

## Suckers or Saviors? Consistent Contributors in Social Dilemmas

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Groups and organizations face a fundamental problem: They need cooperation but their members have incentives to free ride. Empirical research on this problem has often been discouraging, and economic models suggest that solutions are unlikely or unstable. In contrast, the authors present a model and 4 studies that show that an unwaveringly consistent contributor can effectively catalyze cooperation in social dilemmas. The studies indicate that consistent contributors occur naturally, and their presence in a group causes others to contribute more and cooperate more often, with no apparent cost to the consistent contributor and often gain. These positive effects seem to result from a consistent contributor's impact on group members' cooperative inferences about group norms.

*Keywords:* cooperation, social dilemma, decision making, rational choice, public good, norms, consistent contributor

A hard core of unconditional cooperators is vital for the survival of societies. (Elster, 1985a)

Cooperation often entails risk. When individuals, groups, or nations are interdependent, mutual cooperation can lead to significant mutual benefits, but the larger short-term payoffs from acting selfishly are almost always tempting. Cooperation among individuals can either succeed or fail: everyone paying their share of a restaurant bill versus having to pay more than your fair share, the efficient and successful completion of a team project versus failing because some members did not pull their weight, and peace rather than war. The bottom line is that cooperation and successful collaboration in social situations are inherently difficult because self-interested action can undermine mutual cooperation. We refer to this pervasive social challenge as “the cooperation problem.”

The cooperation problem is important for many fields, including economics (Milgrom & Roberts, 1992; Williamson, 1981), evolutionary biology (Trivers, 1971), the multidisciplinary literature on social dilemmas and public goods (Dawes, 1980; Ledyard, 1995; Ostrom et al., 2002; Weber, Kopelman, & Messick, 2004), and

organizational behavior (Argyris, 1964; Barnard, 1938; Follett, 1941; McGregor, 1960). All of this attention confirms Elster's (1985a) assessment that “there is no more important problem in the social sciences [than understanding collective action], and none that is more difficult.” Current research in social psychology continues to emphasize the cooperation problem's importance and tenacity (e.g., Insko, Kirchner, Pinter, Efav, & Wildschut, 2005; Parks, Sanna, & Posey, 2003; van Dijk & Wilke, 2000; Weber et al., 2004; Wit & Kerr, 2002).

Rational choice theory, expected utility models, and game theory provide formal models of the cooperation problem. Because these models assume that individuals attempt to maximize narrowly defined personal utility, they predict that rational individuals should rarely if ever cooperate in finite mixed-motive interactions (Andreoni, 1995; Ledyard, 1995; Luce & Raiffa, 1957). Fortunately for society and human institutions, these bleak predictions have rarely been empirically supported: A huge volume of data has consistently shown that many people cooperate when these theories say that they should not (Ledyard, 1995; Weber et al., 2004). At the same time, individuals, groups, organizations, and nations only rarely achieve completely efficient outcomes in which they take maximum advantage of their potential mutual benefits. In addition, experimental data on social dilemmas have consistently shown that as the end of an interaction approaches, cooperation declines, often precipitously (Camerer & Fehr, 2006; Kahn & Murnighan, 1993; Ledyard, 1995). Thus, on one hand, the cooperation problem retains its force, as groups continue to be less than completely efficient; on the other hand, strict economic models cannot accommodate the mutual cooperation that people do achieve.

Recently, experimental economists have proposed that the presence of “strong reciprocators,” who willingly spend personal resources to punish free riders, accounts for some of rational choice's prediction failures (Camerer & Fehr, 2006). In the current research, we present an alternative, complementary solution. Specifically, we suggest that consistent contributors (CCs) can act as catalysts for cooperation by altering the perceptions and actions of their fellow group members. We define CCs as individuals who always contribute, regardless of

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others' choices. They are not reciprocators; instead, they initiate cooperation and always contribute. We suggest that one individual's consistently cooperative actions send a clear, unambiguous signal that cooperation is appropriate, which allows other group members to view their group's norms as cooperative. In other words, CCs can change other group members' perceptions of their social context, increasing the chances of additional cooperation.

This approach stands in stark contrast to previous theoretical frameworks that predict that CCs should not exist in temporally finite groups, that if they do exist they are suckers and lack intelligence, that they should have little or no impact on others' choices, and that they will be gleefully exploited by their fellow (more rational) group members. In particular, long-standing research on two-person prisoners' dilemmas has characterized CCs as suckers (Rapoport & Chammah, 1970). When groups include three or more members, however, opportunities to influence cooperation increase (cf. Dawes, 1980; Weber et al., 2004) and provide substantial opportunity for CCs to have a more positive impact.

### Social Dilemmas

The social dilemma literature has investigated the cooperation problem directly. Social dilemmas are interactions in which mutual cooperation gives all of a group's members higher payoffs than does mutual noncooperation, but each group member can obtain higher short-run payoffs by not cooperating (Dawes, 1980; Messick & Brewer, 1983; Weber et al., 2004). Prisoners' dilemmas, the prototypical two-person social dilemma, are analogs for many important social interactions (e.g., Trivers, 1971); larger,  $n$ -person social dilemmas (i.e.,  $n > 2$ ) are more representative of broader social interactions (Dawes, 1980; Messick & Brewer, 1983).

Another archetypal form of social dilemma is the public goods dilemma, in which individuals choose whether to contribute to establish or support a public good. One of many examples in the United States is National Public Radio (NPR). A large portion of NPR's budget comes from listeners' contributions. With sufficient contributions, listeners benefit because NPR can thrive and offer better programming; without sufficient contributions, NPR might not be able to offer quality programming, much less survive. Like most public goods, noncontributors can benefit as much as contributors but pay none of the costs.

Many group tasks also qualify as public goods dilemmas: Success requires multiple contributions, but undercontributors, and free riders may benefit more in the short run than do contributors. If everyone in a student project group pulls their weight, for example, the group will undoubtedly do better than if everyone sits back and waits for others to get things done. At the same time, however, everyone has an incentive to let others do more.

The game theoretic prescription to not cooperate in a dilemma with a fixed and known endpoint is particularly strong as the endgame nears, when individuals' choices cannot influence others' future choices or their own future outcomes. But if everyone expects that no one will contribute at the very end of a social dilemma and everyone knows that their interaction is finite, then early cooperation becomes increasingly risky, to the point of being irrational (Ledyard, 1995). Uncertainty about the endpoint opens the door for rational cooperation (Kreps, Milgrom, Roberts, & Wilson, 1982; Roth & Murnighan, 1978), but noncooperative

action is always tempting and, once taken, can quickly lead to universal noncooperation.

### Consistent Contributors

Despite bleak predictions by some theorists, cooperation does occur in finite social dilemmas (cf. Ledyard, 1995). We argue that the presence of even a single CC can lead to more cooperation in groups than rational choice theories predict. In short, we suggest that CCs influence how other group members interpret and understand social dilemmas, prompting them to cooperate more than they might otherwise.

The notion that a small number of people can have pervasive effects on many others appears repeatedly in the history of the social sciences. Margaret Mead's quotation is a justly famous example: "Never doubt that a small group of thoughtful, committed citizens can change the world. Indeed, it is the only thing that ever has." Elster (1985a) has argued that Kantian actors who apply a moral maxim of unconditional cooperation "may be a necessary condition for the emergence of conditional cooperators. These, in turn, may bring the level of participation up to the point at which new people join because they would be ashamed of being free riders." In organizations, Brief and Motowidlo (1986) suggested that "supervisors and co-workers who behave prosocially . . . have the effect of stimulating others around them to behave prosocially, too." Weick's (1969, 1993) theory of organizational action concurs, suggesting that one person's actions can initiate a continuing chain of "double interacts" that ultimately has large consequences.

