level in any of the three sessions, both dummys were over twice the estimated standard errors in the pooled run. *Ceteris paribus,* "I's" contributed more when $k = 2$ and less when $k = 1$. The estimated effects of the message were slight compared to those of cost.

- *Spending behavior is highly sensitive to communication.* Contribution behavior is strongly conditioned on k, the number of individuals giving "intend to contribute" messages. Evidence is presented in Table III. For each of the three communications experiments, a likelihood-ratio test rejects the null hypothesis that the spending probit coefficients are equal for all k.

TABLE II
PROBIT ESTIMATES FOR THE COMMUNICATIONS SESSIONS

Dataset amd	Constant	Cost	% Correctly classified	N
Session $C1$	−2 ln-likelihood = 293.885 (without k = 0 decisions)			
Intent message	0.757	−0.281	74	180
	(6.068)	(−6.921)		
$k = 3$	0.102	−1.551	90	39
	(0.307)	(−2.682)		
$k = 2$	0.712	−1.552	93	81
	(2.177)	(−2.794)		
$k = 1$	−1.111	−0.875	92	51
	(−2.845)	(−2.336)		
$k = 0$	−0.629	−0.459	89	9
	(−0.665)	(−0.334)		
Session $C2$[a]		−2 ln-likelihood = 328.750		
Intent message	0.227	−0.071	57	180
	(2.006)	(−1.750)		
$k = 3$	−0.138	−0.429	83	24
	(−0.342)	(−1.935)		
$k = 2$	0.496	−0.985	91	78
	(1.772)	(−2.622)		
$k = 1$	−1.007	−1.614	91	69
	(−2.299)	(−2.198)		
Session $C3$[a]		−2 ln-likelihood = 303.619		
Intent message	0.779	−0.189	68	180
	(5.618)	(−3.937)		
$k = 3$	0.627	−0.691	82	57
	(2.861)	(−3.344)		
$k = 2$	1.025	−1.005	86	81
	(4.203)	(−4.669)		
$k = 1$	−0.362	−0.884	88	33
	(−0.866)	(−1.776)		

Note. t-statistics in parentheses.
[a] No estimates for $k = 0$. See text for discussion.

• *Our theory accounts for behavior better than a variety of alternative, rule-of-thumb type models.* For example, predicting that no one with a cost greater than the value of the public good contributes while those with costs below the value contribute randomly, with a probability that is independent of cost, is inferior to the cutpoint predictions of our model. A similar statement applies to the message decisions.

With regard to the communication sessions, one alternative model of contribution would be that individuals behave optimally given the belief

TABLE III
LIKELIHOOD-RATIO TESTS OF NULL HYPOTHESIS OF β'S EQUAL FOR ALL k

Session	$-2 * \ln(\text{likelihood})$: Separate coefficients for each k	$-2 * \ln(\text{likelihood})$: Coefficients constrained to equality	P-level of χ^2 test for likelihood ratio	Degrees of freedom
$C1$	265.765	291.156	0.0003	6
$C2$	328.745	347.956	0.0007	4
$C3$	303.619	313.398	0.0443	4

Note. There are four possible k conditions. Each condition corresponds to two estimated coefficients, one constant and one coefficient on the cost variable. This leads to a total of eight contribution stage coefficients. However, in $C2$ and $C3$, the ability to perfectly classify the $k = 0$ data meant that probits could not be estimated for the $k = 0$ subsets, resulting in only six coefficients. In the constrained model, there are only two coefficients. When the constrained model was estimated, the $k = 0$ spending decisions were deleted from the data for $C2$ and $C3$ in order to preserve comparability with the unconstrained log-likelihood. Degrees of freedom equal the number of unconstrained coefficients minus 2.

that the other two members of the group will carry out the provisions embodied in their intent messages. For $k = 0$ and $k = 3$, this model predicts no one contributes. For $k = 2$, the prediction is that the two "**I**'s" will spend and the "**NI**" will not. For $k = 1$, the prediction is that "**I**'s" will not spend but "**NI**'s" will, when their costs are below the value of the public good.

The predictions of this alternative model differ from those of an equilibrium model only when $k = 3$ or when $k = 1$ and only for those announced "**NI**'s" who have costs below the public good value. Our model performs much better than this alternative in all sessions. In the three sessions combined, there were 166 decisions where the two models could make different predictions. The alternative model predicted 86 decisions incorrectly compared to 39 errors for our equilibrium model.

• *Behavior is more systematic in the noncommunication setting than in the communication setting.*

Several items support the above finding.

• *Behavior becomes more systematic in later rounds of the no communication sessions. In contrast, there is no strong indication of "learning" in the communication setting.* The evidence for the noncommunication sessions can be seen in Table I, where the cost coefficient becomes increasingly negative as earlier rounds are deleted from the sample. In Fig. 1, we have plotted the response curves for estimates from the first five rounds only and from the last five rounds. Note that, except for the tails, the "last five" curve is considerably steeper than the "first five."

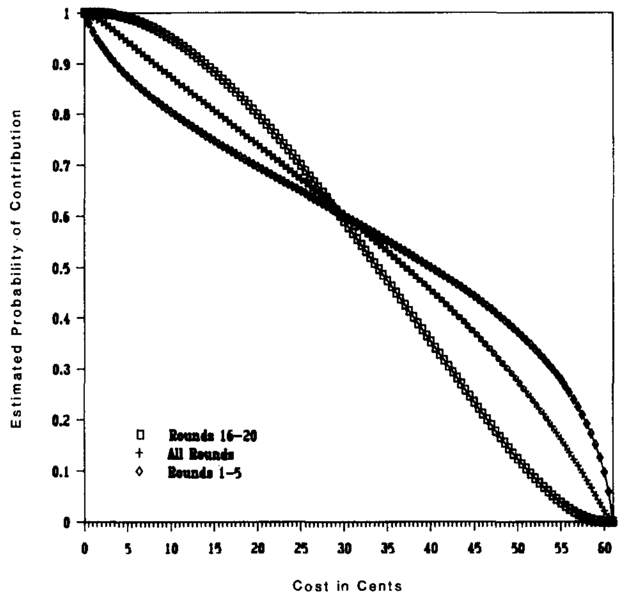

FIG. 1. Experiments without communications (pooled data). (□) Rounds 16–20, (+) all rounds, (◇) rounds 1–5.

