Thinking Like a Game Theorist: Comment

by

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Abstract

In a recent paper in this journal, Rachel Croson shows that when subjects' beliefs about the contributions of others are elicited in a voluntary contributions public goods game, those subjects contribute less than do other subjects in the same game when beliefs are not elicited. We report briefly on a similar experiment where we do not observe this. There are small differences between this experiment and Croson's that may explain these different results, but none of these explanations are completely satisfying. So while Croson shows that belief elicitation procedures may have unwanted effects, this does not always occur; and it may be difficult to predict when and where belief elicitation procedures will have such undesirable effects.

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In contemporary game theory, players' beliefs about the likely play of other players are frequently thought to be crucial determinants of game play. As a result, researchers have begun to elicit subjects' beliefs about the likely play of other subjects in game experiments (McKelvey and Page 1990, Offerman et al. 1996). Is it possible that asking subjects about their beliefs can change the way those subjects play a game? There are many reasons for suspecting this, not least of which is that asking subjects to think about their beliefs may cause them to "think like a game theorist;" and in a recent number of this Journal, Rachel Croson (2000) provides evidence that this can indeed happen. This is a serious issue since it suggests that belief elicitation procedures may have unintended effects on game play itself, and that past studies of game play that employed those procedures may have been flawed. But we report results which show that Croson's results are not completely general: In a game similar to Croson's game, our data show no evidence that belief elicitation alters the play of subjects. Taken together, our work and Croson's work leave a small puzzle for future researchers: While there are some differences between Croson's game and our game that may be responsible for our differing results, no completely satisfying explanation emerges from our examination of those differences.

The games are both public good games using the well-known "voluntary contribution mechanism", which we refer to as the VCM game. In the games discussed here, each player i, $i = 1, 2, 3,\ldots N$, receives an endowment of E tokens in each of $p = 1, 2, 3,\ldots P$ consecutive periods of the game. Each player i decides how to allocate E between a private and public good in each period p—how many tokens to "keep" (that is, allocate to their private good) and how many to "contribute" (that is, allocate to the public good). Letting X_{ip} denote the number of tokens subject i decides to contribute to the public good in period p, aggregate contributions to the public good in period p are $X^A{}_p = \Sigma_j X_{jp}$. In such a linear and symmetric version of general VCM games, the earnings of subject i in period p are given by $\Pi_{ip} = \alpha(E-X_{ip}) + \beta X^A{}_p$, where α is the per-token return to a subject's private good allocation and β is the subject's per-token return from aggregate contributions to the public good. Rational and self-interested players have a dominant strategy to keep all tokens whenever $\alpha > \beta$, but group rationality dictates that all players contribute all tokens whenever $\alpha < N\beta$. Table 1 shows the differences between Croson's and our own parameter choices in these two VCM game experiments, as well as other design differences. For now, we point out that our choice of endowment E is a single token. This makes subjects' choices simple binary ones,[2] and makes the reporting of behavior particularly simple as a contribution rate, rather than a distribution of contribution levels.

We have two experimental treatments. One is a standard VCM game.[3] The second otherwise identical treatment involves the elicitation of each subjects' belief about the contributions of other subjects to the public good at the beginning of each period of play (prior to making their own contribution decision), as described in this instruction excerpt:

> "[Y]ou will have an opportunity to receive a ten dollar bonus by correctly guessing how many of the <u>other</u> players will contribute their [token] to the group account. Obviously, since there are six of you, the number of other players who will contribute is a number somewhere between zero and five. You should pick one of these numbers before you decide whether to keep or contribute in each round…[After other payments have been made to all of you at the end of this session], we will draw a card to select one of your player numbers, and that person will come to the front of the room. We will then draw a card one more time, to select one of the six rounds. We will then look at this player's response form for the round. If that player correctly guessed how many other players would contribute in that round, that player will receive an extra ten dollars.[4]