The enormous literature on the prisoners' dilemma, however, suggests the exact opposite, that consistent cooperation is ineffective if not suicidal. Solomon's (1960) participants, for instance, were puzzled by the behavior of unconditional cooperators and exploited them. Lave (1965) found that although unconditional cooperation elicited early cooperation, participants who realized that this "Gandhi strategy" was unprovocable tended to shift to consistent noncooperation. Similarly, using a variation of the prisoners' dilemma, Deutsch, Epstein, Canavan, and Gumpert (1967) found that a turn-the-other-cheek strategy ultimately elicited more noncooperation than a contingently cooperative strategy. These findings suggest that consistent cooperation is foolish, a conclusion that is consistent with game theoretic prescriptions and social dilemma research in experimental economics and social psychology.

Groups of three or more people, however, generate different dynamics than does a dyad (e.g., Caplow, 1968). Social contagion effects (e.g., Burt, 1987; Coleman, Katz, & Menzel, 1966; Meindl, 1993), for instance, are irrelevant in two-party interactions. In addition, successful two-party strategies in the prisoners' dilemma do not easily apply to larger groups. The tit-for-tat strategy, for example, in which one party begins by choosing cooperatively and then chooses whatever their counterpart has chosen on the previous interaction, has been hailed as a simple and robust strategy for encouraging and sustaining cooperation in repeated prisoners' dilemmas without communication (Axelrod, 1984). Determining the appropriate criterion for imitation and reciprocity is not clear, however, for larger groups. Retaliating after a few other group members have not cooperated may signal that other cooperative group members should also retaliate, increasing noncooperation and collective inefficiency; not retaliating may implicitly condone

noncooperation and encourage free riding. In these kinds of ambiguous contexts, CCs may be influential because their actions are clear, unequivocal, and easily interpreted. Indeed, simulation studies have suggested that cooperative strategies are more effective in “noisy” social dilemmas, when others’ intentions are difficult to discern (e.g., Bendor, Kramer & Stout, 1991; Kollock, 1993). Recent empirical work has lent support to the simulations’ conclusions (Van Lange, Ouwerkerk, & Tazelaar, 2002).

To summarize, (a) rational choice models of decision making in social dilemmas warn against consistent cooperation as a strategy and predict that it should not positively influence others’ choices—especially in finite interactions; (b) these conclusions are based primarily on prisoners’ dilemma theory and research; (c) *n*-person interactions are qualitatively different from two-party interactions; and (d) consistent contributions in larger groups may stimulate additional cooperation by altering how other group members perceive their social norms. This article presents the first research of which we are aware to experimentally investigate the effects of CCs in *n*-person social dilemmas.

### A Theory of CC Effectiveness

In a social dilemma, one person’s consistent contributions can signal that cooperation is appropriate for individual action and as a group norm. This mechanism, we predict, can increase the likelihood of cooperation by other group members.

Norms can be descriptive or prescriptive, that is, what most people do (descriptive) or should do (prescriptive) in a given situation. Norms form early in the life of a group (Bettenhausen & Murnighan, 1985), with initial interactions confirming or challenging group members’ individual scripts, and repeated interactions quickly evolve to become expectations about how everyone will behave (Bettenhausen & Murnighan, 1991).

In the ambiguous context of an *n*-person social dilemma, individuals who are steadfastly and repeatedly cooperative, without fail, send a signal that cooperation is appropriate. Consistency helps to make their signal clear and unambiguous and therefore more influential (Moscovici & Lage, 1976; Moscovici, Lage, & Naffrechoux, 1969). Group members who are uncertain about what is appropriate but who are initially inclined to cooperate—conditional cooperators—should be most affected. Ward (1989) described conditional cooperators as tentative and in need of reassurance that they will not be exploited. Kondo (1990) noted that cooperation that requires far-sighted analysis about future, reciprocal benefits is unstable and unlikely. Thus, early, consistently cooperative signals can reassure conditional cooperators and contribute to establishing a cooperative group norm.

The idea that norms might facilitate cooperation is not new; Simon (1990, p. 1665) argued that “receptivity to such social influence” (which he attributed to a combination of social rewards and bounded rationality) can facilitate cooperative behavior. Whereas Simon primarily addressed normative influence on individuals, we focus on how a cooperative actor can catalyze cooperative norms.

### The Current Research

Our first empirical question was whether CCs occur naturally in social dilemmas. Given the strong stands of economic models and

previous prisoners’ dilemma research, merely discovering this effect would be significant. We then investigate (a) whether the presence of a CC increases other group members’ contributions; (b) the reliability and boundary conditions of CCs’ effects; (c) whether CCs’ choices are self-beneficial or, as previous work has suggested, self-destructive; (d) whether CCs promote cooperative norms, (e) whether individuals’ social motives moderate a CC’s effects, (f) whether fellow group members recognize the influence of successful CCs, and (g) whether social status augments a CC’s effects.

Studies 1 and 2 use existing data sets to determine the existence and basic effects of CCs. Existing data provide a unique inferential advantage: Finding CC effects in others’ data means that our own research methods did not produce them. After addressing the basic issue of existence, Studies 1 and 2 assess CCs’ effects on their fellow group members’ cooperative choices and how their own outcomes compared with those of individuals in groups that did not include CCs.<sup>1</sup> Study 3 assesses the causal impact of CCs by manipulating their presence in a group. It also investigates the potential influence of encouraging cooperative norms and group members’ social motives. Study 4 provides an experimental replication of a CC’s effects, assesses fellow group members’ perceptions of CCs, and assesses the impact of social status on a CC’s influence.

The experimental procedures in all four studies gave participants an endowment before every round of a repeated social dilemma. In Studies 1, 3, and 4, they could contribute all or none of their endowment in each round. In Study 2, they could contribute any part of their endowment, that is, a continuous contribution procedure; Study 2 also addresses a limitation in Study 1’s design. Finally, analysis of pooled data from Studies 3 and 4 assesses whether perceived cooperative norms mediate the relationship between the presence of a CC and other group members’ contributions.

## Study 1

### Method

Sonnemans, Schram, and Offerman (1999) conducted a large public goods experiment that included four-person groups interacting repeatedly in a series of dilemmas for 3, 6, 9, or 12 rounds. The last round of each of their interactions was known in advance. From their larger data set, we identified 35 6-round interactions, the largest subset of the data that included enough rounds to allow other group members to notice the consistency of an actor’s contributions. (Many of their interactions lasted only 3 rounds, insufficient for group members to observe the presence of a CC, and there were only a few longer interactions—not enough for powerful statistical analyses.) We did not obtain or analyze any other data sets to test our initial hypotheses. We requested these particular data because they met the simple criterion of being a

<sup>1</sup> CCs suffer losses if others do not contribute. This means that they take more risks than other group members. As a result, like Axelrod’s (1984) tit-for-tat players, they cannot get better outcomes than their fellow group members. Thus, intragroup outcome comparisons never favor CCs. If CCs influence others to contribute, however, they can avoid losses and may actually do better than the members of groups that do not include CCs.

study of a repeated public goods dilemma, and the authors kindly and willingly shared their data.

*Participants.* Eighty participants (57 men, 22 women, and 1 unidentified) interacted in one of five 16-person sessions. Each participant was randomly assigned to an initial 4-person group and interacted in a social dilemma repeatedly via computer. After the first interactions, at a predetermined and known time, 1 participant rotated to another group. Participants knew about the rotation scheme and the ID numbers of their fellow group members; personal identities were not revealed. Most of the participants were economics students ( $n = 46$ ); they averaged 23 years of age. Their pay, which depended on their outcomes, was very good, averaging \$37.50 for the 2-hr experiment.

*Procedure.* Each participant started each round with the equivalent of \$0.60. They chose whether to keep the \$0.60 or use it to buy a marker, which yielded a \$0.40 benefit for each group member, including themselves. Everyone’s choices were simultaneous and anonymous, without communication. The results were displayed to all group members after each round. Participants’ possible outcomes (see Table 1) fit the definition of a social dilemma: Everyone did better if everyone contributed, but anyone did better individually in any given round by not contributing (regardless of others’ choices).