The estimated change in behavior is dramatic. For example, at a cost of 50¢, in theory there should be no contributions. In the first five rounds, the estimated contribution probability for this cost is over $\frac{1}{3}$ whereas it is near $\frac{1}{10}$ in the last five rounds. At a cost of 10¢, contribution should occur. The estimated probability is near 0.95 in later rounds but below 0.8 early on.

• *The three noncommunication sessions produced very similar data. In contrast, the three communication sessions produced data with some interesting differences.*

Statistical support for this statement comes from the likelihood-ratio tests for pooling. For the noncommunication sessions, the p-level for the test (4 df) is 0.299. In light of this test, we only present results for the pooled data from all three noncommunication sessions.

In contrast, the likelihood-ratio test has a p-level of only 0.0011 (16 df) for the communications sessions. One major source of the difference between the three sessions is that one session (see $C2$ in Fig. 2) looked much like a "babble" equilibrium in its communication stage. The flatness of the estimated probit curve shows that the message dependence on cost was not as strong as in the other sessions. Similarly, the probit curves for the spending decision in this experiment look quite different from those in the other two (cf. Figs. 3–5).

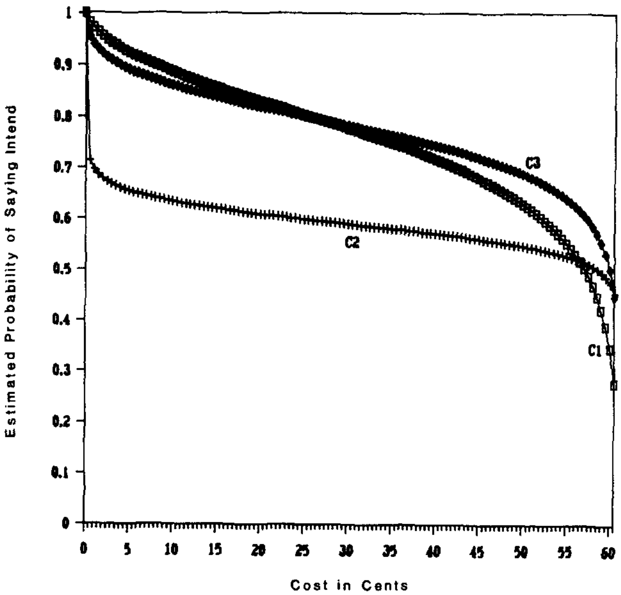

FIG. 2. Communications decision.

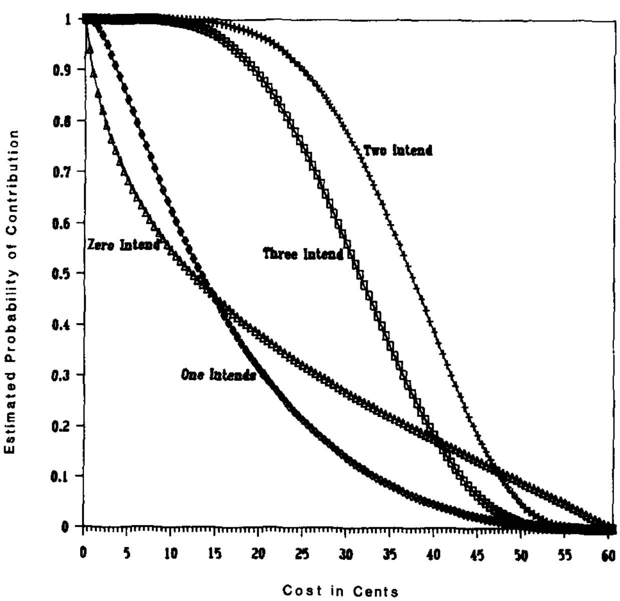

FIG. 3. Spending decision by number intending (Session $C1$).

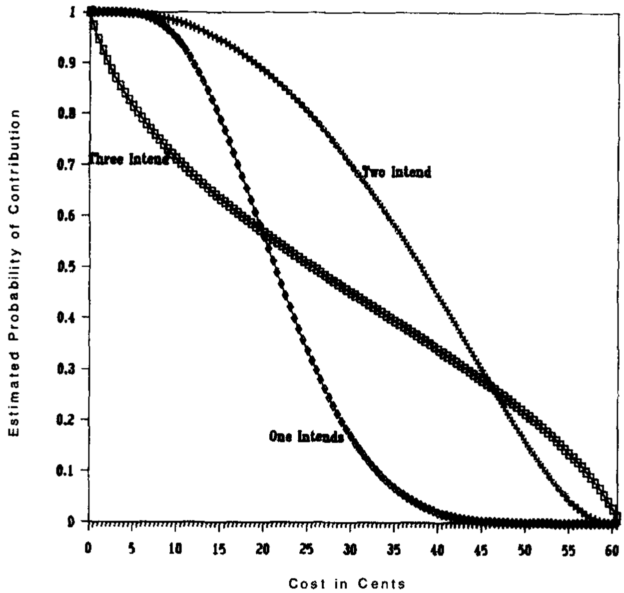

FIG. 4. Spending decision by number intending (Session $C2$).

