Encouraging cooperation: revisiting solidarity and commitment effects in prisoners' dilemma games

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Encouraging Cooperation: Revisiting Solidarity and Commitment Effects in Prisoner's Dilemma Games

Matthew Mulford and Jonathan Jackson

London School of Economics and Political Science, London, UK

Henrik Svedsäter

University of Göteborg**,** Göteborg, Sweden

Abstract

Pre-play discussion consistently increases cooperation in dilemma interactions. Most explanations of this "cheap talk" effect are based on either commitment or group solidarity effects. Because discussion about the upcoming dilemma allows participants to both make promises and creates group solidarity, the two explanations are often confounded. This paper aims to clarify past results by having participants engage in an "unrelated" discussion prior to a dilemma interaction. We find that solidarity effects can be induced by minimal group categorizations but are relatively weak. Discussions involving consequential but unrelated coordination tasks are shown to prime cooperative norms and increase cooperation with both in-group and out-group members. Our findings suggest that cheap talk may work for even cheaper reasons than previously thought.

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Social dilemmas are situations where behaviour motivated by individual maximisation leads to socially sub-optimal outcomes. Traditional solutions to social dilemmas rely on changing the consequences of non-cooperative and cooperative acts so that rational individuals will act in ways that promote the public good. Solutions based on this notion of incentive compatibility achieve their intended result either by adding individual costs to defection (acting in ways that are detrimental to the public good), or adding individual benefits to cooperation in order to make that choice a non-dominated strategy.[1]

A second major class of incentive compatible solutions relies on non-myopic reasoning--taking into account how one's present behaviour will affect *future* interactions (Axelrod, 1981, 1984; Frank, 1988; Hardin, 1982; Rapoport & Chammah, 1965; Trivers, 1971). Unlike traditional incentive compatibility, this class of solution does not require changing outcomes such that any single cooperative act becomes compatible with self-interest. Rather, it relies on iterated play and the consequences that has for rational strategies across a series of choices. Both traditional and non-myopic rational choice theories often solve social dilemmas by making individually maximising behaviour compatible with the advancement of public welfare.[2]

This paper investigates another solution to the dilemma problem--pre-play communication. Allowing dilemma players the opportunity to engage in pre-play discussion consistently increases the incidence of cooperation in non-iterated dilemma interactions (see Caldwell, 1976; Caporael, Dawes, Orbell, & van de Kragt, 1989; Chen, 1996; Dawes, Mactavish, & Shaklee, 1977; Issac & Walker, 1988; Ledyard, 1995;

Liebrand, 1984; Messick & Brewer, 1983; Orbell, van de Kragt, & Dawes, 1988; Orbell, Dawes, & van de Kragt, 1990; Parks, Henager, & Scamahorn, 1996; Sally, 1995). From the perspective of game theory, this finding is anomalous. Pre-play discussion does nothing to alter the dilemma's incentive structure (objectively defined). Hence, pre-play discussion is, according to economic theory, cheap talk. Given the importance of finding solutions to dilemma interactions, an understanding of why cheap talk works to increase cooperation is important. This is particularly true since pre-play discussion increases cooperation by the relatively low cost act of communication.

Decades of research on this topic (for reviews, see Bicchieri, 2002; Kerr, Garst, Kiehle, & Harris, 1997; Sally, 1995) have ruled out all but one explanation: the commitment effect. Study after study has found that allowing people to discuss the upcoming dilemma dramatically increases cooperation--in some studies to close to 100% (for a review see Sally, 1995). The most common explanation is that discussion provides players the opportunity to make promises to cooperate that, under certain circumstances, acquire force. The act of publicly committing to a future act will lead people to feel a need to keep their word. Such promises also provide a basis for expectations that others will also be so bound (Kerr & Kaufman-Gilliland, 1994; Ledyard, 1995; Palfrey & Rosenthal, 1991).

This is largely seen as a settled issue: only the commitment of promise-making in pre-play discussion elevates cooperation rates in social dilemmas. Competing explanations of group identity and cooperative norms have seemingly been discounted. But we are not so sure. In this paper we argue that it is premature to discount group identity and cooperative norm effects. We demonstrate that key studies have suffered

from methodological and conceptual problems. We present an experiment that we believe more effectively disentangles competing explanations.

The main finding is that a coordination task conducted by the participants prior to the dilemma situation induced higher rates of cooperation. Although being relatively weak, a solidarity effect through group discussion was also noticeable among participants. Cooperation rates were affected both between in-group and out-group members among participants. Hence, the effect was not limited to individuals toward whom the participant has interacted with prior to the dilemma task. Since commitment by definition should only affect behaviour toward in-group members, this alone cannot explain the present findings.

Group Identity Effects and Cooperative Norms

One reason why pre-play discussion may increase cooperation rates is that the discussion itself creates group identity. Making common category membership salient has been shown to increase levels of contribution to that group's welfare in some circumstances (e.g., Brewer & Kramer, 1986; Brewer & Schneider, 1990; DeCremer & Van Vugt, 1999; Kaori, 1999; Kramer & Goldman, 1995; Thompson, Kray, & Lind, 1998; Wit & Wilke, 1992). The social identification effect is often explained using the theories of social identity (e.g., Tajfel & Turner, 1986; Turner, 1975; Turner, Brown, & Tajfel, 1979) and social categorization (e.g., Turner, Hogg, Oakes, Reicher, & Wetherell, 1987). Here the transition of self-perception from individual to group transforms goals or motives, placing greater weight to the outcomes of the group as a whole. In the context of a social dilemma, this may increase cooperation.

