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Thinking like a game theorist: factors affecting the frequency of equilibrium play

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Abstract

Previous research has reported conflicting results on whether and the extent to which individuals play equilibria of experimental games. Two experiments reported in this paper ask whether the act of eliciting beliefs about the actions of others influences a subjects' likelihood of playing an equilibrium in a social dilemma or public goods game. The first experiment compares two versions of a linear public goods game, one with and one without an elicitation of beliefs. Contributions in the two treatments were significantly different, with the actions of subjects in the elicitation treatment closer to the equilibrium prediction of full free riding. A second experiment investigates the same question using a prisoner's dilemma game with similar results; subjects in the elicitation treatment play the dominant strategy significantly more than subjects in a control treatment. ©2000 Elsevier Science B.V. All rights reserved.

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1. Introduction

Previous research in experimental economics has identified arenas where equilibrium predictions are generally observed (e.g. experimental markets), as well as others where they are generally not (e.g. individual decision-making experiments). Even within a particular experimental setting, some treatments yield results which are consistent with equilibrium predictions while others do not. For example, although the equilibrium predictions for the English and second-price auctions are identical, subjects persistently overbid in second-price auctions (Camerer, 1999). Other research has demonstrated that whether an identical game is presented in a matrix or in a tree affects the extent of equilibrium play ob-

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served (Croson, 1990; Schotter et al., 1994). The unresolved question, then, is what factors cause participants to act as game theorists predict when involved in strategic situations.

In this paper I examine the influence of one such factor, belief elicitation. Results suggest that when subjects are asked to state beliefs of what the other party will do in a game they are significantly more likely to play the equilibrium outcome than if they are not asked. This result suggests that eliciting beliefs of what other players will do causes subjects to think about the game in ways that did not occur to them initially; in particular, to think more like a game theorist.

In order to be able to discern an increase in game-theoretic thinking and play, we need to begin with a setting in which most subjects do not play in accord with equilibrium predictions. Experimental economists and psychologists have long noted significantly less equilibrium play than predicted in prisoners' dilemmas and public goods games (see Davis and Holt, 1994 and Ledyard, 1995 for reviews). This suggests the general area of social dilemmas as a natural place to look for the effect of belief elicitation in equilibrium play.

In the first study I examine the effect of belief elicitation on a linear public goods game. In the second study I examine the same question in a standard prisoner's dilemma game. Both experiments clearly show that the act of eliciting subjects' beliefs decreases contribution (in study I) and cooperation (in study II) and increases the frequency of equilibrium play.

Section 2 reviews previous and relevant literature. Section 3 describes the experimental design and results in the linear public goods game with and without elicited beliefs. Section 4 describes the experimental design and results in the prisoner's dilemma game with and without elicited beliefs. Finally, section 5 concludes.

2. Previous experiments

In this section I briefly review previous public goods and social dilemma experiments which have explicitly addressed beliefs of others' behavior, either by eliciting or controlling them. More general reviews of these experiments can be found in Rapoport and Chammah (1970), Davis and Holt, and Ledyard.

2.1. Elicited beliefs

A number of studies in experimental psychology and economics have investigated play in prisoner's and social dilemma games when beliefs about others' moves are elicited. One strand of literature in experimental psychology has focused on demonstration of the *false consensus* effect; that players guess others will play as they themselves intend to play (Kelley and Stahelski, 1970; Kuhlman and Wimberley, 1976; Dawes et al., 1977; Messé and Sivacek, 1979; Wade-Benzoni et al., 1996).

Evidence from these studies of a positive correlation between individuals' beliefs of others' actions and their own was typically taken as support for the false consensus effect. However, this correlation could also be caused by a reciprocity or matching effect; their beliefs about others could cause their actions. This relationship and alternative hypothesis is explored more completely in Croson (1998). In all of these studies, the correlation between an individual's beliefs and behavior is presented, but no comparison of games with and without such elicitation is made.

In a second strand of research in experimental economics, beliefs have been elicited in public goods games with interior Nash equilibria. Some researchers investigate step-level public goods games (with interior Nash equilibria) and compare behavior with expectations of others' behavior in an effort to categorize subjects' actions as best responses to their beliefs (Rapoport and Eshed-Levy, 1989; Suleiman and Rapoport, 1992; Rapoport and Suleiman, 1993; Offerman et al., 1996). None of these studies compare games with and without the elicitation of beliefs.