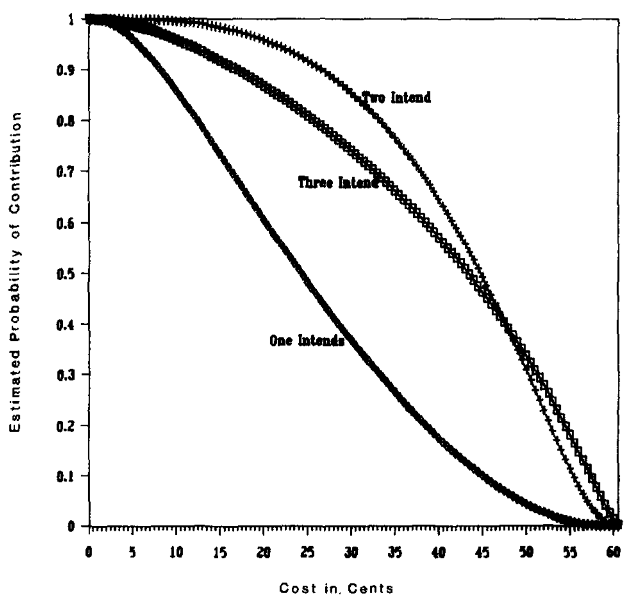

FIG. 5. Spending decision by number intending (Session $C3$).

• *The message stage of all the communications sessions showed a measurable response to cost.* But as seen in Tables I and II, the β_1 coefficients for the intent messages are all smaller in magnitude than the corresponding coefficients for spending decisions. The point is illustrated by Fig. 2, which shows relatively flat response curves for all three sessions.

• *There is no evidence that subjects reversed the meanings of the two messages.*

B. *Specific Hypotheses*

With these initial observations in mind, we now move to discuss our hypotheses.

We begin by examining Hypothesis 1 which predicts that subjects will not use dominated strategies. This hypothesis is strongly supported. In 360 cases where the endowment was greater than 60, there were only four observations of contribution, all occurring in the first six rounds. No individual subject violated strict dominance more than once.

This result is not surprising theoretically, but it is striking when viewed against previous experimental studies. Dawes *et al.* (1985), Isaac *et al.* (1984, 1985), and others find substantial violation of the dominant strategy condition. Typically, at least 20 to 30% of the decisions violate this condition, even after considerable opportunities for learning. In our experiments, if we adopt the null hypothesis that the probability a subject ever uses a dominant strategy is 0.3, the probability of observing 4 or fewer violations in 54 "trials" is only .010. If we are less conservative and treat all 360 observations as independent, the probability of 4 or fewer violations in 360 "trials" when the true probability is .3 is, of course, infinitesimal. Thus, we firmly reject the "null" hypothesis that our results are consistent with those generally reported in the literature and accept Hypothesis 1 in the form that dominated strategies are "almost never" used.

We conjecture that there are two, related reasons why we have found such strong support for the dominant strategy hypothesis. First, our random assignment of costs induces considerable heterogeneity among subjects. Thus, a high cost subject can reasonably expect (in equilibrium) that others with lower costs might provide the public good. In contrast, in previous dominant strategy experiments, subjects had identical endowments. Second, the previous experiments were essentially prisoner's dilemma situations. In a prisoner's dilemma, the all cooperate outcome Pareto dominates all defect. In our situation, this is not true, at least when this Pareto comparison is made at the interim, as opposed to ex ante, stage. A high cost individual is worse off when everyone contributes than when no one contributes. Also, in contrast to our game, the all contribute outcome in past designs has a strong focal property (Schelling, 1958) due to a combination of symmetry, optimality, fairness, and complete information.

Turning to Hypothesis 2, first observe that it is a relaxation of Hypothesis 3. Hypothesis 3 is clearly assumed by our theoretical model. However, in Palfrey and Rosenthal (1988), we noted that previous public goods experiments where all costs were public information had not been totally successful in controlling subject preferences via monetary incentives. We assumed that each subject also received an "altruism" payoff from the act of contributing and that the amount of this payoff was private information. A Bayesian equilibrium analysis of this situation succeeded in accounting for aggregate contribution rates across a wide variety of experimental settings.

In designing the experiments reported on here, we were hopeful that the uncertainty concerning monetary costs would swamp the uncertainty concerning nonmonetary payoffs. Were this not true, however, there would be individual heterogeneity in monetary cutpoint rules. Even in the context of an "altruism" model, however, individuals should be using definite cutpoint rules. That is, when we examine individual contribution records, we should find a cost (or a range of costs given that there are only 20 observations per individual) below which the individual always contributes and above which the individual never contributes. Consequently, for each individual, we searched over the values of the cost variable, to find the cost or costs which minimized classification errors for the individual. We report on the computations solely for the no communication sessions. In the communication sessions, small N's for each k rule out any meaningful individual analysis of individual spending decisions. As to the communication decision, the very flat probit response curves of Fig. 2 suggest that a cutpoint model is a poor description of that decision. We defer further discussion of the communication sessions until we address Hypotheses 5–13.

As to the no communication experiment, we should, in theory, be able to find a cutpoint generating zero classification errors for each individual. Not surprisingly, the results, summarized in Table IV, are only weakly consistent with the theory.

Although the number of classification errors is low, we are able to have perfect classification for only 5 of the 27 subjects. Consequently, it is important to see whether the theory at least points in the right direction.

The easiest way to do this is to calculate how many classification errors are generated by estimating a best cutpoint when decisions are purely random. Such a test was conducted and strongly rejects the random model. However, one could argue that such a test is too weak because the results are partially driven by the fact that virtually no one contributes for

TABLE IV
MINIMUM CLASSIFICATION ERRORS FOR INDIVIDUAL CUTPOINTS

Number of errors	Number of subjects
0	5
1	7
2	4
3	7
4	4
5	0
Total number of subjects	27

costs in excess of 60. Consequently, we considered as a stronger null model the hypothesis that (a) no contribution occurs for costs above 60 and (b) contribution occurs at random for costs below 60. To test this model against observed contribution patterns, we used the following "bootstrap" Monte Carlo model. We first estimated an individual random contribution probability by the observed frequency of contribution for the individual in all trials where his actual cost was below 60. We then generated a simulated experimental result for each individual by using his actual costs. The individual always contributed for costs at or above 60. Random draws were used to find the simulated contribution decision for costs below 60. This was done for all nine individuals in a session to generate a simulated round for the session. To parallel the actual experiment, we generated 20 rounds. For each simulated session, we could find the minimum classification errors for the simulated data for each individual. We could then check whether the simulated minimum was at or below the actual minimum. We could also check whether the total minimum, over all nine subjects, was at or below the actual total.