Another explanation for the effect of cheap talk is that communication primes cooperative norms. Discussing what they, as individual members of the group, "ought"

to do may prime norms of cooperation--i.e. universal cooperation leads to the best outcome for the group and therefore people ought to cooperate and ought not to free ride. These norms, once primed, would be followed and cooperation would increase.

In the environmental economics literature, interactions between decision makers often change the way in which they solve various conflicts between self-interested wants and environmental goals. For instance, prior group discussions tend to alter people's understanding of the public goods dilemma (Brouwer, Powe, Turner, Langford, & Bateman, 1999), to generate more thorough deliberation of the valuation task (Clark, Burgess, & Harrison, 2000; Macmillan, Philip, Hanley, & Alvarez-Farizo, 2002), and to favour non-consumptive, and hence less individual, perspectives on the decision problem (Kaplowitz & Hoehn, 2001). Overall, group contexts often lead to a greater emphasis being placed on collective rather than individual interests.

Elegantly designed experimental studies on social dilemmas have effectively ruled out group identity and cooperative norm explanations, at least in the eyes of most social dilemma researchers. In the following two sections we argue that these studies have methodological and conceptual limitations that mitigate the conclusions that are drawn.

Discounting Group Identity Effects

Perhaps the most straightforward way to disentangle group identity from commitment effects is to have participants engage in a group discussion *prior* to knowing they will face a dilemma on a topic not directly related to the dilemma. In this way, group identity may be created but commitments to cooperate cannot be made.

In one of the first examinations of the group identity hypothesis, Dawes et al. (1977) did just this by examining the effect of four pre-dilemma conditions on

subsequent cooperation rates: (a) the group had no opportunity to discuss anything (control), (b) the group discussed an unrelated issue (the estimated population of the State of Oregon), (c) the group discussed the dilemma, and (d) the group discussed the dilemma and each member answered a non-binding role call of intentions. Dawes et al. (1977) found that only when individuals discussed the dilemma did cooperation rates increase.

But asking a group to estimate the population of Oregon is not an obvious group-building exercise. Another, more relevant discussion topic could have created more group solidarity and affected cooperation rates. Brewer and Kramer show that group identity can be established when group members share a common fate (see for example Brewer & Kramer, 1986; Kramer & Brewer, 1984).

Orbell and Dawes (1992) tested this in a later experiment. Prior to the start of the prisoner dilemma study, and before the subsequent dilemma was explained, half of all participants completed a ten-minute discussion of a Schelling's (1960) coordination problem. In the control condition participants sat through a six-minute "quiet time". In all other aspects the conditions were identical.[3] Orbell and Dawes (1992) report no difference in cooperation rates between the groups who had discussed the coordination problem and the control group. For the authors, this demonstrates that discussing situations of shared-fate is not a sufficient condition to create levels of cooperation greater than observed in control groups.

Conceptually similar results are reported by Bouas and Komorita (1996), who found that a common fate condition was insufficient at inducing more cooperation. Only when associated with a discussion of a relevant issue did it alter the level of cooperation, thus lending some support to the argument that only commitment effects

matter. Finally, Kerr and Kaufman-Gilliland (1994) came to the problem of a different angle using a series of experiments where they manipulated the level of choice efficacy in examinations of cheap talk effects. They argue that if feelings of solidarity lead to concern for others' payoffs, which in turn lead to increases in cooperation, then as the efficacy of an individual act of cooperation falls, cooperation should be less likely. Yet, the degree of efficacy of cooperation did not mediate the effect of pre-play discussion.

On the basis of these findings one may conclude that common fate and other group identity effects are insufficient at inducing higher cooperation rates. However, note the difficulty of designing a task that elicits group identity. For instance, in the study of Orbell and Dawes (1992) the subjects were asked to imagine a situation in which they would share a fate with an imaginary other: this was not a personal shared fate of the group but rather a shared fate of a hypothetical other. This was not a demonstration that a consequential, as opposed to hypothetical, common fate manipulation would not be successful at eliciting greater cooperation.

Furthermore, in Bouas and Komorita (1996) a random event (in this case a lottery) was introduced in order to define overall payoffs, which is likely to reduce the incentives for cooperation since now the outcome does not rest solely on how participants act toward each other. A weakened or more ambiguous link between choices and outcomes has in other studies shown to reduce the degree of non-selfish behaviour, simply by altering the likelihood of and personal responsibility felt for the negative consequences of selfish actions (Dana, Weber, & Kuang, 2004; Svedsäter & Johansson, 2006). Finally, in Kerr & Kaufman-Gilliland (1994), participants were shown comments made by participants in other groups prior to the group discussion, some of which indicating how these groups went about to solve the dilemma problems.

In this case the potential effect of group identity is compounded with the normative influence exerted by the solution adopted by participants in other groups. There is also a framing effect potentially at work here in so far as these comments phrased the problem either in terms of group or individual benefit (that is, formulated either as *we will benefit or I will benefit*.)[4]

Group Identity and the Minimal Group Paradigm

So far we have seen the difficulty in eliciting group identity in experimental tasks. There is another aspect of group identity that lurks behind some of the studies so far discussed. Dawes et al. (1977) and Bouas and Komorita (1996) both find no difference in cooperation rates between unrelated discussion groups and control groups. Dawes et al. (1977) see this as evidence of insufficient group identity. Bouas and Komorita (1996) interpret this as evidence that group identity is itself insufficient to affect cooperation rates. Yet the findings are compatible with another, equally plausible explanation. We know from work by Tajfel and Turner (1979) that group identification, and subsequent in-group biases, can be formed by the most trivial distinctions. The fact that unrelated discussion groups in both studies cooperated no more than the control groups could mean, as they authors argue, that group identity was either insufficient, or that group identity itself is insufficient to affect choices. Another explanation consistent with the finding is that group identity had been created in both the control and the unrelated discussion conditions. Whatever gains in cooperation were to be had through the creation of group identity had been attained by *both* the control and the unrelated discussion groups, and thus they had the same cooperation rates.[5] In neither study did members of one group make decisions related to the outcomes of out-group members and therefore tests of this hypothesis were not carried out.