Only one previous study, Dudley (1993), compares an elicitation with a non-elicitation treatment. The game used is a nonlinear public goods game with an interior Nash equilibrium. Dudley finds, consistent with results presented here, that fewer subjects act as if they are social welfare maximizers when forecasts are elicited than when they are not. [1]

2.2. Controlled beliefs

Along a similar vein, research in experimental common pool resource games has investigated the effect on an individual's behavior of receiving false feedback about what their counterparts are doing (Messick et al., 1983; Schroeder et al., 1983; Poppe and Utens, 1986; Fleishman, 1988). These studies generally show a positive correlation between the feedback received and the actions taken consistent with either false consensus or matching/reciprocity. Weimann (1994) used a similar tactic in a linear public goods game. He deceived subjects about the past contributions of others in their groups and showed a positive correlation between subjects' actions and the feedback they had received. Another study in this line of research controls subjects' beliefs in the prisoners' dilemma game used in study II of this paper. Shafir and Tversky (1992) found that subject who were told what their counterpart had done were significantly more likely to defect than subjects who were not told. Unfortunately, their study does not directly address the question of the impact of belief elicitation.

While other researchers have elicited or controlled subjects' expectations of others' contributions in public goods games, this paper is the first to explicitly address the effect of these manipulations on equilibrium behavior.

3. Study I

3.1. The linear public goods game

The linear public goods game models voluntary contribution to a pure public good. Pure public goods are goods that are both *nonrival* and *nonexcludable.* [2]

[1] I thank an anonymous referee for calling this result to my attention.

[2] That is, multiple agents can consume the good at the same time (nonrival) and it is not possible to exclude agents who did not pay for the good from consuming it (nonexcludable).

Assume each player i in a group of N identical players has some endowment E_i which can either be contributed to a group account and used to produce units of a public good or can be privately consumed. Call the amount contributed to the group account by i, x_i. The individual's earnings from private consumption is simply some multiple of the amount privately consumed $k(E_i - x_i)$. [3] The individual's earnings from is a multiple of the sum of contributions to the group account by all participants $P\ (\Sigma_i x_i)$. [4]

There is a pure public goods problem whenever $k/N < P < k$. When $P < k$, contributing to the public good is never optimal for the self-interested individual. Contributing one unit to the public good earns a subject only P, and costs k. Thus the dominant strategy equilibrium of this one-shot game is to contribute zero to the public good. [5] When $k/N < P$, contributing to the public good is always optimal for the group as a whole. Contributing one unit to the public good costs an individual k, but earns NP for the group. Thus the social optimum of this one-shot game is to contribute all one's endowment to the public good.

3.2. Experimental design and procedure

This experiment involved two treatments, one with belief elicitation and one without. In each treatment 24 subjects were arranged in six groups of four; although subjects could recognize the other participants in the room they did not know exactly which individuals were in their groups. Subjects remained in the same groups and the same treatment throughout the entire experiment.

Subjects were paid a US \$5 show-up fee along with their earnings in the experiment. Average earnings were US \$14.30 (plus the US \$5 fee) for less than an hour of experimental time. The experiment was computerized; instructions were given through the computer screen; subjects entered their contribution decisions through the keyboard and, at the end of each period, feedback about their earnings was displayed on the screen. [6]

Following Andreoni (1988), subjects were told they would play one game of 10 periods. Full free riding is the unique Nash equilibrium of the finitely repeated public goods game as described above. At the end of the game, subjects were told there was time to play another identical game of 10 periods, and their earnings in the second game would be added to their earnings in the first game. Subjects were told that the groups remained the same across the two games. In addition there were three practice periods before the first game began to familiarize subjects with the computer program and the process. Subjects were not paid their earnings during the practice periods and no practice periods were run before the second game.

[3] Our assumption of k being a multiplier makes this public goods game linear. One can imagine k as a function, which would create a nonlinear game. When each individual has their own k_i, this game becomes asymmetric.

[4] P/k is a constant in a linear public goods game, and is often called the marginal per capita return (MPCR). It represents the marginal return to each individual on a contribution of one unit to the group account. One can imagine P as a function, which would again create a nonlinear game. When each individual has his own P_i, the game becomes asymmetric.