For each of the three sessions, we repeated the simulation 500 times. The fraction of the 500 times that the simulated minimum errors fell below an individual's actual minimum level is taken as the bootstrap "*P*-level" for rejecting the null hypothesis. The results of this exercise are shown in Table V.

The table shows that it is difficult to reject, at conventional significance levels, the null hypothesis of random (below 60¢) choice for individual subjects. The reason is basically one of small sample size. Each subject will average only 13.333 decisions with a cost below 60. If a subject chooses randomly with probability .5 and makes a large number of choices, there will be close to 50% classification error. But our simulation shows that expected classification errors are far fewer than $\frac{1}{2}$ of 13.333 or

TABLE V
SIMULATIONS OF RANDOM CHOICE
BELOW 60¢

No communication	
Average minimum	1.926
Classification errors (N = 27)	
Simulated average (N = 27 × 500)	3.498
P-level for the average	
$N1$	0.002
$N2$	0.
$N3$	0.
Number of individual P-levels < 0.05 (N = 27)	7
Number of individual P-levels < 0.50 (N = 27)	19

6.667. There are two reasons for this. First, even if a subject mixes with probability .5, the expected errors will be less than 6.667 as a result of the small sample size. Second, several subjects had observed frequencies distant from .5. To cite the most extreme example, one subject contributed (below 60) with frequency 0.917 and had two classification errors. The simulations showed an average of only 0.856 errors for this subject, corresponding to a P-level of 0.954.

Although we were unable to reject the null hypothesis for some individual subjects because of small sample size, the distribution of P-levels, as shown by the last two lines of the table, is far from the expected distribution given random choice. Even more striking is the result for the P-levels for the experiment averages. When averaged over nine subjects, thus reflecting an expected average of 120 choices rather than 13.333, the classification errors of the simulation were less than or equal to those observed in only one (the $N1$ entry in the table) of 1500 simulations. We thus can strongly reject the null hypothesis of conditional random choice. Although classification errors are not zero, as called for by a strict cutpoint model, they are clearly substantially less than those that would be generated by random choice. Even stronger rejection would have occurred if we had set the choice probabilities for each individual to the nine subject averages and still stronger had the probabilities been set to .5. Rather than choosing randomly, individuals contribute when the cost is "low" and do not contribute when the cost is "high."

Individual differences in estimated cutpoints have only a minor impact on the results. Although errors jump from a total of 52, with individual cutpoints, to 84, when we impose a common cutpoint (at 35.5), the increase of 32 errors is small, given that individual cutpoints represent 26 addition degrees of freedom.

Indeed, if a common cutpoint model makes somewhat more errors than individual specific cutpoints, the errors remain substantially below those that would be found in a simple null model where noncontribution is predicted for costs above 60 and a coin toss is used otherwise. Such a model would have 180 expected errors for our 540 decisions.

An alternative approach is to estimate the cutpoint as the cost at which the probit estimate of the contribution probability is .5. The fact that the probit curves get steeper as the subjects become more experienced (Fig. 1) lends support to our cutpoint model. By the last five rounds classification based on the probits are excellent, with 90% of the last 135 observations being correctly classified by a single estimated cutpoint of 33.9. While future research might want to consider models that are based on decision rules other than cutpoints, such efforts will find it difficult to improve on a common cutpoint model.

We now turn to the remainder of our hypotheses, which concern the exact theoretical cutpoints. Examination of Tables I and II and Figs. 1–5 discloses that our results are noisy in the sense that we do not find the sharp step function plots that would result if decisions closely followed a cutpoint rule that was constant across individuals. Consequently, we focus primarily on whether our cutpoint models capture some central tendency in the data.

To do this we compute the estimated classification "cutpoint," the value of c for which the decision probability is .5. Then, using standard techniques for computing the asymptotic variance of a nonlinear function of estimated coefficients (Wilks, 1962, p. 260), we compute a 95% confidence interval for this cutpoint. The results appear in Table VI.

With reference to Hypothesis 4, the estimated cutpoint for the no communications setting is 37.2 for the pooled sample over all 20 rounds and 33.9 over the last 5 rounds. The theoretical cutpoint of 22.5 is not in either confidence interval. Some specifications of risk aversion and altruism (Palfrey and Rosenthal, 1988) are consistent with this observation.

The remaining hypotheses focus on the communications experiment. Hypothesis 5 states that the message cutpoint should be 43.4. Although the probit curves for the three experiments exhibit substantial differences (see Fig. 2), the estimated cutpoints of 57.1, 58.6, and 60.0 are remarkably similar. Again the theoretical cutpoints are outside the confidence intervals. The theory underpredicts the frequency with which promises are made. On the other hand, the theory is qualitatively consistent with the observation that promised contributions outnumber actual contributions.

Hypotheses 6 through 11 concern the spending cutpoints in the second

TABLE VI
95% Confidence Intervals for Probit Prediction Cutpoints

Environment		Theory cutpoint	Point estimate	Confid. interval High end	Confid. interval Low end
Spend, no communication, 20 rounds		22.5	37.16	40.87	33.46
Spend, no communication, last 5 rounds		22.5	33.91	41.07	26.75
Message, communication	$C1$	43.4	57.15	59.58	54.31
Message, communication	$C2$	43.4	58.64	66.43	50.85
Message, communication	$C3$	43.4	60.03	61.53	58.53
$k = 3$	$C1$	27.7	31.51	38.44	24.58
$k = 3$	$C2$	27.7	25.62	57.92	0.0[a]
$k = 3$	$C3$	27.7	43.46	53.46	33.46
$k = 2$	$C1$	43.4	37.38	44.99	29.77
$k = 2$	$C2$	43.4	38.01	45.09	30.93
$k = 2$	$C3$	43.4	44.82	51.29	38.36
$k = 1$	$C1$	0.0	13.37	23.91	2.84
$k = 1$	$C2$	0.0	21.28	26.69	15.88
$k = 1$	$C3$	0.0	24.34	38.45	10.23
$k = 0$	$C1$	0.0	11.04	86.00	0.0[a]

[a] Estimated interval below zero.

stage that result once the number of "intent" messages is made public. In contrast to the communications stage, the estimated cutpoints in the second stage indicate remarkable support for the theoretical model.