There is evidence of this minimal group effect from other studies. For example, in another important study investigating the effects of cheap talk on dilemma behaviour, Orbell et al. (1988) initially told participants that any surplus from a social dilemma they were to play would either go to themselves, or to an out-group. In no-discussion conditions, when the consequences of contribution decisions were to go to an out-group, the average contribution level was 19.6%. When the participants believed the surplus would go to their own group, 37.5% contributed. Recall that this was no-discussion condition. This indicates that some sort of minimal group effect was affecting choice *absent* discussion. Even if group identity effects are confined to modest, step level increases in cooperation, they are potentially important. Similar effects have been presented by Bohnet and Frey (1999), who showed that silent identification suffices to raise solidarity in prisoner's dilemmas and dictator games. While these outcomes may be confounded with identified victim effects, they still provide a powerful argument against the thesis that commitment effects constitute the sole explanation of increased cooperation.

Some models have been proposed aimed at explaining the effects of group identity. Lind & Tyler (1988) presents a group-value model where the extent to which one feels valued by the group is likely to affect perceptions of distributive justice. In interpreting that model, Hegtvedt, Clay-Warner, and Johnson (2003) argue that it is the sense of group membership that drives the desire for fair procedures and outcomes. This type of theorizing has been empirically investigated by van Dijk, Sonnemans, and van Winden (2002), showing that (a) through interaction people form positive or negative sentiments about each other, and (b) that these sentiments influence the extent to which individuals care about each others welfare. Interestingly, common group membership

also tends to enhance helping beyond the dyadic effects of interpersonal similarity (e.g., Flippen, Hornstein, Siegalm, & Weitzman, 1996), and has consequences for actions taken against individuals not part of the group (e.g., Dovidio, Gaertner, Validzic, Matoka, Johnson, & Frazier, 1997; Gaertner et al., 2000; for a review see Penner, Dovidio, Piliavin, & Schroeder, 2005). Thus, not only are the effects driven by attachment and sense of ownership. At the same time it seems to alter the reasoning and perception taken by individuals. The latter finding is especially important for the purpose of this study. It suggests that inducing a common group identity generalizes beyond the minimal group.

This sets this research apart from previous papers which have argued that only promise making or commitment directly related to the forthcoming task would affect people's behaviour. The question then is what underlies this effect. We believe it increases solidarity among the discussants, making them more supportive or cooperative toward both in-group members and outsiders, albeit perhaps to a different degree. Put differently, group discussion may sometimes alter the perspective taken by the participants, inducing them to think about themselves as group members rather than as isolated individuals. This, in turn, may cause major shifts in motives and behaviour. This is what is observed in various environmental valuation studies, where prior group discussions tend to alter participant's understanding of the decision problem, making them more focused on collective rather individually goals and objectives (e.g., Brouwer et al., 1999; Kaplowitz & Hoehn, 2001).

Discounting Cooperative Norm Effects

Another explanation of the cheap-talk effect is that communication primes cooperative norms. In the examination by Orbell et al. (1988), participants were initially

told that any surplus from a social dilemma they were to play would either go to themselves, or to an out-group. Half of the groups were then allowed to discuss the dilemma. In half of these "discussion" conditions, the participants learned that the recipient of their surplus had been switched. If originally they were told that it would go to the in-group, they were told it would go to the out-group and vice versa. Orbell et al. (1988) reasoned that if dilemma discussion worked to increase cooperation by priming cooperative norms, then these norms should create equal levels of cooperation, regardless of the recipient of the surplus. If discussion worked by creating group identity, then cooperation rates in groups who had, after the discussion, the recipient of the surplus switched from themselves to an out-group, should see a lower cooperation rates than groups who had not experienced this switch.

In the groups that did not experience the switch, the cooperation rate was 79%, once again finding that the dilemma discussion leads to high levels of cooperation. The cooperation rate in groups who experienced the switch was nearly 59%--a significant reduction. This difference led Orbell et al. (1988) to reject the effect of general norms of cooperation as a possible cheap-talk explanation.

This conclusion is problematic for a number of reasons. For a start, it could be the case that any cooperative norms created were contextually contingent. Bicchieri (2002) argues that there are "social norms"--those that are binding if and only if all relevant parties abide by them--and "personal norms"--those which are binding regardless of what others do. In both explanations, the groups define a social goal--cooperation contingent on high expectations of others doing the same. Promise making, in this explanation, serves the purpose of creating the expectation that others will cooperate. Using Biccieri's (2000) norm distinctions, the group's discussion primes a

social norm--mutual cooperation--and then it uses a personal norm--promise keeping--to reduce the perceived risk that others are free riding. When combined, these two effects create a very high level of cooperation. Yet, this does not rule out the possibility that the social norm is still influential among participants who do not have the opportunity to make promises.