*Analytic Strategy*

We used the same analytic strategy in all four studies. First, we operationalized CCs stringently; they contributed in every round. Second, because we were interested in their effects on others’ choices, CCs were excluded from all between-condition analyses of a CC’s influence. Third, groups were categorized as including a CC or not. Fourth, because participants needed to notice the presence of CCs on their own, our analyses excluded the first two rounds of choices in Study 1 and the first four rounds in the longer interactions of Studies 2, 3, and 4.

*Multilevel modeling.* All participants were nested within groups; thus, their choices were not independent. One analytic strategy would be to aggregate individual data within groups and compare different groups (e.g., Kenny, Mannetti, Pierro, Livi, & Kashy, 2002), but this would reduce power and make it difficult to assess the impact of individual-level covariates. Thus, all analyses of between-condition differences used multilevel modeling techniques (Raudenbusch & Bryk, 2002; Singer, 1998) that allow for an assessment of the variation of individual-level choices across conditions, controlling for the variance attributable to each individual’s group (treated as a random factor).

Table 1  
*Individual Payoffs per Round in Studies 1, 3, and 4 as a Function of Participants’ Own Choice and the Choices of Their Fellow Group Members*

Choice	No others contribute	One other contributes	Two others contribute	Three others contribute
Participant contributes	\$0.40	\$0.80	\$1.20	\$1.60
Participant does not contribute	\$0.60	\$1.00	\$1.40	\$1.80

*Results*

In the 35 groups in this dataset, 9 included at least one CC. Thus, even in an anonymous interaction without communication, populated largely by economics majors, CCs emerged. Twelve CCs emerged, representing 8.6% of the total sample; one group included 3, another included 2. Thus, the data confirm that CCs exist, even in experimental games.

The data also suggest that CCs had a positive impact on their fellow group members’ choices. In the last four of their six rounds, the members of groups with CCs, excluding the CCs themselves, made significantly more contributions ( $M = 1.96, SD = 1.22$ ) than did the members of groups without CCs ( $M = 1.34, SD = 1.21$ ),  $t(39.40) = 1.93, p = .03$ , one-tailed. Put another way, the non-CC members of CC groups contributed 60% more in their last four rounds than did the members of non-CC groups. Thus, the answer to our second research question is also clear: CCs in these groups had a salutary effect on their fellow group members’ choices.

CCs also did well: They obtained significantly better outcomes ( $M = \$7.45, SD = 1.96$ ) than the members of groups that did not include CCs ( $M = \$5.93, SD = 1.34$ ),  $t(39.39) = 1.95, p < .03$ , one-tailed. Their fellow group members also did significantly better than the members of groups without CCs ( $M = \$8.08, SD = 1.22$ , and  $M = \$5.93, SD = 1.34$ , respectively),  $t(36.78) = 5.30, p < .001$ . Thus, all of the members of groups that included at least one CC obtained significantly better outcomes than the members of groups that did not. In other words, CC groups were significantly more efficient, economically, than non-CC groups. Thus, our final research question in Study 1 was also answered positively: Not only do CCs exist and positively influence their fellow group members, but they do not suffer in the process.

*Discussion*

First, these data document the existence of CCs. Although anecdotes shared by social dilemma researchers at conferences have long suggested that such individuals exist, the current findings indicate not only that they exist but also that they may not be unusual, even in an unsympathetic context like an experimental laboratory with an anonymous sample of mostly economics students (Frank, Gilovich, & Regan, 1993).

The data also suggest that CCs are effective influence agents: Their presence is associated with an increase in their fellow group members’ cooperative contributions. In the process, everyone benefits, even the CCs themselves.

In the broad context of rational choice models, these are provocative results. As is always the case, however, the results of a single study are far from conclusive. In particular, these results do not provide a basis for causal inferences. In addition, because of the original experiment’s rotation scheme, some of these groups were not independent of one another. Although multilevel modeling analyses may have limited the impact of this issue, the experimental conditions require particularly cautious conclusions. Thus, Study 1 might be best characterized as an encouraging pilot study. Study 2, a conceptual replication using a different archival data set, provides a second investigation of CC effects, in a different interactional context.

## Study 2

Rather than all-or-none contributions, Isaac, Walker, and Williams's (1994) experiment allowed participants to contribute any portion of their endowments to the public good. Their participants experienced only one multiround social dilemma, with no changes in group memberships, that is, no rotation scheme. In addition, participants experienced either a low (.30) or a high (.75) marginal per capita return (MPCR). In public goods dilemmas, MPCR identifies the portion of an individual's contribution that returns to the contributor and everyone else in the group. For example, an MPCR of .60 means that each contribution of  $x$  leads to each group member receiving  $.6x$ , including the contributors themselves. If only one group member contributes, she or he will lose  $.4x$ .

An MPCR of .30 created a particularly risky context, as all four group members needed to contribute almost their entire endowment for any contributor to benefit, and contributing alone would result in a 70% loss of the contribution. The higher MPCR created a much less risky context, as only two group members needed to contribute their endowments for the contributors to benefit. We expected that the less risky, higher MPCR context would facilitate contributions and the emergence of CCs. Thus, inclusion of the two MPCR conditions allows us to assess a potential boundary condition for CC effects. With MPCRs less than 1, however, the cooperation problem still exists.

Study 2 also provided an independent replication and test of Study 1's hypotheses, that is, whether CCs occur naturally, whether they influence other group members to contribute more frequently, and whether they do better than the members of other groups that do not include a CC. Because this experiment allowed participants to choose the size of their contributions, we also investigated whether CCs influenced the size of their fellow group members' contributions.

### Method

One hundred eight students in a micro-economic theory course participated in four-person groups,<sup>2</sup> 40 in the high-MPCR and 68 in the low-MPCR conditions. Each participant was randomly and anonymously assigned to a four-person group for a month-long, 10-round, computer-mediated public goods dilemma with a known endpoint. They made their contribution decisions once every 3 days or so. They were told their own and the group's outcome after each round. They could not identify how much anyone else had contributed (unless theirs was the only contribution).

We operationally defined CCs as group members who made contributions of any size in all 10 rounds. Participants received 50 tokens, each worth a penny, before each round and chose how many tokens they would contribute to the public good. Although the outcomes were described monetarily, participants knew that their earnings would not be paid in money but would be converted to extra course credit. (Those who did well could improve their grade from a B to a B+, a B+ to an A-, etc.)

### Results

Of the 108 participants, 19 were CCs. As expected, significantly more CCs emerged in the high-MPCR conditions (13 of 40 participants; 32.5%) compared with the low-MPCR conditions (6 of

68; 8.8%),  $\chi^2(1, N = 108) = 9.74, p = .002$ . Thus, the existence question is again answered positively, and as predicted, more CCs emerged in cooperation-friendly conditions.

The presence of a CC again influenced others' contribution frequencies, in both MPCR conditions. Multilevel analyses led to two significant main effects; the interaction between MPCR and the presence or absence of a CC was not significant. Specifically, individuals in groups with a CC contributed more than twice as often in the last six rounds ( $M = 3.34, SD = 2.16$ ) than did individuals in groups without a CC ( $M = 1.62, SD = 1.70$ ),  $t(24) = 2.60, p = .016$ , and individuals in high-MPCR groups ( $M = 3.04, SD = 2.12$ ) contributed significantly more often than individuals in low-MPCR groups ( $M = 1.81, SD = 1.88$ ),  $t(30) = 1.81, p = .04$ , one-tailed.