The first of the cutpoint hypotheses, $c_0 = 0$, is well supported in the data. While, in $C3$, there was somewhat more contribution than expected, the presence of only 5 contributions in 27 $k = 0$ opportunities is supportive of the theory. Indeed, the "public good" was never produced in the 9 cases where $k = 0$.

The results for Hypothesis 7, $c_1 = 0$, are, like those for Hypothesis 6, in line with the theory. First, as expected theoretically, contribution is always less when $k = 1$ than when $k = 2$ or 3. The estimated cutpoints of 13.4, 21.3, and 24.3 are small, if significantly different from zero. (No confidence interval includes zero.) Moreover, the $k = 1$ cutpoints are significantly less than the $k = 2$ cutpoints.

Hypothesis 8 states that the $k = 2$ cutpoint should equal the communications cutpoint and Hypothesis 9 restricts this equality to the theoretical value of 43.4. The estimated cutpoints of 37.4, 38.0, and 44.8 strongly support Hypothesis 9 but do not support Hypothesis 8. Promises occur too frequently, not only with respect to the basic theory, but also with regard to the observed patterns of contribution.

The data provide very strong support for Hypotheses 10 and 11. When everyone says "I," not everyone spends. The estimated spending cutpoints are 31.5, 25.6, and 43.5, values that are substantially below the communication cutpoints. Indeed, the theoretical value of 27.7, specified in Hypothesis 11, is in the confidence interval in two of the three cases.

We next turn to Hypotheses 12 and 13 which examine whether individuals' contribution decisions are consistent with their message decisions. Hypothesis 12 is reasonably supported in a qualitative sense. Looking only at subjects with costs less than 60, we find that 80 of 113 subjects who had said "**I**" contributed when told that k equaled 2 whereas only 11 of the 43 subjects who had said "**NI**" contributed. Although some individuals who say "**I**" do not contribute and some who say "**NI**" do contribute, contribution rates are far higher for "**I**" types than for "**NI**" types.

Hypothesis 13 finds similar support. In comparison to the 80 of 113 "**I**" who contributed when k was 2 only 54 of 95 contributed when k equaled 3 and 11 of 42 when k equaled 1. Thus, those who say "**I**" are somewhat more likely to contribute when they are in the "critical" $k = 2$ situation than when they are in the "surplus" $k = 3$ situation.

The previous hypotheses concerned equilibrium behavior. Our last hypothesis relates to the welfare consequences of communication. Is communication ex ante more efficient than noncommunication? Do subjects, taken together, earn more money when they can communicate using a simple binary message space?

In Table VII, we present average subject earnings from the two sets of experiments. The table also contains several benchmarks including:

- average earnings in the degenerate equilibrium where each subject keeps his or her endowment,

TABLE VII
AVERAGE DOLLAR EARNINGS PER SUBJECT

	Session		
	1	2	3
Actual earnings			
No communication	11.54	11.08	11.50
Communication	10.80	11.34	12.10
Theoretical earnings			
No spending—all endowments kept	9.26	9.09	9.36
No communication—22.5¢ cutpoint rule	9.73	10.52	11.31
Communication—$c_c = c_2 = 43.4$, $c_0 = c_1 = 0$, $c_3 = 27.7$	12.94	12.63	13.70
Coordination—no side payments	15.08	15.60	15.69
Coordination—sidepayments	16.68	16.40	16.73

- average earnings if each subject had in fact followed the equilibrium cutpoint rule of the no communication game,
- average earnings if each subject had in fact followed the equilibrium cutpoint rules of the communication game,
- average earnings given optimal coordination without sidepayments—the two low cost subjects contribute provided both have costs ≤ 60.
- average earnings given optimal coordination with side payments—the two low cost subjects contribute.

In examining the table, one can see that the nondegenerate equilibrium without communication offers only modest gains relative to the autarchy solution, less than $2 per subject for a play of 20 rounds. The modesty of the gains reflects the low value (22.5) of the equilibrium cutpoint. If subjects could coordinate, much greater gains would be possible. Instead of the roughly $9 that would be earned were each subject simply to always keep his or her endowment, earnings would average over $15 under full coordination without side payments. The ability to make side payments would generate about one more dollar of earnings. Simple binary cheap talk has the potential for realizing over half the gains of full coordination, since in the equilibrium we have focused on average earnings are predicted to be on the order of $13.

The actual experimental results, however, did not provide strong support for Hypothesis 14. On the one hand, the communication sessions generated greater efficiency in two of the three matched comparisons. On the other, aggregating all sessions shows no significant difference in earnings between the communication and noncommunication conditions.

6. Conclusion

These experiments produced some very interesting findings about the effects of cheap talk. Clearly there is communication that affects behavior in systematic ways. Subjects tend to use cutpoint decision rules in a situation where it is optimal to do so, and these decision rules, at least in the contribution stage, correlate well with the theory. Certainly the most evident discrepancy between our theoretical model and experimental evidence concerns the great variability in the message decisions in the communication sessions. This discrepancy is closely related to the finding that communication does not improve efficiency. It is therefore all the more striking that the behavior in the contribution stage of the communication experiments corresponds closely to the theoretically predicted equilibrium. We obtained this finding in spite of the fact that the communication behavior deviated systematically from the theoretical prediction. This interesting anomaly needs to be investigated in further research.