This is, in fact, what we see in Orbell et al. (1988). In the second study by all participants discussed a dilemma together. The whole group (14 participants) were told that they would soon be divided into two groups of 7. The dilemma was framed as an investment problem. All individuals were given US$5 and were given the following choice: (1) Keep the US$5; give the US$5 to their own sub-group of 7 and each member of that group would receive US$2, or; give the US$5 to the other sub-group of 7 and each member of that group would receive US$3. To examine the effects of promise making, groups were stratified into three subsets: those where there was unanimous promise making to contribute to the other group; those where some promised, and; those where none promised. When there was unanimous promising to give to the other group during the whole group discussion the cooperation rate was 84%. The remaining two groups had indistinguishable levels of cooperation around 60%.

This leads Orbell et al. (1988) to two conclusions. First that universal promise making seems to be necessary to create sufficient levels of group identity to affect cooperation rates and that personal norms of promise making do not seem to increase cooperation. If they did, then those groups where some promised would have had higher cooperation rates than in those groups where none promised and they did not. Thus

norms, both social and personal, are rejected as possible explanations for cheap talk's ability to raise cooperation rates.

But this is only one interpretation of this finding. As previously argued, promises to uphold that norm would, of course, raise people's expectations that the norm would be followed and result in increased cooperation rates. Assuming some people would follow the cooperative norm if primed, minus the support of promises, we should see some increase in cooperation rates in situations where people can discuss the dilemma, but where relevant promise making is not possible.

This is what we see in Orbell et al. (1988). Recall that in the first of the two studies by them, cooperation was lower (59%) in groups which had discussed the dilemma but who had the recipient of the cooperative surplus switched to the other group than those groups where the surplus stayed in their own group (79%). Even though the rate is lower, it is still relatively high. This is particularly so if we look at the reported difference in cooperation between groups who were allowed discussion and experienced the switch versus those who were not allowed discussion and experienced the switch. In the later group, the cooperation rate was 30%. Thus, in comparable conditions, not discussing the dilemma leads to nearly a 50% drop in cooperation rates. Further, in the conditions where the groups were told their surplus would go to the other group, and who did not experience a switch, those who discussed the dilemma contributed more (30%) than those who did not (20%).

Finally, the cooperation rates in the second study of Orbell et al. (1988) for groups that did not have unanimous promise making, but were allowed to discuss the problem, are roughly the same as in the discussion plus switch condition in Study 1. These findings are consistent with a primed norm explanation. They are also consistent

with the authors' contention that unanimous promise making is necessary to produce high cooperation rates.

The norm based explanation put forward here is similar in some respects to the perceived consensus explanation. In both, cooperation has to be recognised as the correct thing to do and then people have to perceive that others will all cooperate. An important difference is that the perceived consensus explanation requires pre-play discussion about the dilemma itself. The norm-based explanation does not. What matters is not the content of the tasks performed, but rather that they draw on similar conflicting decision rules, and once a particular decision rule has been activated it tends to carry over onto other domains and situations where it is deemed relevant.[6] Our argument is in this sense very similar to Bichieri's (2002) focus theory of norms, where she argues that once a (social) norm has been activated it will show some inertia and continue to be influential across a variety of contexts. In order to test this hypothesis, participants will first perform a simple and unrelated coordination task. It will then be possible to study whether any cooperative norms generated from this will carry over to influence how they solve the dilemma problem.

To summarize, we believe that explanations of cheap-talk effects relying on minimal group effects have been ignored (see Bohnet & Frey, 1999 and Orbell et al., 1988) or rejected as a result of design errors (see Dawes et al., 1977 and Bouas & Komorita, 1996). Further, norm-based explanations which do not rely on promise-making have also been prematurely rejected (see Orbell et al., 1988). The study reported here avoids these problems and in so doing, produces evidence of non-promise making effects of cheap talk.

The Study

In this study we hope to address some of the methodological problems identified in previous experiments. We seek to test more effectively the competing explanations of group identity and cooperative norms. To examine the group identity effect, we designed a "pure solidarity" task that sought only to elicit group identity. To examine the cooperative norm effect, we designed a coordination task that had real consequences through financial incentive; this would elicit group identity through the creation of a shared fate, but would also establish norms of cooperation because individuals would need to work together to solve the coordination problem. A control condition involved an irrelevant questionnaire task.

After the pre-play conditions, individuals made single-shot, anonymous prisoner's dilemma choices with people from their own experimental condition and people from another group. Thus in-group/out-group processes could be observed. For example, we suspect that group identity affects cooperation rates at the very least at a minimal level--minimal both in terms of effect size and the sufficient manipulation to create group identity. In this study we test whether minimal group effects occur. If minimal group definitions work to elicit increased cooperation, then groups should cooperate more with members of their own group than members of out-groups.

But perhaps more importantly, the in-group/out-group dynamic also allows us to observe the boundaries of any effects of the two experimental tasks. If the cooperative norm task was successful at creating group identity strong enough to affect cooperation beyond a minimal group effect, then the participants' cooperation rates with their own group members should be higher than with others. In contrast, if the discussion primed norms of cooperation, then we would expect a carry over effect and increased

cooperation with members of their own and the other groups. Finally, by measuring the level of similarity and the success at solving coordination problems, we can also test intracondition differences. Namely, did those groups that were particularly successful at solving their coordination problems or establishing similarities cooperate more with each other than members of groups that were less similar, successful or both?