Analysis of the size of individuals' contributions (the average proportion of potential contributions in the last six rounds) also yielded two significant main effects. The members of groups with a CC made contributions that were more than twice as large ( $M = 30.07\%, SD = 31.36$ ) as did the members of groups without CCs ( $M = 13.13\%, SD = 22.42, p = .011$ ), and individuals in high-MPCR groups made contributions that were more than twice as large ( $M = 29.53\%, SD = 32.04$ ) as did individuals in low-MPCR groups ( $M = 13.91\%, SD = 22.73$ ),  $t(26.59) = 2.78, p = .01$ .

Analysis of the size of individuals' contributions also yielded a significant interaction,  $t(22.78) = 2.21, p = .038$ . As Figure 1 illustrates, the presence of a CC was most effective in the high-MPCR groups; they had no impact on the size of contributions in the low-MPCR groups. Thus, groups that included a CC contributed more frequently in both low- and high-MPCR groups, and their contributions in the high-MPCR groups were particularly large.

Once again, CCs did well: They obtained significantly better outcomes ( $M = 1,683, SD = 594$ ) than did individuals in groups without CCs ( $M = 1,085.4, SD = 168.31$ ),  $t(24.11) = 3.10, p = .005$ . A significant interaction between CC versus non-CC groups and MPCR,  $t(24.40) = 5.61, p < .001$ , indicates that CCs did particularly well when MPCR was high ( $M = 2,031.04, SD = 326.25$ ) relative to the members of groups without CCs ( $M = 1,292, SD = 217.70$ ),  $t(7.7) = 3.80, p = .006$ . They also fared significantly better than the members of groups without CCs when MPCR was low ( $M = 1,033.75, SD = 104.14$ , vs.  $M = 930.33, SD = 147.97$ ),  $t(52) = 2.19, p = .033$ , but to a lesser degree (see Figure 2).

### Discussion

These data replicate and extend Study 1's key effects in a different context and in social dilemmas that allowed continuous contributions. As before, CCs emerged, even among students enrolled in an economics class. In addition, the members of CC groups contributed more and cooperated more often than did the members of non-CC groups, and CC members received higher payoffs than non-CC group members, especially in a cooperation-friendly context (the high-MPCR condition).

Study 2 also identified a potential moderator of one aspect of the CC effect. Although individuals cooperated more when their group

<sup>2</sup> This research also included groups of other sizes. We only analyzed the four-person groups to retain comparability with Study 1.

included a CC, fewer CCs surfaced when the return on their contributions was low. Thus, CCs have an impact, even in difficult situations, but they do not appear to be oblivious altruists. Instead, they emerged more often in a friendlier context.

Another element of note in this study is that group members could not identify the CCs. Thus, CCs could not influence their fellow group members' perceptions of them; instead, it seems that they influenced the groups' ideas of their collective cooperative behavior.<sup>3</sup> Research on the strong effects of the knowledge of the identity of an actor (e.g., Jenni & Loewenstein, 1997) has suggested that the influence of a CC might increase in a more public, identity-revealing context. Study 3 addresses this by ensuring that as in Study 1, group members knew who was contributing consistently (even though they could not identify them personally). However, the fact that CCs in Study 2 were not specifically identified implies that their effects resulted from their ability to suggest the presence of a cooperative social norm. Studies 3 and 4 test this idea directly.

Finally, because most of the participants in Studies 1 and 2 were economics majors, these first two data sets provide a particularly conservative test of the CC effect (cf. Frank et al., 1993). A more diverse, less economically oriented sample like that of Study 3 might augment the effects of a CC even further.

### Study 3

In Studies 1 and 2, we observed the natural emergence of CCs and then assessed their impact. As a result, neither study could conclude that CCs caused increased contributions. To facilitate causal conclusions, Study 3 manipulated the presence or absence of a CC.

Study 3 also expanded the scope of our research by directly measuring social motives and group members' perceptions of their groups' norms. As noted, past research has suggested that people with prosocial motives tend to be more sensitive to the cooperative-competitive nature of their environment than people with proself motives (Kelley & Stahelski, 1970). Thus, we expected that prosocials would respond more positively to CCs than proselfs. We also expected that CCs would positively influence

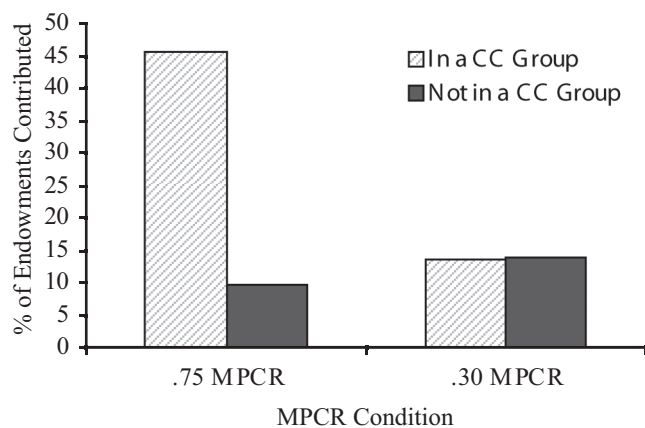


Figure 1. Size of contributions as a function of consistent contributor (CC) condition and marginal per capita return (MPCR) in last six rounds of Study 2.

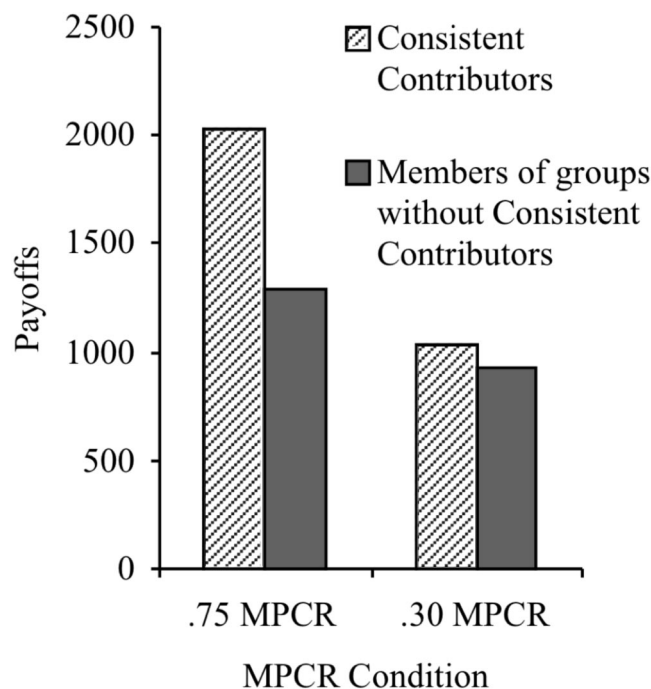


Figure 2. Consistent contributors' payoffs versus those in groups without consistent contributors in Study 2. MPCR = marginal per capita return.

other group members' perceptions of a cooperative group norm, which would, in turn, increase their contributions.

### Method

**Participants and design.** Forty-eight students from a major midwestern university participated in randomly constructed four-person groups. One member of a group in the control condition contributed for the first 18 of the 20 rounds; this group was excluded from the analyses.<sup>4</sup> Participants ranged in

<sup>3</sup> This suggests that the presence of CCs was more important than the size of their contributions: Participants could not see the size of the contributions of someone who was acting as a CC. The presence of a CC, however, ensured that there were never any zero contribution rounds in a CC group. In the non-CC groups, 79% had zero contribution rounds, and more than 85% had zero or virtually zero contribution rounds (i.e., contributions less than 5% of the possible amount).