To do so will require not only more theory and more subjects but also more rounds per subject. In the no communication environments, we found important differences in behavior between the first 10 rounds and the last 10 rounds. Because breaking down the last 10 rounds into the various k conditions would have resulted in small N's, we were unable to control for experience in our study of the subgames.

Finally, a novel feature of our design produced an interesting new finding. With respect to behavior concerning dominated strategies, the introduction of heterogeneity and private information completely eliminates the perverse individual choice behavior widely observed in other public goods experiments. On the other hand, the result for the noncommunication setting that the estimated cutpoint was persistently above the equilibrium prediction suggests that altruism (or maybe some other explanation) remains important in the undominated range. This raises some new and interesting issues about the effects of experimental design on subject motivations.

APPENDIX

This appendix characterizes symmetric Bayesian equilibria to the two-stage replay game. It is assumed that "**I**" and "**NI**" have their conventional meaning. We further restrict our attention to equilibria in which cutpoints in the second stage depend on the communication stage only through the aggregated message.

While the resulting set of equilibria is fairly large, this set does not exhaust all equilibria of the replay game. One can construct, for example, equilibria in which cutpoints in the second stage depend upon one's *own* choice in the first stage, as well as the aggregate message in the first stage. The equilibria we focus on are the simplest ones in which communication plays a nontrivial role.

In the first stage, all players must have the same decision rule. The only piece of information that can be used to condition the decision rule is c_i, so a simple cutpoint rule is natural in the first stage. This takes the form:

Say "**I**" *if and only if* $c_i \leq c_c$. *Otherwise say* "**NI**."

In the second stage, players know not only c_i but also k. Therefore, second-period moves can be characterized as being of the form:

spend ($s_i = 1$) *if and only if* $c_i \leq c_k$, $k \in \{0, 1, 2, 3\}$. *Otherwise do not spend* ($s_i = 0$).

Let EU(·) denote expected utility and let $F(\cdot)$ be a cumulative distribution function. The conditions for a cutpoint equilibrium are:

$$\left.\begin{array}{ll}\text{(I1)} & \mathrm{EU}(\mathbf{I}; c_c) = \mathrm{EU}(\mathbf{NI}; c_c) \\ \text{(I2)} & \mathrm{EU}(s_i = 1 | k; c_k) = \mathrm{EU}(s_i = 0 | k; c_k)\end{array}\right\} \quad \text{Indifference conditions,}$$

$$\left.\begin{array}{ll}\text{(B1)} & \Pr(\mathbf{I}) = c_c/1.5 \\ \text{(B2)} & \Pr(s_i = 1 |\, c_i < c_c,\, c_k < c_c) = c_k/c_c \\ \text{(B3)} & \Pr(s_i = 1 |\, c_i \geq c_c,\, c_k < c_c) = 0 \\ \text{(B4)} & \Pr(s_i = 1 |\, c_i \geq c_c,\, c_k > c_c) = (1.5 - c_k)/(1.5 - c_c) \\ \text{(B5)} & \Pr(s = 1 |\, c < c_c,\, c_k > c_c) = 1\end{array}\right\}$$

Bayes' rule conditions.

The first indifference condition says that an individual at the cutpoint in the communications stage must be indifferent between the two messages. The second condition says that, after k is made public, an individual with endowment c_k must be indifferent between contributing and not.

Conditions (B1)–(B5) are only slightly more complicated. The first of these says that the probability a player uses **I** equals the probability of having a cost below the cutpoint. Once k is revealed, in equilibrium, each player knows with certainty how many of the other two players have costs below the cutpoint and how many are above. This information is incorporated in conditions (B2)–(B5).

While these conditions are useful for computing equilibria and are nearly necessary and sufficient, there are some additional conditions that arise in special cases. For example, if $c_k = c_c$ the indifference condition (I2) will not hold. A player at c_c is indifferent about his message in stage 1. If he in fact said "**I**," in equilibrium he must strongly prefer to contribute in stage 2; the reverse holds if he said "**NI**." Other special inequality conditions will be dealt with as they arise.

PROPOSITION 0. *For all* k, $c_k \leq 1$.

Proof. The move $s_i = 0$ is dominant if $c_i > 1$. Hence the indifference condition cannot be satisfied if $c_k > 1$.

PROPOSITION 1. $c_2 > 0 \Rightarrow c_c \leq 1$.

Proof. Assume $c_c > 1$. By Proposition 0, we need only consider $c_2 \leq 1$. If an individual at c_2 does not spend, his expected payoff is at least zero. If, on the other hand, the individual spends, the expected payoff is

$$-c_2 + c_2/c_c < 0.$$

This is a contradiction, because it implies that c_2 types will not contribute.

Analysis of Second-Stage Strategies

Remark 1. For every k, $c_k = 0$ defines an equilibrium to the second-stage game.

Next we characterize equilibria in the spending continuation games when $c_k > 0$ for some k.

Case A. $c_3 > 0$.

This is the continuation game after all players say "**I**." Consequently, players will update their priors over other types to $U[0, c_c]$. At the cut-point, the cost of contribution must equal the probability that the player is pivotal to providing the public good. Consequently, the equilibrium condition can be written (for $c_c \neq 0$) as

$$c_3 = 2(c_3)(c_c - c_3)/(c_c)^2.$$

This equation has two solutions, $c_3 = 0$ and

$$c_3 = c_c - (\tfrac{1}{2})(c_c)^2.$$

Thus, for every $c_c > 0$, there is a unique solution strictly between 0 and c_c.

Case B. $c_0 > 0$.