Method

Participants were recruited by advertisements in various undergraduate and graduate classes (at the London School of Economics). All participants were students. The advertisement specified that the experiment would last between 1 and 2 hours. The participants were told that the exact amount of pay would depend on "decisions you make and the simultaneous decisions of others in the experiment." There were nine replications, each having six to ten participants. A total of 80 people participated in the study. Efforts were made to prevent people who knew each other from participating in the same replication.

Participants were assigned to available time slots according to their convenience and within the time slots participants were randomly assigned to one of three conditions: control, coordination, and solidarity groups. In each replication, participants were randomly assigned to one of two conditions. One third of the replications had a control group and a solidarity group, one third had a control group and a coordination group, and one third had a solidarity group and a coordination group. For example, the first third of the replications involved the random assignment of participants into one of two groups: control; and solidarity. Groups were divided into one of two rooms, and after engaging in the task appropriate to that group, the participants would reconvene to play the prisoner's dilemma game. Critically, participants played with individuals from

both their group and the other group--in the first third of the replications, therefore, they played with individuals from the control group *and* the solidarity group.

In the control group, participants were presented with a questionnaire on public perceptions of biotechnology. Subjects sat together in a room but completed the task individually. This task took approximately 30 minutes to complete and each subject was paid UK£10 upon completion.

In the unrelated discussion groups, each subject was given a sheet containing three randomly assigned questions relating to an individual's life, including their religion, where they were born and grew up, their current course of study and why they chose it, what they hope to do after graduating, and their favourite book, film and sport. Participants then proceeded to ask each other three such questions, as well as answer each themselves. Once all participants had shared aspects of themselves with the group they completed a short questionnaire that contained two open-ended questions. The first asked them to detail the things they felt in common with the group as a whole (if anything); the second about the things they felt they did not have in common (if anything). Content analysis was used to calculate two scores--the first score was the total number of things each felt they had in common with the group, minus the features they did not have in common; the second score was the average of this score across all the participants in the group. Before the group members returned to engage in the dilemma interactions, each was paid £10 for this part of the study.

The coordination groups attempted to solve two coordination tasks. At the start of this condition the participants were given an "example problem." This consisted of a map of a countryside landscape. They were asked to imagine that they were each somewhere in the area described by the map but could not communicate with each other

in any way. They had to choose a place on the map where they would go in the hope of meeting the others in their group. They were to indicate that location by circling it on the map. This task would have been relatively straightforward if there had been only one prominent point on the map (i.e. a single crossroads). In contrast, the example task was made more difficult by having the map contain multiple prominence points (i.e. a single tree, crossroads, building, hill, . . .etc.). They were told that they would need to complete two similar tasks. It was explained that their pay for this part of the study would depend on what proportion of them decided on each of the upcoming coordination tasks. If the majority agreed on a particular answer, then each subject, whether being part of the majority or not, would receive UK£7. If a majority did not circle the same answer, all participants would receive UK£4. Participants were then given 10 minutes to discuss how they should approach this type of problem by using the countryside map task as an example of the tasks they would be asked to complete. It was made clear that they would not be allowed to discuss what to circle prior to making their choices in the two upcoming coordination problems. They were told that after each coordination task, they would be given an additional 10 minutes to discuss what had happened.

In one of the actual tasks the participants had to circle a location on a map of a museum where they would go to meet for lunch if no meeting location had been pre-determined. A number of possible meeting spots were identified, (e.g. the “main entrance,” the “information desk,” the “gift shop,” . . . etc.). North was not indicated on any of the maps and a different directional orientation was adopted on each map to make the coordination task somewhat more challenging (so strategies of, “always choose the top left option” would not be successful). The other task involved choosing

one book from a list of three. Each book was described by title, author name, and number of copies sold. This problem was made more challenging by making the row and column order of the information different on each answer sheet. The order the tasks were given to the participants was randomly determined. To measure groups' coordination success, the mean proportion of the size of the majority was recorded. That is, if four of five participants made the same choice on Task 1, and three of five participants made the same choice on Task 2, then the group's coordination score would be (.80 + .60)/2 = .70.

After all the participants had finished their pre-dilemma tasks, they went to another room where they were seated around the periphery on chairs marked with identification letters. When everyone was settled, an experimenter read standardized instructions.[7] From the outset these emphasized that there was no deception in the study and that it was very important that everybody understood what was going on. The participants were told to ask at any time about anything that was not clear to them, that their questions would be answered in full, and that the experiment would not go on until they were confident that they understood what was going on. The participants were also told that their decisions would be strictly anonymous and that no other subject would ever know what they chose.

A prisoner's dilemma matrix was explained and the dominance of defection over cooperation was stressed along with the consequences if "everyone acted on that logic."[8] The objective payoffs were unambiguously a prisoner's dilemma and were the same for each decision. Payoffs were in UK Pounds Sterling and are presented in Table 1 below. After the instructions were given, participants completed a quiz to make sure that they understood the dilemma. The answers were checked and any necessary

explanations repeated until the experimenter was satisfied that everyone understood the game.

Insert Table 1 about here

Each subject then played prisoners' dilemma games, one at a time, with each other subject in the room. All participants had to make their choices in a single game before anyone moved on to the next. Participants knew only that they would be making one choice with respect to each of the other person in the room but did not know the order of the play (their own or others') or who was making a decision with respect to whom on any given play. Notice that it would have been impossible for every pair of people to interact with each other simultaneously. By the end of the entire sequence, however, each subject had made a decision with respect to each other subject in the room.

Participants recorded their cooperate/defect decisions on forms affixed to clipboards. They were told to use the clipboards so that the other participants could not see what they were writing. Finally, they were informed that when the experiment was completed they would be excused, one at a time, to the "payoff room" where they would be given what they earned and then dismissed. Only after one subject was paid and had left the area was the next directed to the payoff room.