<sup>4</sup> This participant contributed 39% more in the first 18 rounds than any participant in any other control group. As the theory and results in this article would anticipate, this participant's contributions had the same kinds of effects as other CCs: The average number of contributions made in the last 16 rounds by this participant's group members was 15.67 ( $SD = 0.58$ ), that is, considerably more than the contributions of most other groups. When asked about his choices after the fact, he indicated that he was majoring in "mathematical methods in the social sciences." The experimenter suggested that if the student followed his field's recommendations, he should have repeatedly defected. The participant smiled and said, "I know, but it's always bothered me that my professors always talk about the payoff contingencies but never talk about the impact of others' choices on your own choices. So I was curious . . . It worked out pretty well though, didn't it?"

age from 17 to 34 ( $M = 20.87$ ,  $SD = 3.06$ ). They responded to flyers, posters, face-to-face recruitment, and e-mail invitations that advertised a group decision-making study. They had taken an average of two university economics courses ( $M = 1.88$ ,  $SD = 3.13$ ); 75% indicated at least some familiarity with game theory ( $M = 3.44$ ,  $SD = 2.1$ , on a 7-point Likert scale ranging from 7 [*very familiar*] to 1 [*not at all familiar*]). They received \$10 for participating plus a chance to win up to \$100 on the basis of their outcomes. Participants were randomly assigned to an experimental condition, in which a confederate consistently contributed, or a control condition that did not include a confederate. Care was taken to ensure that group members did not know one another.

**Procedure.** The experimenter presented a verbal overview of the procedures before escorting each participant to a separate room that included a computer display of the instructions. Participants' choice options and payoffs were identical to those in Study 1 (see Table 1). They each received an endowment of 60 units before each round of a 20-round public goods dilemma. They could put their endowment in either a group or a personal account. The MPCR was .67: Each 60-unit contribution resulted in 40-unit payoffs to each group member. Each person's total for any given round was their share of the group account plus 60 if they had chosen their personal account. The instructions included Table 1; they were encouraged to study it carefully to make sure that they understood all of the outcome possibilities.

The participants knew that they would receive a lottery ticket for each unit that they accumulated and that the 20th round would be their last. After Round 20, participants completed a postexperiment questionnaire that included a manipulation check, a measure of social motives (Van Lange, De Bruin, Otten, & Joireman, 1997), items measuring norm perceptions, their general reactions, questions about their exposure to economics and game theory, and demographic items (e.g., age and gender). After the study was completed, a lottery was conducted and six prizes of \$25, three of \$50, and a grand prize of \$100 were awarded. The instructions emphasized that participants' chances in the lottery would increase as they accumulated more units ("virtual lottery tickets").

In the experimental condition, a confederate chose the group account in every round. To make this choice noticeable, everyone received complete feedback about each of the other group members' choices in the current round and all previous rounds, with the players identified as Number 1, Number 2, and so forth. Thus, each group member could see everyone's choices but not their identities.

**Materials.** Four items measured participants' cooperative norm perceptions: "When I saw group members put their units in the group account, I felt I should put my units in the group account too," "I felt good when I put my units in the group account," "I felt smart when I put my units in the group account," and "I thought putting my units in the group account was the morally right thing to do." We summed their responses to these items to form an index that was internally reliable (Cronbach's  $\alpha = .76$ ).

Social motives were assessed using the triple dominance measure of social value orientation (Van Lange et al., 1997).

## Results

As before, individuals in groups that included a CC contributed significantly more in the last 16 rounds ( $M = 9.50$ ,  $SD = 3.95$ ) than did individuals in non-CC groups ( $M = 6.95$ ,  $SD = 2.48$ ),  $t(9.64) = -1.98$ ,  $p = .037$ , one-tailed. Also, 29.2% of the CC group members made more than 14 contributions; no one in the control groups made this many contributions. The effect of CCs was particularly evident in the last 5 rounds: Members of CC groups contributed 65% of the time during these final rounds ( $M = 3.25$ ,  $SD = 1.45$ ); members of the control groups contributed only 36% of the time ( $M = 1.80$ ,  $SD = 1.44$ ,  $p = .009$ ). Thus, CC groups created significantly more joint value than non-CC groups ( $M = 10,587$ ,  $SD = 967$ , vs.  $M = 8,780$ ,  $SD = 572$ ),  $t(11) = -3.75$ ,  $p < .005$ .<sup>5</sup> Finally, members of CC groups obtained higher, but not significantly higher, payoffs than did the members of non-CC groups ( $M = 2,315$ ,  $SD = 387$ , vs.  $M = 2,195$ ,  $SD = 188$ ),  $t(26) = 1.12$ ,  $p = .28$ . Thus, the effect of CCs on their group members' contributions surfaced again, without cost to the CC (although this time without statistically significant gain) and with significant gains and greater economic efficiency for their groups.

## Perceptual Measures

None of the members of the non-CC groups responded positively to this postexperiment item: "Was there anyone in your group who *always* put their units in the group account?" All of the members of the CC groups did. Thus, the manipulation of the presence of a CC was effective, and everyone was accurate in their perceptions. Although participants in groups with CCs seemed to perceive stronger cooperative norms, this difference was not significant ( $M = 19.38$ ,  $SD = 4.26$ , vs.  $M = 17.80$ ,  $SD = 4.53$ ),  $t(10.78) = 0.98$ ,  $p = .35$ .

Thirty-five participants could be classified as prosocial (i.e., cooperators) or proself (i.e., individualists or competitors); the others either had incomplete data ( $n = 1$ ) or could not be clearly categorized ( $n = 8$ ). For those who could be categorized, a 2 (CC or control)  $\times$  2 (prosocial or proself) multilevel analysis indicated that as expected, prosocials contributed more ( $M = 9.88$ ,  $SD = 3.65$ ) than proselfs ( $M = 6.74$ ,  $SD = 2.8$ ),  $t(31) = 2.71$ ,  $p = .01$ . The significant main effect for CC conditions reiterated the main effect noted above, with CC group members contributing more often than control group members ( $M = 9.14$ ,  $SD = 3.85$ , vs.  $M = 6.54$ ,  $SD = 2.22$ ,  $p = .05$ ). The interaction was not significant,  $t(31) = .834$ ,  $p = .41$ .

## Discussion

These results replicate the CC effect experimentally, making causal inferences possible. CCs did not do significantly better than the members of groups without CCs in this experiment, but their cooperative actions did not hurt them. This continues to contradict the predictions of game theory and rational choice and the empirical expectations derived from prisoners' dilemma research. These findings, then, are optimistic about the impact of consistent coop-

<sup>5</sup> This is the only analysis that includes CCs in their own groups.

eration in larger group interactions; they suggest that consistently cooperative action can be a “rational” individual choice with positive collective consequences.

Although we did not find the predicted effect on norm perceptions, the means were in the predicted direction and the sample size was relatively small. Thus, Study 4 also investigated the effect of CCs on their group members’ norm perceptions. In addition, it was designed to replicate the core findings of Studies 1, 2, and 3 and to determine (a) whether CCs’ fellow group members recognized their influence and (b) whether CCs’ social status affected their impact.

### Study 4

In our first three studies, participants knew virtually nothing about one another. These studies consisted of contexts in which game theory could be expected to predict well, that is, anonymous interactions with clear, fixed endpoints and considerable social distance. In real-world dilemmas, however, people often know something about each other (especially in smaller groups), and a natural question about CCs is whether their personal characteristics or identities matter. Thus, Study 4 focused not only on replication, but also on whether CCs’ status affects their impact.

People tend to cope with new, ambiguous situations by attending to the actions of others who act as models (Bettenhausen & Murnighan, 1985) and whose characteristics influence their impact (Bandura, 1977, 1986). Because norms form quickly, people must make quick decisions; in particular, they must make quick decisions about whether they should emulate models’ actions. On one hand, high-status individuals might be more influential than low-status individuals. On the other hand, however, even low-status group members may be influential if their message is credible (Hovland, 1959; Hovland, Janis, & Kelley, 1953). Study 4 investigates whether the characteristics of CCs affect their influence or whether their actions are influential independent of their personal characteristics. If CCs’ effects result from their ability to promote cooperative social norms, as we have suggested, then their personal characteristics should have little impact. Alternatively, if both factors influence people’s decisions, high status might augment the CC effect.