This case parallels the previous one. This is the continuation game when all players said "**NI**" and the players update priors over other types to $U[c_c, 1.5]$. The equilibrium condition can be written as

$$c_0 = 2(c_0 - c_c)(1.5 - c_0)/[1.5 - c_c]^2. \tag{2}$$

This equation has the "No communication" solution of $c_0 = 0.375$ if $c_c = 0$. For $0 < c_c < 0.0359$, there are two solutions that satisfy $c_0 > c_c$. For $c_c = 0.0359$, there is a unique solution of $c_0 = 0.232$. For $c_c > 0.0359$, no solution exists. The upshot of this discussion is that any equilibrium with $c_0 \neq 0$ implies using almost no communication (separation of types) in the first stage. Also note that

Remark 3. If $c_0 \neq 0$, $c_0 > c_c$.

Case C. $c_2 > 0$.

Remark 4. Either $c_2 = 0$ or $c_2 = c_c$, or $c_c = 1$ and $c_2 = c_3 = \frac{1}{2}$ and $c_0 = c_1 = 0$.

Proof. 1. That the stated condition, in conjunction with Proposition 1, defines equilibria is obvious.

2. Assume $c_2 > c_c$. At c_2 an individual must be at least as well off spending as not spending. Since $c_2 > c_c$ by hypothesis, a player at c_2 must have said "**NI**." Since $k = 2$, this player knows that both of the other players will spend. Hence, this player is strictly better off not spending, contradicting indifference.

3. Assume $c_2 < c_c$. Now a player at c_2 must have said "**I**." The player knows that at least one other player will not spend. If the player does not spend, the payoff is zero. If the player spends, the expected payoff is

$$-c_2 + c_2/c_c \geq 0, \qquad \text{since } c_c \leq 1 \text{ by Proposition 1.}$$

If $c_c < 1$, $-c_2 + c_2/c_c > 0$.

If $c_c = 1$, then $-c_2 + c_2/c_c = 0$ for any c_2! This means that if $c_c = 1$, then *any* value of c_2 is an equilibrium in the $k = 2$ subgame. However, one can show that $c_2 = \frac{1}{2}$ is the only value for which there are also values of c_0, c_1, c_3 that constitute an equilibrium. To see this, first observe that $c_c = 1$ implies $c_0 = c_1 = 0$. Second, note that $c_c = 1$, $c_2 < 1$ jointly imply that $c_2 = c_3$ by the following argument. If $0 \leq c_2 < c_3$ than all types are better off in the communication stage saying "**I**," a contradiction to $c_2 = 1$. The reverse is true for types between c_2 and 1 if $0 < c_3 < c_2 < 1$. Finally, if $c_c = 1$ then either $c_3 = 0$ or $c_3 = \frac{1}{2}$. Thus, the only equilibria with $c_c = 1$ are (1) $c_0 = c_1 = c_2 = c_3 = 0$ (No Communication) and (2) $c_0 = c_1 = 0$, $c_2 = c_3 = \frac{1}{2}$.

Case D. $c_1 > 0$.

Remark 5. If $k = 1$, either $c_1 = 0$ or $c_1 = 1.5/(2.5 - c_c) > c_c$.

Proof. 1. $0 < c_1 < c_c$ is impossible because at most one player would spend under this rule, leading to a contradiction.

2. The remainder of the result follows from recognizing that if $c_1 > c_c$, one player contributes with probability one. The other players then play a "one of two" contribution game with the indifference condition being:

$$\text{Spend payoff} \equiv 1 - c_1 = (c_1 - c_c)/(1.5 - c_c)$$
$$\equiv \text{Not Spend Payoff.}$$

Solving this equation for c_1 leads to the condition in the remark. We also must verify an additional inequality constraint. If $c_1 = 1.5/(2.5 - c_c)$, the person who said "**I**" must be at least as well off spending as not spending. This will be true for all $c_i < c_c$ if and only if

$$c_c \leq 2c_1(1 - c_1).$$

This implies that we must have $c_c \leq 0.41$ in order for $c_1 > 0$.

Case E. $c_3 > 0$.

PROPOSITION 2. *If* $c_k > 0$ *for at least one* k, $k < 3$, $c_c \leq 1$. *If* $c_3 > 0$, $c_c \leq 1$ *or* $c_c > 1.5$.

Propositions 0 and 1 and Remarks 3–5 demonstrate that the proposition is correct if either c_0, c_1, or c_2 is strictly positive. The only case to be considered then is $c_0 = c_1 = c_2 = 0$ and $c_c > 1 > c_3 > 0$. Consider the indifference condition for c_c. By $c_c > 1$, if $k = 3$, a player at c_c will not contribute. If the player says "**NI**," the expected utility is zero. If the player says "**I**," the expected utility is the probability both others say "**I**" and contribute. By $c_3 > 0$, this probability is strictly positive. Hence, the indifference condition cannot be satisfied for $c_c \leq 1.5$.

Our analysis of the second-stage continuation subgames is now complete. To analyze the various equilibria to the complete game, it is useful to have some building blocks.

Define V_{kn} as the product of (a) the probability that k individuals say "**I**" given that one player with cost c_c says "**NI**" and (b) the expected payoff to this player in the resulting subgame. V_{ky} is defined analogously when the player says "**I**."

We have

$$V_{0n} = \underbrace{\left[\frac{1.5 - c_c}{1.5}\right]^2}_{\text{Prob. } k = 0.} \Bigg[\underbrace{-c_c}_{\substack{\text{Player contributes} \\ \text{and pays cost.}}} + \underbrace{1 - \left[\frac{1.5 - c_0}{1.5 - c_c}\right]^2}_{\substack{\text{Benefit provided if at least} \\ \text{one other player contributes.}}}\Bigg]$$

$$V_{1n} = \underbrace{2\left[\frac{c_c}{1.5}\right]\left[\frac{1.5 - c_c}{1.5}\right]}_{\substack{\text{Probability one other} \\ \text{"I" or } k = 1.}} \underbrace{(1 - c_c)}_{\substack{\text{Player contributes. Benefit} \\ \text{certain; other "I" has low cost.}}}$$