[5] In experimental implementations of this game, it is often repeated a finite number of times in order to allow subjects to learn the game (see Davis and Holt or Ledyard for a review). In this case, the unique Nash equilibrium of the game is full free riding (contributions of zero to the public good).

[6] Instructions and experimental protocols are available from the author, as is the raw data.

In each period of the game, each subject was endowed with 25 tokens which could be allocated either to a private account, which paid 2¢ per token to the individual only, or to a group account (the public good), which paid 1¢ per token to each of the four members of the individual's group. At the end of each period, subjects were told the aggregate contribution of the other three members of group, the total group contribution and their earnings. Experimental parameters like the size of the group and the MPCR were chosen to be consistent with previous experiments in this area (e.g. Andreoni, Croson, 1996).

Each period of this experiment incorporates a pure public goods problem. Under traditional assumptions of self-interest, regardless of the decisions of the other players, each individual strictly prefers to place all of their tokens in the private account, earning 2¢ per token, than in the group account, earning 1¢ per token. However the group as a whole earns 4¢ when a token is placed in the group account (1¢ to each of the four members) but only earns 2¢ when the token is placed in a private account.[7] Thus the equilibrium of the stage game is a dominant strategy equilibrium of full free riding. The equilibrium of the finitely repeated game is a unique Nash equilibrium of full free riding. In contrast, the pareto optimal outcome (which is not an equilibrium outcome) is for all subjects to contribute their entire endowment to the public good.

The control treatment consisted of multiple repetitions of the game. In the *guess* treatment of the experiment there was an additional estimation stage before each period. In this estimation stage, subjects guessed the total number of tokens the other three members of their group would contribute to the group account in the upcoming game.[8] Subjects earned 50¢ if their guess was exactly right. If their guess was a bit off, they earned 25¢ divided by the (absolute) distance between their estimate and the true contribution.

At the end of each round in this treatment, subjects saw their own guess, the true contribution of the rest of the group, their earnings from the guess and all the information available in the control treatment.

3.3. Results

Contributions: In the control treatment, contributions in this game are similar to those observed in other linear public goods games. Contributions begin at around half the endowment and decrease over the course of the game. In addition, the restart effect of increased contributions in period one of the second game as observed by Andreoni and Croson was also observed in this data.

Fig. 1 shows average group contributions in the two treatments as well as the average estimates from the guessing treatment.

Clearly, contributions in the guess treatment are robustly lower than those in the control treatment. We can compare these statistically. Table 1 presents a Wilcoxon test to compare independent observations of six groups per treatment in each period. In 17 periods out of 20, contributions are significantly lower in the guess treatment than in the control.

[7] In our previous notation, $k=2$, $N=4$ and $P=1$. As required, $k/N < P < k$.

[8] Since they estimated the contributions of the other three members of the group, subjects could not influence the accuracy of their guess by strategically changing their own contribution.