Results

To test the hypotheses above, a binary response logistic regression is used to estimate the likelihood of individual cooperative choices. Since we are interested in how the pre-play discussion affects individual choice, the individual decision to cooperate or defect is used as the unit of analysis. However, to use the decision (and not the subject) as the unit of analysis, the analysis must take into account that each subject

contributed five separate cooperate/defect choices. Specifically, any model must include a term that takes into account the fact that any given subject may have a greater probability of repeating the same choice (cooperation or defection) than would be normally be expected (even after conditioning on the other covariates). In other words, the inclusion of such a term allows the model to control for any amount of residual correlations between choices due to unmeasured sources of heterogeneity at the subject level. If this is not done, then in effect we are claiming to have more information about the choice process than is warranted by the data and thus we run the risk of overestimating the certainty with which parameters in the model are estimated. With the subject-specific term we *are* assuming that the choices across the matrices are independent of one another, but we also assume that they are correlated. Formally, the random-effects logistic regression takes the following form:

$$\text{logit}\,(\pi_{ij}) = \beta_0 + \beta' x_{ij} + u_i$$

where $u_i \sim n(0, \sigma_\mu^2)$ are subject-specific effects (random intercepts) and σ_μ^2 is their variance, which remains to be estimated.[9] The parameter estimates of performed regressions are presented in Table 2 and 3 below.

Insert Table 2 and 3 about here

The results indicate that those in the coordination discussion condition were more likely to cooperate than either those in the solidarity only or control conditions. After participating in the coordination discussion, the odds of cooperating on any given

play of the dilemma interactions increased by nearly a factor of 4, $e^{1.36} = 3.88$, *Wald* = 2.24, $p < .05$.[10] There was no significant difference in cooperation rates between the control and solidarity conditions, *Wald* = 0.93; $p > 0.10$.

For all groups (including the control condition), cooperation rates with members of one's own group were higher than the cooperation rates of those in other groups. The odds of cooperating with one's own group members were about 2 times greater than the odds of cooperating with other group members, $e^{0.71} = 2.04$, *Wald* = 2.32, $p < .05$. Interestingly, while in-group cooperation rates were significantly higher for all groups, there was not a significant difference in the size of this effect across the discussion condition. Neither people in the solidarity only nor coordination condition had different propensities to cooperate with their own group members than did the control group. In other words, the interactions between discussion condition and in-group/out-group cooperation were not significant, *Wald* = 0.37 and 1.37 respectively. To illustrate the pattern of results, Figure 1 shows the proportion of cooperative choices in each relevant condition. The mean cooperation rates and standard errors are presented in Table 4.

Insert Figure 1 and Table 4 about here

Finally, we explored the intra-condition variation in cooperation rates. Firstly, we found evidence of a relationship between the degree of success at the coordination problems and dilemma choices, $e^{1.03} = 2.81$, *Wald* = 2.29, $p < .05$. The more successful groups were at solving the coordination problems, the more likely they were to cooperate. Interestingly there was no interaction with in-group/out-group, $e^{-0.12} = 0.89$, *Wald* = -0.33, $p > .05$. In other words, doing well at the coordination task raised cooperation rates for individuals from one's own *and* from the other group. Assuming that performing well on this task induces cooperative norms, it follows that the effects

of the latter are not limited to behaviour toward own group members. Contrary to expectations, there did not appear to be a significant relationship between the amount of discovered group similarities (in the solidarity only condition) and cooperation rates. Whether measured at the individual level, where each individual had a unique score consisting of the sum of perceiving similarities and differences between themselves and the group as a whole, $e^{0.16} = 1.17$, $Wald = 0.74$, $p > .10$, or measured at the group level, where each individual had a score averaged across all those in the group, $e^{-0.40} = 0.67$, $Wald = -1.18$, $p > .10$, amount of solidarity did not seem to affect cooperation rates. Again, there were no significant interactions between each of these measures and the in-group/out-group variable.

Discussion

This study sought to address a number of limitations of previous studies investigating solidarity effects in social dilemma situations. Here, before prisoner dilemma interactions, participants engaged in one of three unrelated tasks. With one control and two experimental groups, we examined whether cooperation increased as a result the performance of a coordination task--designed to elicit cooperative norms and social solidarity--and a pure solidarity task. Success in both these tasks was measured. Furthermore, the conditions were crossed with an in-group/out-group factor.

The data here suggest that cooperative norms, primed in a real, shared-fate situation, affects subsequent dilemma behaviour; the greater the success at the coordination problem, the more cooperative participants became. Furthermore, this effect is not limited to behaviour within one's own group. As Orbell et al. (1988) expected, but unlike what they found, primed cooperative norms worked across group membership. Hence, participants become more cooperative toward both in-group and

out-group members. This result is important for two reasons. First it indicates that cooperative norms alter the reasoning and perspectives taken by the participants of how to solve the dilemma problem, rather than simply affecting group belongingness, implying that the increased solidarity induced by this manipulation seems fairly universal. Second, it questions the proposition that merely commitment effects are driving outcomes in similar contexts. If that were the case, we would not expect any influence to transcend to out-group members toward whom no "promise-making" has been made.[11]

However, engaging in a pure solidarity task did not influence the level of cooperation. The task's design may have simply failed to elicit group members' similarities and this is reflected in the relatively low scores of group identification we observed in this condition. On average, participants had as many things in common with the group as they did not have in common. It seems that the process of revealing and sharing social categories served to highlight as many differences as commonalities. Yet, despite this, it seems unlikely that students attending the same university at about the same time in their lives should be drastically dissimilar. And we do observe a minimal group social identity effect. People cooperated more with members of their own group, regardless of the discussion condition. This is true even if group membership simply consisted of being selected for the control group and sitting together in silence completing a questionnaire. This suggests that social identification effects in social dilemmas may be easier to elicit than some believe, but that the associated effects are relatively minor. Additionally, exercises such as the solidarity task employed here may not increase cooperation. Indeed, this instead risks highlighting differences between individuals who otherwise are rather similar to one another.