$$V_{2n} = \underbrace{\left[\frac{c_c}{1.5}\right]^2}_{\text{Prob. } k = 2.} \underbrace{1}_{\text{Player does not contribute. Benefit certain.}}$$

$$V_{1y} = \underbrace{\left[\frac{1.5 - c_c}{1.5}\right]^2}_{\text{Prob. } k = 1.} \Bigg[\underbrace{-c_c}_{\substack{\text{Player contributes} \\ \text{and pays cost.}}} + \underbrace{1 - \left[\frac{1.5 - c}{1.5 - c_c}\right]^2}_{\substack{\text{Benefit provided if at least} \\ \text{one other player contributes.}}}\Bigg]$$

$$V_{2y} = \underbrace{2\left[\frac{c_c}{1.5}\right]\left[\frac{1.5 - c_c}{1.5}\right]}_{\substack{\text{Probability one other} \\ \text{"Yes" or } k = 2.}} \underbrace{(1 - c_c)}_{\substack{\text{Player contributes. Benefit} \\ \text{certain; other "Yes" has low cost.}}}$$

$$V_{3y} = \underbrace{\left[\frac{c_c}{1.5}\right]^2}_{\substack{\text{Prob.} \\ k\,=\,2.}} \underbrace{\left[\frac{c_3}{c_c}\right]^2}_{\substack{\text{Probability both} \\ \text{others contribute.}}} \qquad \text{Player does not contribute.}$$

The proposition and remarks we have developed imply that we can confine our investigation to the following exhaustive cases:

A. *Case with No Strictly Positive* c_k

Case 0. $c_0 = c_1 = c_2 = c_3 = 0$, c_c arbitrary.

B. *Cases with One Strictly Positive* c_k

Case 1. $c_1 = c_2 = c_3 = 0$, $c_0 > c_c \geq 0$.

The equilibrium here is $c_c = 0$, $c_0 = 0.375$, and involves no meaningful communication.

To see this, consider the first-stage indifference condition. The expected utility of saying "**I**" is 0. If a player says "**NI**," the player's expected utility is also 0 unless $k = 0$. If $k = 0$, the player at c_c will definitely contribute since that player is below c_0. Formally,

$$\begin{aligned} \mathrm{EU}(\text{``}\mathbf{NI}\text{''}; c_c) = \Pr(k = 0) \times [-c_c &+ \Pr(\text{one other contributes}) \\ &+ \Pr(\text{two others contribute})]. \end{aligned}$$

But from the $k = 0$ subgame condition, Pr(one other contributes) $= c_0 > c_c$. Hence, the expected utility of saying "**NI**" is strictly positive, contradicting the indifference condition for $c_c > 0$. If $c_c = 0$ and everyone says "**NI**," no information is generated; we are back to a no communications game. The cutpoint in this game is 0.375.

Case 2. $c_0 = c_1 = c_2 = 0$, $c_c > c_3 > 0$.

The only equilibrium here is also a "No Communications" equilibrium. Everyone says "**I**" at stage 1. This can be shown by a slight extension of the argument used in Proposition 2. A player with endowment c_c will definitely not contribute when $k = 3$ since $c_3 < c_c$. Again we have 0.375 as the stage 2 cutpoint. Case 2 can be distinguished from Case 1 only in its predictions about communications. Actual contributions should be identical in both cases.

Case 3. $c_0 = c_1 = c_3 = 0$, $c_2 = c_c$.

We can compute the equilibrium from the first-stage indifference condition:

$$\mathrm{EU}(\text{``}\mathbf{NI}\text{''}) \equiv V_{2n} = V_{2y} \equiv \mathrm{EU}(\text{``}\mathbf{I}\text{''}).$$

The equilibrium condition is a simple quadratic equation. The only relevant solution is $c_c = c_2 = 0.634$.

This case represents a very simple form of coordination—make announcements and contribute only if the number of announcements exactly matches the number of needed contributions. Even here simple calculations show that there are welfare gains relative to the no communications equilibrium with cutpoint 0.375.

Case 4. $c_0 = c_2 = c_3 = 0,\ c_c < c_1 < 1.$

To find the equilibrium in this case, we need a simultaneous solution to the $k = 1$ indifference condition and

$$\mathrm{EU}(\text{``}\mathbf{NI}\text{''}) \equiv V_{1n} = V_{1y} \equiv \mathrm{EU}(\text{``}\mathbf{I}\text{''}).$$

The numerical solution is $c_1 = 0.667$ and $c_c = 0.250$.

C. *Cases with Two Strictly Positive c_k Values*

Case 5. $c_0 = c_1 = 0,\ c_c = c_2 > c_3.$

The equations that must be solved are the $k = 3$ equation and

$$\mathrm{EU}(\text{``}\mathbf{NI}\text{''}) \equiv V_{2n} = V_{2y} + V_{3y} \equiv \mathrm{EU}(\text{``}\mathbf{I}\text{''}).$$

The solution shows $c_c = c_2 = 0.724$ and $c_3 = 0.462$. Provision of the public good is increased over Case 3. The probability the good is provided is .421.

Case 6. $c_0 = c_3 = 0,\ c_c = c_2 < c_1.$

To solve this case we use the $k = 1$ condition and

$$\mathrm{EU}(\text{``}\mathbf{NI}\text{''}) \equiv V_{1n} + V_{2n} = V_{1y} + V_{2y} \equiv \mathrm{EU}(\text{``}\mathbf{I}\text{''}).$$

The solution has $c_c = 0.38$ and $c_1 = 0.71$.

Case 7. $c_0 = c_1 = 0,\ c_2 = c_3 = \frac{1}{2},\ c_c = 1.$

See Remark 4.

The other four possible cases with two nonzero c_k all have no solutions.

D. *Cases with Three or Four Strictly Positive c_k Values*

There are no cases in which three or more c_k values can simultaneously have nonzero solutions. This implies that other than the degenerate "No Communication" equilibrium, there is no cutpoint equilibrium in which contribution occurs for $k = 0$.

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