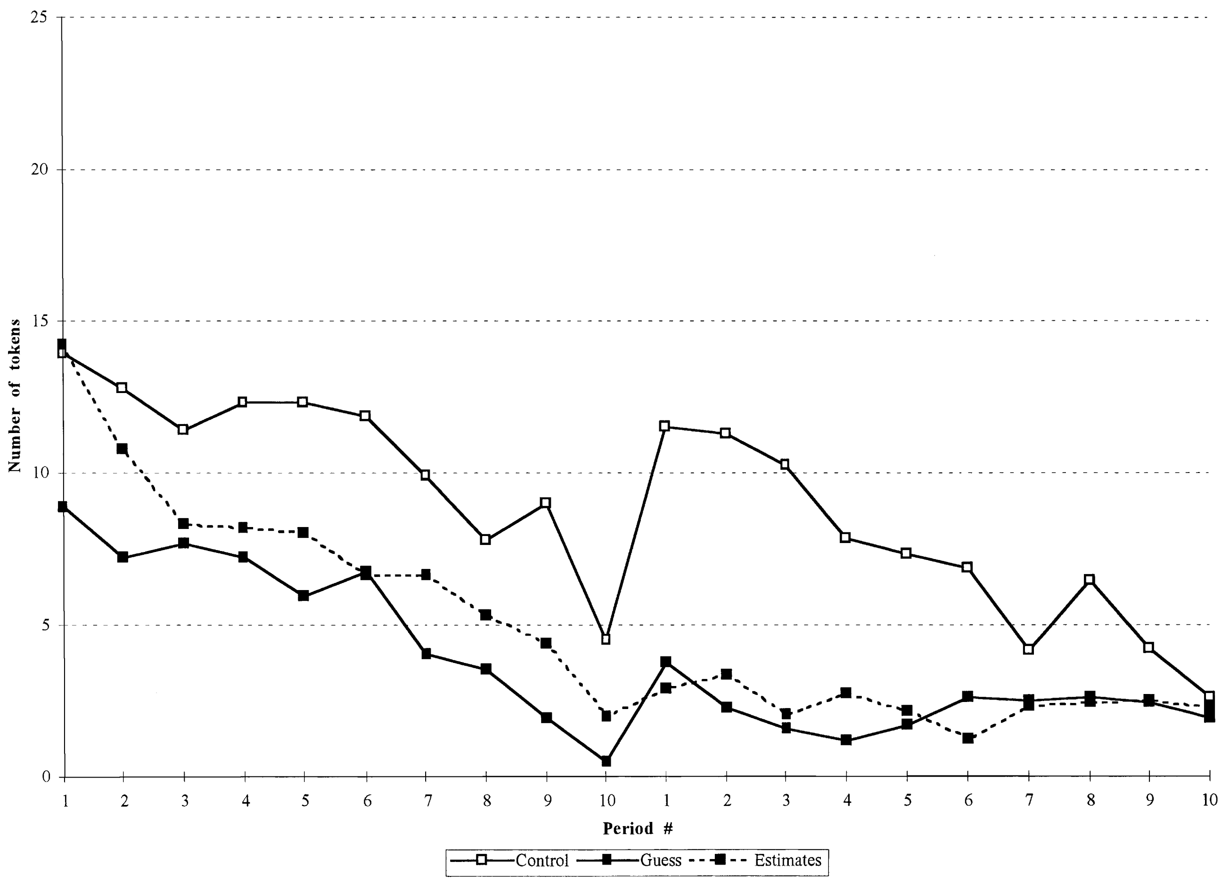

Fig. 1. Average contributions and estimates.

Table 1
Average tokens in public good

	Round									
	1	2	3	4	5	6	7	8	9	10
Control	13.96	12.83	11.42	12.33	12.33	11.88	9.92	7.79	9.04	4.54
Guess	8.92	7.25	7.71	7.25	6.00	6.79	4.08	3.58	1.96	0.54
Difference	5.04*	5.58*	3.71*	5.08*	6.33*	5.08*	5.83*	4.21	7.08*	4.00*
Restart control	11.54	11.33	10.29	7.88	7.33	6.88	4.21	6.50	4.25	2.67
Restart guess	3.79	2.29	1.58	1.21	1.71	2.63	2.54	2.67	2.50	1.96
Difference	7.75**	9.04**	8.71**	6.67**	5.63**	4.25**	1.67*	3.83**	1.75*	0.71

* $p<0.05$; **$p<0.01$.

In addition, if we pool the data over games and again use a Wilcoxon test (on non-independent observations), we find a similar statistically significant difference between the treatments ($n=120$, $m=120$, $z=7.19$, $p=0.0001$).

Free riding: There is also significantly more full free riding (contributions of zero) in the guess treatment than in the control treatment. Table 2 shows the proportion of subjects free riding in each treatment. A *t*-test of proportions compares period-by-period free riding in the two treatments. In 16 out of the 20 periods, there are significantly more free riders in the guess treatment than in the control treatment. [9]

In addition if we pool the data over games and use the same test (on non-independent observations), we find a similar statistically significant difference between the treatments. In the control treatment, 27.5% of the contributions made were contributions of zero, in the guess treatment 56.5% were ($n=480$, $m=480$, $t=9.51$, $p=0.0000$). [10]

These differences in contribution behavior between the treatments speaks directly to our motivating question. Asking subjects their beliefs about the contributions of others significantly affects their own contributions. The behavior we observe in the guess treatment is much closer to the equilibrium than has been observed in previous linear public goods experiments and is significantly different than behavior in our control treatment as well.