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[2] The single-token or binary choice paradigm in VCM games is quite common and many of these experiments involve belief elicitation. See, for instance, Kuhlman and Wimberly (1976), Dawes et al. (1977), Rapoport and Eshed-Levy (1989) or Offerman et al. (1996). In some literatures, some of these games are known as "social dilemma" games but their structure is identical to a binary choice VCM game. Of these, the game studied by Dawes et al. is most similar to ours. Kuhlman and Wimberly's game is a two-player game presented as a normal-form prisoner's dilemma, while we have six players and present our game as a VCM game. The games of Offerman et al. and Rapoport and Eshed-Levy are "step-level" public goods provision games—unlike the linear provision game we use.

[3] Such experiments are widely known, but see Ledyard (1995) for a review of procedures and findings.

[4] Clearly this procedure is not a "proper scoring rule" (Aczel and Pfanzagl 1966). However it is an incentive-compatible way of eliciting subjects' beliefs about the mode of the distribution of contributions if risk preferences

Subjects in all sessions were paid \$5.00 for participation plus their total money earnings from game play itself (a total of \$9.00 to \$13.00 per subject);[5] and sessions lasted less than an hour.

Table 2 shows the rate of contribution to the public good in each period, and the rate across all periods, by treatment. There is no significant difference between the contribution rates in the two treatments either across all periods ($\chi^2_1 = 0.077$, $p = 0.78$) or period by period ($\chi^2_6 = 4.151$, $p = 0.66$). Thus we find that in our game, overall levels of contributions, as well as the shape of the path of decay of contributions, are equivalent under the two treatments.[6] This contrasts with Croson's results: She observes a significant <u>decrease</u> in the level of contributions in her treatment where beliefs are elicited.[7]

We thought that belief elicitation procedures would cause subjects to play games more in accordance with belief-based theories than other kinds of theories, so we were surprised by our negative findings. Therefore, let us compare our work with Croson's work and reflect on some possible sources of our differences. The incentive to free ride is stronger in our design than in Croson's (see Table 1), and this may leave belief elicitation more room to have a depressing effect on contributions in Croson's game. Contributions <u>are</u> generally higher in Croson's "no beliefs" treatment than in ours (56% of total endowments versus 42%, respectively, in the first period). But in Croson's game, belief elicitation reduces contributions in every one of the first

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obey only first-order stochastic dominance, and it is highly transparent. Proper scoring rules require expected utility behavior and an assumed or measured risk attitude, and they are less transparent to subjects.

[5] Additionally, in two of the eight "beliefs" sessions, the randomly selected subject received the \$10 payment for correct prediction in addition to her other earnings.

[6] We have looked for other subtler differences between the treatments, such as in: (a) The dynamics of contributions—that is, the first-order autoregressive characteristics of aggregate contribution rates; (b) the variance of subject-specific overall contribution rates; and (c) the variance of session-specific overall contribution rates. Under various readings of various theories of contribution behavior in VCM games and the effects of belief elicitation, such differences could be of interest (Feltovich and Wilcox 1999); but none of these potential differences are reliably significant between the treatments either.

[7] However, other ancillary results of ours are in agreement with Croson's results. For instance, we observe a similarly significant, though not enormous, upward bias in subjects' beliefs about the contribution rates of other subjects (in the treatment where beliefs are elicited). Because of this, and because of the broad similarity of our own

ten periods—even in the 10^{th} period where contributions under beliefs are only 18% of the total endowment. Therefore we think this potential culprit can be dismissed.