### Method

*Participants.* Seventy-six undergraduates were recruited via e-mail, flyers, and posters. Two groups were dropped from the analyses because group members knew each other well. This left a total of 69 participants (47 women and 22 men).

*Design.* The study included three conditions. Participants were randomly assigned to (a) a control group with no confederates; (b) a low-status CC condition, which included a consistently cooperating confederate who was described as a part-time secretary; or (c) a high-status CC condition, which included a consistently cooperating confederate who was described as a PhD student. As before, the analyses, unless otherwise noted, used multilevel modeling with two levels, individuals nested within groups.

*Materials and procedure.* Study 4 used the same materials and procedures as Study 3. The only changes were the elicitation and

administration of information about other group members before their interaction and the addition of a “group member questionnaire,” which asked participants to evaluate their group members, including themselves, on a number of dimensions after their interaction. They also indicated how much each of the other group members influenced their decisions. The reliability of the cooperative social norms index (Cronbach’s  $\alpha = .72$ ) was comparable to its reliability in Study 3.

When participants were escorted to their individual rooms, they filled out a short form indicating their ages, their year in school, or if they were not students, their job description. The experimenter ostensibly compiled the data on summary sheets and returned them to participants so they “had some idea who they were interacting with.” Every summary sheet indicated that the group included a 20-year-old 3rd-year history major, a 19-year-old part-time university secretary, a 22-year-old PhD student, and the actual participant. In the low-status CC condition, the part-time secretary chose the group account in every round; in the high-status CC condition, the PhD student chose the group account in every round.

### Results

*CC effects.* Seven CCs in five groups emerged in the control condition;<sup>6</sup> one group included three CCs. We treated these groups as a separate, “naturally occurring CC” condition. Thus, we compared the choices of participants from four kinds of groups: control groups with CCs ( $n = 20$  individuals) or without CCs ( $n = 16$ ) and high-status ( $n = 18$ ) and low-status ( $n = 15$ ) experimental groups.

An overall analysis indicated that the members of groups that included a CC (naturally occurring or confederate) contributed significantly more, almost three times as much, in the last 16 rounds ( $M = 10.98$ ,  $SD = 4.22$ ) as did the members of groups without a CC ( $M = 4.0$ ,  $SD = 4.0$ ),  $t(16.24) = 4.00$ ,  $p < .001$ . (CCs were excluded from these analyses.) Restricting the analysis to groups with naturally occurring CCs ( $M = 11.15$ ,  $SD = 4.05$ ),  $t(12.41) = 4.05$ ,  $p = .001$ ; to high-status CCs ( $M = 12.33$ ,  $SD = 4.56$ ),  $t(8.07) = 3.79$ ,  $p = .005$ ; or to low-status CCs ( $M = 9.73$ ,  $SD = 2.87$ ),  $t(7.17) = 2.76$ ,  $p = .03$ , led to the same significant effects. In addition, the contributions of members of groups with naturally occurring CCs versus confederate CCs did not significantly differ,  $t(13.73) = 0.18$ , *ns*.

The experimental groups also revealed an effect for status: Participants in high-status CC groups contributed significantly more than participants in low-status CC groups,  $t(31) = 1.91$ ,  $p = .03$ , one-tailed.<sup>7</sup> In addition, as in the previous three studies, CCs did well for themselves. Whether they were naturally occurring ( $M = 2,751$ ,  $SD = 555$ ),  $t(6.95) = 2.11$ ,  $p = .036$ , one-tailed; a PhD student ( $M = 2,633$ ,  $SD = 410$ ),  $t(7.93) = 2.84$ ,  $p = .03$ ; or a part-time secretary confederate ( $M = 2,312$ ,  $SD = 229$ ),  $t(7.26) = 1.96$ ,  $p = .05$ , one-tailed, their payoffs were significantly higher than the payoffs for group members whose groups did not

<sup>6</sup> This is similar to the rate of naturally occurring CCs in the archival data.

<sup>7</sup> Because the assignment of status was integral to this study’s design and because we could not control which identities were associated with naturally occurring CCs, control groups with naturally occurring CCs were excluded from this analysis and from the remaining analyses.



include a CC ( $M = 1,806$ ,  $SD = 470$ ). A comparison of the outcomes of the different kinds of CCs indicated that they were not significantly different from one another.

**Perceptions and social motives.** Forty-six of the participants could be classified as prosocial (i.e., cooperators;  $n = 17$ ) or proself (i.e., individualists or competitors;  $n = 29$ ). A  $2$  (CC or control)  $\times$   $2$  (prosocial or proself) multilevel analysis resulted in neither a significant interaction, nor, in this study, an effect for social motives. Figure 3 displays participants' choices across two-round blocks. The analysis and the figure suggest that social motives had no impact; instead, it appears that the only important variable was the presence or absence of a CC. This is particularly striking because an abundance of previous research in two-player settings has found significant interactions between social motives and the strategies of counterparts (e.g., Kuhlman & Marshello, 1975; McClintock & Liebrand, 1988; Van Lange & Visser, 1999).

Finally, as predicted, participants in CC groups perceived more cooperative social norms ( $M = 15.59$ ,  $SD = 3.50$ ) than did the participants in the control groups ( $M = 12.31$ ,  $SD = 4.03$ ),  $t(11.92) = 2.52$ ,  $p = .03$ .

**Evaluations of the CC.** In the experimental groups, the CC (confederate) was arbitrarily assigned to be Group Member 4. In the control groups, Group Member 4 was randomly determined. Thus, participants' perceptions of Group Member 4 provide the basis for important comparisons (see Table 2).

CCs were seen as more cooperative, less competitive, less concerned with their own outcomes, more concerned with group outcomes, and fairer than Participant 4s in the control groups. Interestingly, high-status CCs and controls were perceived to be more rational and more responsive to the choices of others than were low-status CCs.

Participants also evaluated their own actions. Although the individuals in CC groups were more cooperative than the individuals in non-CC groups, between-group analyses yielded no differences in their cooperative self-perceptions. This suggests that they may not have been aware of the impact that CCs had on their choices.

The presence of a CC also influenced how the members of their groups saw themselves. People in groups with high-status CCs

described their own choices as smarter ( $M = 6.00$ ,  $SD = 0.94$ ) than did people in groups with low-status CCs ( $M = 5.15$ ,  $SD = .80$ ),  $t(28) = 2.61$ ,  $p = .014$ . They also described their decisions as more rational ( $M = 6.00$ ,  $SD = 1.17$ , vs.  $M = 4.92$ ,  $SD = .86$ ),  $t(9.30) = 2.52$ ,  $p = .032$ , and themselves as more focused on group outcomes ( $M = 4.94$ ,  $SD = 1.64$ , vs.  $M = 3.77$ ,  $SD = 1.17$ ),  $t(28) = 2.19$ ,  $p = .044$ .

Participants also ranked each other on the influence of their fellow group members on their own choices. Despite the fact that both PhD student CCs and part-time secretary CCs had a significant impact on their choices, participants allotted credit differentially: Of the participants, 54% ranked the PhD student as most influential (or tied for most influential); none of the participants ranked the part-time secretary as being most influential. In addition, 31% of the participants in the high-status CC groups ranked the confederate as least influential, whereas 100% of the participants in the low-status CC groups ranked the confederate as least influential.

## Discussion

Study 4 clearly replicated the CC effects that we observed in the first three studies: People in groups with CCs contributed more than people in groups without CCs, and members of groups with CCs did significantly better than the members of groups that did not include a CC.

Study 4's results also showed that CCs can have positive effects on a group even when they have low status and when group members do not recognize their influence. These effects are even more noteworthy when we consider that the members of the low-status CC groups included a high-status group member who was not contributing consistently (i.e., the non-CC PhD student). Lower status CCs not only had to combat the group members' inherent temptations to defect but also the example of group members, some of higher status, who provided models of inconsistent contributions (at best). The fact that low-status CCs were not recognized as being influential attests even more to the power of CCs.