Estimate accuracy: Each subject in the guess treatment estimated what others in the group would contribute to the group account. An other interesting question involves the accuracy of these estimates. Fig. 1 shows the estimated contribution level, in comparison with the actual contributions in the guess treatment. [11]

Fig. 2 shows the average absolute estimation error made by each individual in each period of the game. This error is calculated by computing the *absolute* error of each subject in each period (the distance between their guess and the other three subject's actual contributions) and averaging over all the subjects. If all subjects were extremely bad guessers this average

[9] The *t*-test examines differences in proportions between treatments, see Andreoni.

[10] An anonymous referee wondered if the decrease in contribution level between the two treatments could be explained wholly by the increase in free riders; that is, more people free ride in the guess treatment, but the contributions of those who contribute is unaffected. This question can be answered by removing the contributions of zero from our sample, and comparing the contribution levels of the remaining decisions. When we do this, the average contribution in the control treatment is still higher than that of the guess treatment (control: 12.34 tokens, guess: 8.84 tokens).

[11] Generated by averaging the estimates over all the subjects and normalizing by dividing by three.

Table 1
Proportion of full free-riding (zen contributions)

	Round									
	1	2	3	4	5	6	7	8	9	10
Control	0.08	0.13	0.21	0.17	0.17	0.17	0.25	0.42	0.42	0.54
Guess	0.21	0.29	0.25	0.38	0.38	0.42	0.50	0.42	0.63	0.83
Difference	−0.13	−0.17^	−0.04	−0.21*	−0.21*	−0.25*	−0.25*	0.00	−0.21^	−0.29*
Restart control	0.13	0.21	0.13	0.21	0.33	0.21	0.42	0.29	0.42	0.63
Restart guess	0.58	0.63	0.67	0.63	0.71	0.71	0.75	0.75	0.79	0.79
Difference	−0.46**	−0.42**	−0.54**	−0.42**	−0.38**	−0.50**	−0.33**	−0.46**	0.38**	−0.17

^ $p < 0.10$; *$p < 0.05$; **$p < 0.01$.