Some alternative explanations for the findings in this study exist. The coordination task could have increased levels of cooperation because it elevated the mood of the participants. Evidence has been found for an increased propensity to cooperate for those in a good mood (Hertel et al., 2000). Yet, in order for mood effects to be the primary explanation for the increase in cooperation associated with the coordination task, it would have to be the case that mood was substantially enhanced only in that condition. This seems unlikely. All participants received more or less the same amount of money from their pre-dilemma activities. Participants in the solidarity *as well as* the coordination group could have experienced mood uplifting (high similarity, coordination solutions) or dampening (low similarity, coordination failure) outcomes.

Another potential explanation for the coordination effect on dilemma behaviour may be that the coordination task engendered greater levels of solidarity than did the solidarity task. This explanation would imply that solidarity effects were key, but that only the coordination condition was successful at establishing sufficient levels of solidarity. Yet, if this were so one would expect the solidarity effect only to apply to members of their own group. This was not the case.

General Discussion

The primary purpose of the present experiments was to test competing explanations for how cheap talk works to increase cooperation in social dilemmas--the group solidarity, the cooperative norm and the promise making hypotheses. Data on the effects of related discussion--discussion on the dilemma to be faced--do not allow one to disentangle the competing explanations. To work around this problem, the effects of unrelated discussions in subsequent cooperation rates were analysed. Unrelated

discussions ought to be able to enhance (or undermine) feelings of solidarity and prime cooperative norms. And yet, given that it occurs prior to participants knowing they will face dilemma choices, unrelated discussion cannot lead to promise making about dilemma behaviour.

Furthermore, we argue that two overlapping mechanisms in group solidarity are also confounded in the literature on explanations of pre-play discussion: cooperation norms and group identity. Indeed, this is the case in work on the group identification effect in social dilemmas, although research has started to disentangle competing process explanations here (DeCremer & Van Vugt, 1999; DeCremer & Van Dijk, 2002). Our basic findings suggest that related discussion, and the promise-making that it allows, appears to have the strongest effect on cooperation rates. Nothing like the near unanimous cooperative rates observed in related discussions were observed in our unrelated discussions. This replicates the findings of many other studies. Unlike other studies however, we do find an effect for primed cooperative norms, combined with solidarity in a real shared-fate situation, and a minimal group formation solidarity effect.[12] Interestingly, the primed cooperative norms extend to non-group members. While all group members were more likely to cooperate with people in their own group, people in the coordination groups cooperated more than anyone else and this effect carried over to non-group members. Thus supporting Bichieri's (2002) argument, when a norm to cooperative has been activated, it tends to impact on decisions taken in situations involving other people. Similarly, it is sustained also for subsequent unrelated tasks where similar conflicts between cooperative and non-cooperative behavior are present.

All this suggests that social psychological processes above and beyond commitment are relevant to explaining cheap talk effects. This is consistent with a number of studies that find varying effects of solidarity and group identity on dilemma behaviour (see Brewer & Schneider, 1990; DeCremer & Van Vugt, 1999; Kramer & Goldman, 1995; Wit & Wilke, 1992). Most importantly, non-binding pre-communication has been shown to increase cooperation in public-goods settings (Ledyard, 1995; Sally, 1995). Furthermore, a common finding in the environmental economics literature is that communicative processes tend to alter the values and perspectives held by the decision-makers (Brouwer et al., 1999; Clark, Burgess & Harrison, 2000; Macmillan et al., 2002), often leading to a less consumptive oriented, and hence less individual, perspectives on the decision problem (e.g., Kaplowitz & Hoehn, 2001).

Yet this is the first time that the relative roles of solidarity and cooperative norms have been teased apart. We find that engendering both solidarity and cooperative norms has a greater effect on dilemma behaviour than solidarity on its own. And we also find that cheap talk may work for even cheaper reasons than previously thought. Discussion involves the formation of minimal group identity (see Abrams et al., 1990), even if the effect is likely to be small. The collective good may be more greatly served by exercises that combine solidarity with norms of cooperation, so long as it is established in a meaningful and consequential prior exercise. Although the present experiment failed to establish a solid solidarity effect of an unrelated discussion in the absence of the coordination task, we believe that there may be other factors at play mitigating such outcomes. Given the vast literature on the prospective influence of social context and communicative processes on human decision-making, and the bulk of

empirical support of this type of theorizing, future studies are indeed warranted that look more closely at these issues.

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Author Note

Matthew Mulford, TRIUM Global Executive MBA; Jonathan Jackson, Methodology Institute, London School of Economics; Henrik Svedsäter, Department of Psychology, University of Göteborg. We would like to thank Rick Wong for his help in data collection and in the preparation of the manuscript. We would also like to thank the anonymous reviewers for their helpful and insightful comments on earlier drafts.