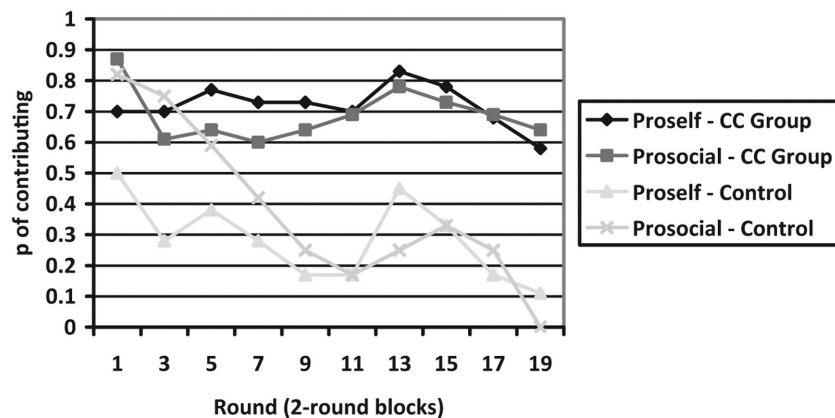


Figure 3. Probability of contributing by proselves and prosocials in groups with and without consistent contributors (CCs) in Study 4.

Table 2  
*Evaluations of Group Member 4, the Consistent Contributor (CC) in Experimental Groups Versus Random Other in Control Groups*

Item	Control group		High-status CC group		Low-status CC group	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Fair	3.8 <sub>a</sub>	1.86	6.18 <sub>b</sub>	1.55	6.33 <sub>b</sub>	1.45
Cooperative	3.8 <sub>a</sub>	2.01	6.47 <sub>b</sub>	0.94	6.33 <sub>b</sub>	0.98
Competitive	5.73 <sub>a</sub>	1.39	2.41 <sub>b</sub>	1.70	2.67 <sub>b</sub>	1.8
Rational	5 <sub>a</sub>	1.31	5.65 <sub>a</sub>	1.54	3.93 <sub>b</sub>	1.49
Focused on own outcomes	5.93 <sub>a</sub>	1.16	2.82 <sub>b</sub>	1.78	3.4 <sub>b</sub>	1.96
Focused on group outcomes	3.47 <sub>a</sub>	1.69	6.82 <sub>b</sub>	0.39	6.27 <sub>b</sub>	1.1
Responsive to others' choices	4.71 <sub>a</sub>	1.94	3.29 <sub>a</sub>	2.42	1.40 <sub>b</sub>	0.63

*Note.* Standard deviations appear in parentheses. Means with different subscripts within rows are significantly different from one another, with significance levels of at least  $p < .05$ , two-tailed. The only exception is the difference between experimental groups with respect to their responsiveness to others' choices, which was significant at  $p = .05$ , one-tailed.

Understandably, high-status CCs garnered particularly positive reviews: They were seen as being concerned, responsive (although they were not responsive), and rational. The presence and perceptions of high-status CCs also seemed to rub off, as their fellow group members considered their own decisions to be smarter, more concerned with group outcomes, and more rational than those in low-status CC groups. Yet none of the CCs—even high-status CCs—got much credit for influencing participants' decisions. Although the status of a CC mattered, it did not matter as much as the fact that they consistently contributed.

Unlike Study 3, Study 4's data indicate that CCs also influenced group members' perceptions of cooperative social norms. Because both studies used the same task and identical payoffs, we pooled the data for mediation analyses.

*Multilevel mediation analyses.* The combined data from Studies 3 and 4 included 93 participants in 28 groups.<sup>8</sup> Following Shrout and Bolger (2002), we used the bias-corrected bootstrap method to test for mediation. Bootstrap methods (Efron & Tibshirani, 1993) can generally be applied to test the significance between statistical estimates; they provide a more appropriate test of direct and indirect effects in mediation models than the more common Sobel (1982) test when the number of observations is small or moderate (Shrout & Bolger, 2002). The bias-corrected bootstrap also provides the greatest power for detecting indirect effects in multilevel mediation models and the most accurate confidence intervals (Pituch, Stapleton, & Kang, 2006).

The bootstrap procedure estimates the standard error of a statistical estimate empirically rather than using a mathematical formula. First, a computer program creates a data set that is akin to one in which the observed experiment would have been replicated many times. Here, 1,000 samples were created by randomly resampling (with replacement) from the observed data. The computer-generated data set is then used to create a confidence interval around the indirect effect. If this confidence interval does not include zero, the mediation test is statistically significant at a level of .05. This procedure is similar to that for single-level data, except that residuals, instead of cases, are resampled to better preserve the structure of the multilevel data (Goldstein, 2003).

This procedure assessed whether perceived cooperative norms mediated the relationship between the presence of a CC and the contributions of other group members. The true, indirect effect of perceived cooperative social norms was estimated to be between 0.18 and 1.65, with 95% confidence. Zero falls outside this confidence interval, leading to a conclusion that the indirect effect of cooperative social norms is significantly different from zero ( $p < .05$ , two-tailed). Thus, perceived cooperative norms mediated the effect of CCs on their fellow group members' contributions.

This finding is consistent with March's (1994) concept of a logic of appropriateness in individual decision making and Weber et al.'s (2004) application of this concept to social dilemmas. March suggested that people in novel social situations ask themselves "What does a person like me do in a situation like this?" Weber et al. argued that this process is particularly applicable when individuals face the ambiguity of a social dilemma. Indeed, the long-term appeal of the prisoners' and other social dilemmas may rest in part on the notion that arguments can support contributing or defecting, making the ultimate choice particularly difficult. The proximal mediator of choice, according to Weber et al., may be whether an individual defines the interaction as competitive or cooperative. The current findings suggest that CCs influence these basic definitions, and this is how they ultimately influence others to cooperate more.

### General Discussion

These four studies tell a simple, powerful story: CCs emerge, even when they might be least expected; the members of their groups contribute more and cooperate more often; and CCs themselves not only do not suffer from their risky cooperative actions, as most theories say they should, but actually seem to benefit from them (in three of four studies). Groups of more "rational" actors,

<sup>8</sup> The pooled sample involved all participants from Studies 3 and 4 who were included in all analyses (i.e., not excluded for reasons identified in each study).

in contrast, are not as efficient. In addition, the influence of CCs seems to be mediated by their effect on fellow group members' perceptions of their group's social norms, specifically by prompting fellow group members to see the social norms that apply as more cooperative than they would in the CC's absence.

Game theory and rational choice models make no allowance for CCs in repeated, finite interactions, especially when the parties are anonymous. The prescriptive advice that emerges from these dominant models is that an economically rational actor should not even think about being a CC, as doing so invites exploitation and the risk of being a sucker. In addition, rational models suggest that CCs will not influence their fellow group members' choices and, if they do, they will encourage opportunistic exploitation. None of these expectations were fulfilled in these four studies. Instead, the data indicate that a CC can have a positive impact on the cooperation problem in small groups.

We suggested that these effects might be caused by a CC's ability to engender cooperative perceptions of a group's social norms. The data from Studies 3 and 4 supported this hypothesis and are consistent with recent emphases on the "social" nature of social dilemmas (Messick, 1999; Weber et al., 2004) and earlier work on the development of social norms in prisoners' dilemmas (Bettenhausen & Murnighan, 1991). The fact that CCs sometimes had stronger effects on group member contributions than did the members' social motives (in Study 4) also suggests that CCs contributed to reshaping participants' perceptions of appropriate behavior in these interactions. Like Bettenhausen and Murnighan (1991), the findings indicate that clear personal signals to cooperate can be particularly effective.