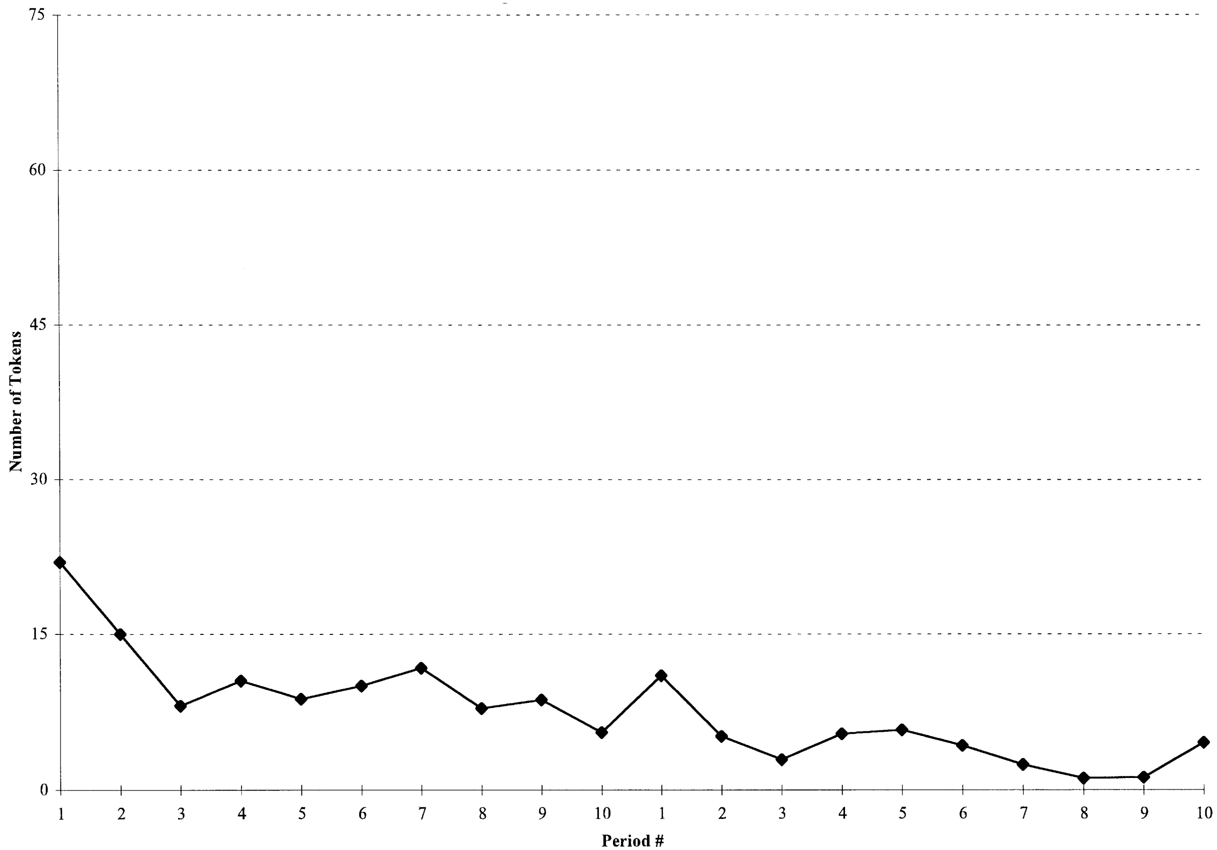

Fig. 2. Average absolute estimation error.

absolute estimation error could be as high as 75. Instead subjects appear to be fairly accurate in their estimations of others' behavior.

We can define a given subject's estimation error as the difference between that subject's estimate in a given period and the actual contributions of the other members of their group in that same period. Over all 10 rounds of the first game, six out of 24 subjects exhibited significantly positive levels of error (in particular, they were overoptimistic; they believed others would contribute more than they actually did). In the second game, no subjects exhibited significant levels of error in either direction. [12] Nonetheless, the act of estimating did appear to affect contributions.

3.4. Conclusions and discussion

Results from Study I demonstrate that eliciting subjects' beliefs of what their counterparts will do in a linear public goods game significantly affects their contributions and moves their behavior closer to that predicted by the unique Nash equilibrium. Why might this be so?

One possibility is an income effect. The gamble earned from the guess could lead subjects to act more selfishly. Since the earnings from the guess are not revealed to the subject until after their own contribution decision, when subjects in the guess treatment make their contribution decision they are richer than subjects in the control treatment by some uncertain amount (they own a gamble with all nonnegative realizations). For this to lead to less contribution rather than more, we need either increasing returns to wealth or increasing risk aversion, neither of which is the normal assumption. Thus while this explanation is possible, it does not seem particularly likely.

Another explanation is that the result is an artifact of the payment procedure for elicited beliefs. In particular, if subjects are focused on earning money for accurate guessing rather than for contributions, they have a collective monetary incentive to contribute a stable amount over the course of the experiment (in order to report more accurate beliefs). While the group would be better off if this stable amount were the social optimum of 25 tokens, each individual has an incentive to free ride and contribute less. Thus it may be that the free-riding outcome of zero tokens contributed is the only stable behavior which subjects can agree upon, and that is primarily what is observed. If this explanation were true, we would expect contributions to be more stable in the guessing treatment than in the control treatment. However, the variance of contributions in the guessing treatment was not significantly different that that of the control treatment (average variance of group contributions in control: 24.7; in guess: 14.2; $t=1.29$, $p=0.225$).

A final and more likely explanation is that asking subjects to think about what others in their group will do encourages them to think about the problem differently, and in particular, to think more like a game theorist. Subjects recognize that, regardless of their counterpart's decisions, they earn less by contributing to the group account. Eliciting beliefs thus focuses them on the dominant strategy equilibrium of free-riding, leading to lower contributions, less socially efficient outcomes and more equilibrium behavior.

[12] The hypothesis that the mean of the distribution of errors in the first game is equal to zero can be rejected at the 5% level for six out of 24 subjects. It cannot be rejected for any subjects in the second game.

	Cooperate	Defect
Cooperate	x, x	z, w
Defect	w, z	y, y

Fig. 3. A standard prisoner's dilemma.