Although Croson's belief elicitation procedure (like ours) simply elicits a most likely level of contributions by others, its reward mechanism is more complex (see Table 1): Croson rewards subjects not only for an exact prediction (as we do) but for close prediction, too (as we do not). If belief elicitation—or any additional cognitive task—has an effect on play, this is most likely because that task makes some fact about a game, or approach to playing a game, relatively more salient to the subject. Anything that deepens or broadens the demands of such a task (simply even the longer instruction required to communicate a more complex reward scheme) might be expected to increase the likelihood of such effects. It is also possible that the structure of the games themselves can reinforce these effects. In our game, subjects only need to consider the binary "contribute or keep" decision of five other subjects, while in Croson's game they must consider the whole spectrum of contribution decisions from zero tokens to the entire twenty-five token endowment for three other subjects (see Table 1). Put differently, belief formation in our experiment only requires subjects to ask "Will others contribute?" while Croson's experiment additionally requires subjects to ask "How much?

There is a problem with these kinds of explanations, however, since Croson also examined behavior in two versions of a two-player, single-shot prisoner's dilemma game and also found that in treatments where subjects are asked to guess the likely play of their opponents, cooperative behavior is significantly less common. Moreover, subjects were not rewarded for correct guesses in these treatments. Since these prisoner's dilemma games are simpler than our VCM game, both in terms of game structure (just two players) and the belief elicitation scheme

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and Croson's basic results with previous results from VCM games (Ledyard 1995), we doubt that our different results on the effect of belief elicitation are due to fundamental problems in either our, or Croson's, experiments.

(no reward mechanism), we are not satisfied with the arguments of the last paragraph. It has, however, been suggested by Hogarth (1975) that complexity and cognitive effort are not normally monotonically related and that effort typically peaks at an intermediate level of complexity. Perhaps our VCM game lies at such an intermediate level of complexity where cognitive effort is already relatively high and where beliefs are already being considered by subjects, so that eliciting beliefs causes no cognitive change; while Croson's VCM game and prisoner's dilemma game lie at higher and lower levels of complexity where effort is relatively low and the addition of belief elicitation procedures can have an impact on cognition.

In sum, Croson's results clearly show that belief elicitation procedures can produce unwanted artifactual changes in subjects' game play. But we have shown that these effects need not occur in a game that is quite similar to Croson's, so it is less clear whether past studies that have utilized such procedures have been flawed. It is also unclear whether, and under what conditions, these procedures will generate game play artifacts since the small differences between our game and Croson's games do not suggest a fully satisfying explanation for our differing results. The factors governing belief elicitation's possibly unwanted effects may be highly idiosyncratic to particular games[8], and perhaps even to different experimenters and subject pools as well.

[8] For instance, we have examined four sessions each of two treatments of the "guessing game" (Nagel 1995, also known as the "beauty contest game"), one with and one without belief elicitation procedures. In that game, belief elicitation procedures moved subjects' decisions <u>further</u> from the iterated dominance-solvable solution of the game, unlike both Croson's results and the results we report here.

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Table 1

Differences in Procedures, Design and Subjects: This Study Versus Croson (2000)

Feature	Study	
	Croson (2000)	This Study
Number of players (N)	4 players	6 players
Endowment (E)	25 tokens	1 token
Private good return (α)	2 cents per token	100 cents per token
Public good return (β)	1 cent per token	25 cents per token
Relative incentive to free ride (α/β)	2	4
Number of periods (P)	10 periods followed by an extra 10 periods announced only after period 10 is played	6 periods
Belief Elicitation Mechanism and Rewards	50 cents for exact prediction, declining linearly from 25 cents for close predictions; all subjects are rewarded for all periods played.	$10 for exact prediction only; one subject randomly selected for this at the end of the experiment, and for just one randomly selected period.

Table 2

Contribution Rates by Treatment

	Treatment	
	Belief Elicitation	No Belief Elicitation
Contribution rate in period p:		
Period 1	0.459	0.416
Period 2	0.354	0.333
Period 3	0.229	0.333
Period 4	0.188	0.271
Period 5	0.313	0.208
Period 6	0.125	0.167
Contribution rate across all periods:	0.278	0.288

Notes: Across all periods, the rates are based on 6 subjects · 8 classes · 6 periods = 288 observations in each treatment. In each period, the corresponding number is 48 observations.