Correspondence concerning this article should be addressed to: Matthew Mulford, TRIUM Global Executive MBA, London School of Economics and Political Science, Houghton Street, London WC2A 2AE, UK. E-mail: [m.mulford@lse.ac.uk].

Footnotes

[1] Classic versions of this include the threat of Hobbes' *Leviathan* (1651/1939), the strength of promises and contracts in law as seen in Hart (1963), or the bribes of side-payments in Olson (1965).

[2] For a description of another alternative--motivational convergence based on expectations of others behaviour--see Mulford, 2001.

[3] Coordination problems are a class of tasks in which an unspecified number of individuals must make independent choices about a prominent solution point.

[4] Cookson (2000), for instance, found that public goods experiments being preceded by a comprehension task placing emphasis on the group rather than the individual tend to increase cooperation.

[5] Notice that in Bouas and Komorita (1996) group identity scores were different across the two relevant groups. However, our point here is that group identity of some amount was created in both groups and thus their cooperation rates were no different.

[6] Note that we assume that the decision maker has to take an active part in solving the prior task, either by discussing it or making a decision. Simply hearing other people talking about the importance of cooperativeness or being exposed by promises from outsiders do not seem to have any effect in this respect (e.g., Brosig, Ockenfels, and Weimann, 2003; Cialdini et al., 1991).

[7] Complete copies are available upon request.

[8] During the explanations, at no time were the words “cooperation” or “defection” used. Instead, a cooperate choice was referred to as “choosing X”; a defect choice was referred to as “choosing Y”.

[9] For details of this approach, see Agresti (2002).

[10] Some may notice that the effects on the odds of cooperation seem large given the observed average differences across conditions. This is due to the fact that a random effects logistic model was used. In these models, the regression coefficients have conditional (rather than marginal) interpretations. Thus, the coefficients tell us the effect of type of pre-dilemma discussion on the odds of cooperation, while holding all other factors constant, including the subject-specific random effect--i.e. the effects of such a change *for a given* individual's odds of cooperating. See Agresti (2002, p. 498-502) for a discussion of the difference between marginal and conditional models of this type.

[11] Knez (1988) and Knez and Camerer (2000) find that cooperation in prisoner's dilemma games does increase with precedents set in weak link coordination games. However, this effect relies on similar game descriptions. This is not the case in this study.

[12] Unlike the work by DeCremer and others (e.g., DeCremer & Van Vugt, 1999; DeCremer & Van Dijk, 2002), it was only the combination of norms and solidarity that worked, not simply solidarity alone (above and beyond minimal group formation).

Table 1

Payoff Matrix in G.B. Pounds

	Cooperate	Defect
Cooperate	2,2	-6,5
Defect	5,-6	-3,-3

Table 2

Effects of Unrelated Discussion on the Probability of Cooperation. Parameter Estimates Generated by a Binary Logistic Model

	Unrelated v. no discussion	**Unrelated discussion content**
Condition		
Unrelated discussion	-0.36 (0.25)	
No discussion	0.00 (—)	
Number of critiques		0.06 (0.04)**
Ratio of defences to critiques		0.43 (0.85)
Controls for matrix		
Matrix 1	0.04 (0.20)	0.08 (.29)
Matrix 2	-0.14 (0.20)	-0.09 (.29)
Matrix 3	0.00 (—)	0.00 (—)
Matrix 4	-0.21 (0.20)	-0.36 (.29)
Matrix 5	0.00 (—)	0.00 (—)
Intercept	0.39 (0.21)*	-0.87 (.59)

	Unrelated v. No Discussion	Unrelated Discussion Content
Standard deviation of the case specific random error	1.45 (0.14)***	1.57 (0.22)***

Note. Matrices 3 and 5 have the same payoff structure and are used as the reference category in both models. Values enclosed in parentheses represent standard errors. *$p <$ .10. ** $p <$.05. *** $p <$ 0.01.

Table 3

Effects of Unrelated Discussion on the Probability of Cooperation. Parameter Estimates Generated by a Binary Logistic Model

	Inter-group measures	**Intra-group measures**	
	Overall	*Coordination*	*Solidarity*
Coordination	1.36 (0.61)**		
Solidarity only	0.56 (0.61)		
In-group	0.71 (0.31)**	1.44 (2.69)	<0.01 (0.42)
Coordination * in-group	-0.18 (0.50)		
Solidarity only * in-group	-0.67 (0.49)		
Coordination score		1.03 (0.46)**	
Coordination score * in-group		-0.12 (0.36)	
Solidarity individual score			0.16 (0.21)
Solidarity group score			-0.40 (0.34)

	Inter-group measures	**Intra-group measures**	
	Overall	*Coordination*	*Solidarity*
Solidarity individual score * in-group			-0.05 (0.21)
Solidarity group score * in-group			-0.02 (0.35)
Intercept	1.24 (0.38)***	-7.66(3.50)**	0.74 (0.41)*
Standard deviation of the case specific random error	1.78 (0.24)***	1.81 (0.48)***	1.32 (0.37)***

Note. Values enclosed in parentheses represent standard errors. *$p < .10$. ** $p < .05$. *** $p < 0.01$.

Table 4

Mean Rates of Cooperation by Condition

Condition	Mean proportion of cooperation
Control	
In-group	.42 (.50)
Out-group	.31 (.46)
Solidarity only	
In-group	.41 (.50)
Out-group	.39 (.49)
Coordination	
In-group	.59 (.50)
Out-group	.51 (.50)

Note. Values enclosed in parentheses represent standard errors.

Figure Caption

Figure 1. Mean proportion of dilemma cooperation for each condition by in-group/out-group.