A further question here is whether subjects anticipate this effect, not in their own behavior but in the behavior of others. Unfortunately, to answer this question we would need to compare beliefs of others' contributions between the two treatments directly. However, we know of no way to elicit beliefs in a non-elicited control treatment.

In the next study, we demonstrate a similar effect in a related setting. Here, subjects play a prisoner's dilemma game. In the control treatment, their beliefs of their counterpart's moves are not elicited, while in the guessing treatment, they are. Unlike the first study, no payment is offered for accuracy of beliefs. If the differences in behavior observed in Study I are being caused by the payment procedure, similar differences should not be observed in this new setting.

4. Study II

4.1. The prisoner's dilemma game

The prisoner's dilemma game has been studied extensively in both the experimental economics and psychology literature. The game was developed by Tucker (1950), and has been used to describe strategic situations from prisoner confessions to the arms race. The standard game involves two players, each with two moves; cooperate or defect. In the symmetric game, payoffs are as in Fig. 3 where $w>x>y>z$. The game has a dominant strategy equilibrium of defect/defect, although both players would be better off if they cooperated. This game thus has a similar structure to the linear public goods game studied above; a unique Nash equilibrium and a pareto optimal outcome which is not an equilibrium. The prisoner's dilemma has motivated literally hundreds of experiments; a review can be found in Rapoport and Chammah.

4.2. Experimental design and procedures

Eighty subjects from the University of Pittsburgh were recruited via posters and phone calls to participate in this experiment. Subjects were equally divided between two treatments. In each, subjects entered the room and were randomly assigned to a computer terminal. Instructions were displayed on the terminal, and a short quiz was administered to ensure subjects understood the payoffs in the experiment. Only after each subject had successfully completed the quiz did the experiment begin.

(a)

	Cooperate	Defect
Cooperate	75,75	25,85
Defect	85,25	30,30

(b)

	Cooperate	Defect
Cooperate	85,65	35,75
Defect	95,15	40,20

Fig. 4. (a) The symmetric prisoner's dilemma; (b) The asymmetric prisoner's dilemma.

In each treatment, subjects were assigned an ID number and to the role of 'row player' or 'column player'. Each session involved 10 row and 10 column players (two sessions for each treatment were run). Subjects played one game against each of 10 different counterparts; at the top of each game screen subjects were informed of the ID number of their current counterpart. Subjects never met the same counterpart twice. At the end of each game, subjects were reminded of their move, and informed of their counterpart's move; their earnings in that game were then added to their cumulative earnings.

In the first, *control*, treatment subjects played 10 different games, were paid their earnings in the games and dismissed. In the second, *guessing*, treatment subjects played the same 10 games but prior to each game were asked for their best guess of what their counterpart in the game would do. The addition of the guessing stage, its accompanying instructions and the recording of the guesses were the only differences between the treatments. The games of interest were interspersed with other, non-dilemma games. For the purposes of this paper, we will discuss results from only two of the 10 games played; a symmetric and an asymmetric prisoner's dilemma. [13]

In the symmetric prisoner's dilemma, subjects were presented with the game in Fig. 4a. In the asymmetric prisoner's dilemma, subjects were presented with the transformed matrix in Fig. 4b to which 10 points were added to all the column player's payoffs and subtracted from the row player's payoffs. This transformation does not change the structure of the game, the dominant strategies or the equilibrium. In the experiment, the choices subjects could made were depicted by letters or numerals (row players could choose rows A or B; column players chose columns 1 or 2), however for expositional purposes, here the actions retain their traditional names of cooperate or defect.

Experimental sessions lasted less than 1 h. Subjects earned a show-up fee of US $3 and an average of US $10.02 for their play in the session (each point in the matrices was worth 2¢ to the subjects). Subjects were asked not to discuss the experiment with other participants and not to reveal their ID number to anyone, even after the experiment had ended.

[13] Croson (1999) analyzes results of the other games in the experiment.

4.3. Results

Cooperation decisions: Our main question of interest is the existence of a significant difference in behavior between the guessing and control treatments consistent with the results of our first study, even though subjects were not compensated for accurate guessing. We find such a difference in both the symmetric and asymmetric prisoner's dilemma games run in this study.