These four studies also document that the effects of a CC are powerful: They influence their fellow group members' contributions even when the returns from contributions are relatively low, when social dilemmas are approaching a known endpoint, and when group members' personal inclinations are not prosocial. Thus, CCs' group members responded positively even when the context did not support cooperation. The same was true in Study 4, which suggested that CCs were just as influential for people with proself motives as they were for people with prosocial motives. These are particularly hopeful findings.

### *Limitations and Future Research*

Our research addressed a simple set of questions about the emergence and impact of CCs. Our findings cannot help in predicting exactly when CCs will surface and who from among a group's members they might be. Nor can we comment, on the basis of our data, on what motivates CCs or what might prompt CCs to stop contributing consistently. These are worthy topics for future research.

In addition, although the data supported our hypothesis about the potential mediating influence of cooperative social norms, these perceptions were measured after individuals had made a series of cooperative and/or competitive choices. Clearly, these choices might have influenced their reports of their perceptions. Thus, future research might also investigate the strength of these perceptions before or independent of actual choices. Future research might also measure social motives before observing peo-

ple's choices, as well as measuring a broader array of perceptual factors.

A significant body of research has indicated that people are highly motivated to avoid being suckers (e.g., Kelley & Stahelski, 1970). One way to avoid being singled out as a sucker is to be particularly sensitive to social norms, which CCs influence. With a high enough MPCR, however, the fear of being a sucker is also ameliorated. Thus, investigating a wide array of situations—including formal and informal social dilemmas, dilemmas with a commons dilemma structure instead of a public goods dilemma structure, or those that permit entry or exit (cf. Orbell & Dawes, 1993)—to determine the generality and the limitations of CC effects seems to also be a worthy topic for future research programs.

### *Practical Implications*

These findings may also have important practical implications. Should an individual who is joining a new group take the risk and be a CC? The alternative is to risk being in a group without one. Even though CCs seemed to benefit economically from their actions, they also tended to get relatively little credit for their positive influence, if they got any credit at all. Thus, future research might explore how consistent contributions can be encouraged and appreciated and how people can overcome the fears that are naturally associated with becoming a CC.

These data also provide further support for Kelley and Stahelski's (1970) observation that people consistently underestimate their roles in creating their own social environments. In particular, in the contexts that we studied here, the common characterization of self-interested choices as "strategic" or "rational" appears to be behaviorally inappropriate. Characterizing CCs as suckers may be both misleading and fallacious (see Moore & Loewenstein, 2004, p. 200). If "rational" choices maximize personal outcomes, our data suggest that the choice to be a CC can actually be rational. In this research, we examined CCs' effects, not their motives or strategies. The data suggest that in these kinds of groups, CCs are saviors rather than suckers.

A serious impediment to the emergence of CCs is the fact that like Axelrod's (1984) tit-for-tat players, CCs can never do better than the other members of their own groups. This means that CCs cannot do better than their exchange partners: Anyone who cooperates less, even if they ultimately move to mutual cooperation, will obtain better short-term outcomes than CCs. The common tendency to make social comparisons (Festinger, 1954) means that these outcome disparities will probably be noticed. Relatively disadvantageous outcomes are particularly noxious (e.g., Loewenstein, Thompson, & Bazerman, 1989), as is feeling exploited (e.g., Kelley & Stahelski, 1970). Thus, in the absence of formal agreements and binding contracts (which have their own problems; Malhotra & Murnighan, 2002), cooperative action can be exploited. The inclination to self-interested action may even be a common default (Moore & Loewenstein, 2004).

Long-term solutions to the cooperation problem may depend not only on the emergence of CCs, but also on garnering broad support when they do emerge. Indeed, the results documented here hinge on the capacity of CCs to encourage the cooperative action of others, thereby taking a vulnerable individual act and creating a more robust collective norm. Inducing individuals to think beyond

the borders of their own groups, to see that they can do well in intergroup rather than intragroup outcome comparisons, might help encourage CCs, but it is likely to remain a significant challenge (cf. Messick & Van de Geer, 1981). Also critical is the need to create greater transparency to highlight consistently cooperative behavior so that group members will be more immediately aware of a CC's actions.

Changing social scientists' conceptions of the meaning of rational action in mixed-motive interactions presents a challenge to successful social dilemma interventions on a broader scale. Our findings suggest that business strategy and public policy should reconsider their narrow definition of rational action in social interactions. The impact of undersocialized, "rational" models with little empirical support has raised concerns about bad public policy (Ferraro, Pfeffer, & Sutton, 2005; Ghoshal, 2005; Ghoshal & Moran, 1996; Moran & Ghoshal, 1996; Weber et al., 2004) and "a norm of cynicism and distrust" (Ostrom, 1998, p. 3). The counterpoint is a growing body of research that identifies strategic advantage in socially normative, cooperative "irrationality." Pillutla, Malhotra, and Murnighan (2003), for example, found that even in a risky, one-shot interaction, people who trusted completely received the highest average outcomes. Similarly, those who take significant risks early in relationships, with little rational justification, may accelerate trust development (Weber, Malhotra, & Murnighan, 2005).

A broad review of research on the capacity of rational choice models to account for and predict human decision making suggests that they are increasingly insufficient as contexts become more social (Weber et al., 2004). The optimistic implications of the current data provide substantive empirical support for Elster's (1985a) "snowball" model of cooperation, in which Kantian actors cooperate unconditionally and influence utilitarian actors:

The presence of unconditional cooperators—acting for the sake of duty or for the pleasure of participation—may be a necessary condition for the emergence of conditional cooperators. These, in turn, may bring the level of participation to the point at which new people join because they would be ashamed of being free riders. (p. 154)

If catalyzing cooperation in social dilemmas encourages future cooperation, as we would expect, then these findings provide an even stronger basis for optimism, as the actions of CCs might also augment the future outcomes—both substantive and in terms of morale—of social and organizational groups.

No group, organization, or institution can survive for long without the cooperative contributions of its members (e.g., Argyris, 1964; Organ, 1988). The research reported here suggests that Elster (1985b) was right when he argued that "a hard core of unconditional cooperators is vital for the survival of societies" (p. 245). Significant gains by groups and organizations may be elusive unless individuals take significant, personal risks that can catalyze effective collective action. A serious challenge for social action, then, is the creation of contexts that will encourage and support the emergence and recognition of CCs. As this research shows, these individuals' local actions can have important and valuable collective consequences.

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### Call for Nominations

The Publications and Communications (P&C) Board of the American Psychological Association has opened nominations for the editorships of **Developmental Psychology**, **Journal of Consulting and Clinical Psychology**, and **Psychological Review** for the years 2011–2016. Cynthia García Coll, PhD, Annette M. La Greca, PhD, and Keith Rayner, PhD, respectively, are the incumbent editors.

Candidates should be members of APA and should be available to start receiving manuscripts in early 2010 to prepare for issues published in 2011. Please note that the P&C Board encourages participation by members of underrepresented groups in the publication process and would particularly welcome such nominees. Self-nominations are also encouraged.

Search chairs have been appointed as follows:

- **Developmental Psychology**, Peter A. Ornstein, PhD, and Valerie Reyna, PhD
- **Journal of Consulting and Clinical Psychology**, Norman Abeles, PhD
- **Psychological Review**, David C. Funder, PhD, and Leah L. Light, PhD

Candidates should be nominated by accessing APA’s EditorQuest site on the Web. Using your Web browser, go to <http://editorquest.apa.org>. On the Home menu on the left, find “Guests.” Next, click on the link “Submit a Nomination,” enter your nominee’s information, and click “Submit.”

Prepared statements of one page or less in support of a nominee can also be submitted by e-mail to Emnet Tesfaye, P&C Board Search Liaison, at [etesfaye@apa.org](mailto:etesfaye@apa.org).

Deadline for accepting nominations is January 10, 2009, when reviews will begin.