In the symmetric prisoner's dilemma there were no significant differences in behavior between row and column players. Thus the data are pooled over 40 subjects in each treatment.[14] The rate of cooperation in the control treatment is 77.5%. The rate of cooperation in the guessing treatment dropped to 55%. A one-tailed *t*-test of proportions shows this difference to be significant at the 5% level ($t=2.19$, $p=0.0157$).[15]

In the asymmetric prisoner's dilemma there were again no significant differences between decisions of row and column players, thus the data are pooled over 40 subjects in each treatment. Although cooperation rates in this game were somewhat lower than those in the symmetric game, a similar difference between the treatments was found. In the control treatment, the rate of cooperative play was 63.5%, while in the guessing treatment it was only 42.5%. Using the same test as above, this difference is also significant at the 5% level ($t=1.83$, $p=0.0357$).

Relationship between estimate and decisions: Another interesting factor in this experiment is the relationship in the guessing treatment between subject's guesses and their actions. As in other social dilemma experiments, subjects in the symmetric prisoner's dilemma game reciprocated their counterpart's expected action (Dawes et al.). When subjects guessed their counterpart would cooperate, 83% of them cooperated themselves. When subjects guessed their counterpart would defect, only 32% of them cooperated ($t=3.89$, $p=0.0002$). The contingency correlation coefficient between guesses and actions is 0.461, significantly different from zero at the 1% level ($\chi^2=10.8$, $p<0.01$).[16]

Evidence of the link between beliefs and choices is somewhat weaker in this asymmetric version of the prisoner's dilemma. Of the subjects who claimed they expected their counterpart to cooperate, 56% of them cooperated themselves; of those who claimed they expected their counterpart to defect, 32% cooperated, a not-quite significant difference with this sample size ($t=1.55$, $p=0.0651$). The contingency correlation coefficient between guesses and choices is 0.295, which is significantly different than zero only at the 10% level ($\chi^2=3.8$, $p<0.10$).

4.4. Conclusions and discussion

Results from this experiment demonstrate that the decrease in contributions when beliefs are elicited, observed in Study I, is a robust finding. Subjects were significantly less likely to cooperate in a prisoner's dilemma game where their beliefs about what others would do

[14] Utility theory suggests that choices between row and column players should not differ, as payoffs are either identical or, below, simply an affine transformation. Indeed, empirically they do not.
[15] A one-tailed test is appropriate in this setting, given our directional results of the first study.
[16] For a description of the contingency correlation coefficient, see Siegel (1956), pp. 196–202.

were elicited without payment than they were in the same game where those beliefs were not elicited. Results from this study also demonstrate a significant and positive relationship between what a subject believes their counterpart will do in the game and what the subject does, consistent with previous studies in this area.

As in Study I, in this experiment significantly more subjects play the dominant strategy (defection) when asked to guess what their counterpart will do than when not asked to guess. Thus the subjects think more like game theorists when beliefs are elicited than when they are not.

5. General conclusions and discussion

These studies were designed to investigate the conditions under which experimental subjects are more or less likely to play equilibria of experimental games. Results of both studies suggest that eliciting subjects' beliefs about their counterparts' actions decreases contributions and cooperation, leading to less socially efficient outcomes but more equilibrium play.

These results have a number of important implications. First, they can help us to better describe the diverse behavior of experimental and other decision-makers. If players in strategic situations act differently when asked to estimate the actions of their counterparts, we can better predict and explain their choices in those situations. Additionally, we can identify situations in which players are likely to defect or free ride (when an estimate of others' actions is natural and/or salient) and when they are instead likely to cooperate or contribute.

The second implication is prescriptive; this result can help us advise people about what to do, or what not to do, in strategic situations like those studied here. Previous work has suggested that having subjects simply think about a social dilemma problem they are facing does not change their play (Dawes and Orbell, 1982). However, results from these experiments suggest that thinking about what *others* will do in a game-theoretic setting moves subjects' actions toward that of the equilibrium. If this is so, applying the methodology of 'what do you think your counterpart will do' to other games or to real-life strategic decisions may move decision-makers closer to equilibrium play.

The final implication is methodological. If the act of eliciting beliefs affects behavior, experimentalists need to think carefully about their procedures and the information collected in their experiments. In particular, they need to examine and take into account the possible effects of their elicitation procedures on behavior.

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