Beyond the Group Mind: A Quantitative Review of the Interindividual–Intergroup Discontinuity Effect

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This quantitative review of 130 comparisons of interindividual and intergroup interactions in the context of mixed-motive situations reveals that intergroup interactions are generally more competitive than interindividual interactions. The authors identify four mechanisms of this interindividual–intergroup discontinuity effect, each based on the theoretical perspective that the discontinuity effect flows from greater fear and greed in intergroup relative to interindividual interactions. Results reveal that each moderator shares a unique association with the magnitude of the discontinuity effect. The discontinuity effect is larger when (a) participants interact with an opponent whose behavior is unconstrained by the experimenter, or constrained by the experimenter to be cooperative rather than constrained by the experimenter to be reciprocal, (b) group members make a group decision rather than individual decisions, (c) unconstrained communication between participants is present rather than absent, and (d) conflicts of interest are severer rather than mild.

One of the enduring issues in social science relates to whether individuals are prone to behave in a hostile and competitive manner when roused together in a group. Early formulations of this problem were based on observations made in the political arena. Plato, for instance, favored the rule of an enlightened individual over democracy because, in his opinion, democracy involved rule by irrational mobs (G. W. Allport, 1966). Plato's (trace. 1994) disgust of democracy comes to light in The Republic, in which Socrates declared that "until philosophers are kings, or the kings and princes of this world have the spirit and power of philosophy the Athenian state will not behold the light of day" (pp. 170-171). Approximately 2 millennia after Plato wrote The Republic, Alexander Hamilton,James Madison, and John Jay—the first two being members of the Constitutional Convention—authored the Federalist Papers under the pseudonym Publius. Although they were less dissimooe of democracy, they shared Pla's

to's wariness of groups in the political arena: "In all very numerous assemblies, of whatever character composed, passion never fails to wrest the scepter from reason. Had every Athenian citizen been a secret, every Athenian assembly would still have been a mob" (Publius, 1789/1948, p. 245).

Around the turn of the twentieth century, Le Bon (1895/1896) formulated the first systematic analysis of crowd behavior in Psychologie des foules. The crowds that Le Bon had found to be so prevalent were those of the National Assembly of France's Third Republic (Brown, 1954). Capturing the essence of his analysis, Le Bon (1895/1896) wrote, "Isolated he may be a cultivated individual, in a crowd he is a barbarian—that is, a creature acting by instinct" (p. 13). This idea was echoed most notably in McDougall's (1920) The Group Mind. Succeeding summarizing the issue, McDougall (1920) noted, "It is a notorious fact that ... the mental operations and actions of each member of the group are apt to be very different from those he would achieve if the faced the situation as an isolated individual" (p. 21).

Floyd Allport (1942) is well known among social scientists for his critique of thecrowd-mind concept. Nevertheless, in his later writing, F. Allport (1962) referred to the relation between the individual and the collective as the "major problem of social psychology" (p. 7). Quoting F. Allport (1962) directly,

"This quote is often attributed to Alexander Hamilton. Indeed, G. W. Allport (1960) suggested that, in comparison to Thomas Jefferson and James Madison Hamilton was particularly concerned that democracy could lead to mob tyranny. The mounting distrust and hostility between Republicans and Democratic Usage over the course of the contested U.S. Presidential Election of 1800 illustrated that his concern was not completely unfounded."

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One can, of course, discount as illegitimate the familiar argument that because individuals behave in a crowd as they would not behave alone, the crowd is therefore a "social entity" that is "revenant" or "descended upon" by its members. But even if we get rid of the crowd mind, the problem of describing the differential of crowd-like behavior would remain. (p. 6)

Unfortunately, from our perspective, F. Allport (1962) shared with other prominent social scientists an uncritical acceptance of an idea that was conceptualized inadvertently (Insko & Schober, 1998). Isolated individuals cannot act in the kind of hostile behavior that concerned Le Bon, McDougall, and others. Hostile, or cooperative, behavior requires the presence of a target and can therefore occur only in the context of social interaction. When individuals or groups are completely isolated, they are limited to activities such as working on tasks. We suspect that this explains why empirical investigations of social psychology's "master problem" have centered on comparing the task performance of individuals and groups, as illustrated by research on social facilitation (Tolman, 1897; Zajonc, 1965), individual versus group problem solving (Shaw, 1932; social loafing: Lamont, Williams, & Hartley, 1979), and brainstorming (Osborn, 1957).

This is, of course, not to say that research on such topics as social facilitation and brainstorming is uninteresting or unimportant. However, if the purpose is to determine whether groups are more cooperative than individuals then the cooperation cannot involve the isolated individual or the isolated group. The appropriate unit of analysis is the social interaction, and the basic comparison should be between interindividual interactions and intergroup interactions. Unlike many of his contemporaries, McDougall (1920) more have recognized this when he proposed that the group's influence on its members is stronger in the context of intergroup interactions involving conflict of interest.

Interindividual—Intergroup Discontinuity

Over the past 3 decades, social psychologists have studied the "differential of crowd-like behavior" by comparing interindividual and intergroup interactions in the context of mixed-motive matrix games such as the prisoner's dilemma game (PDG). The PDG involves an interaction between two sides (individuals or groups), usually over monetary outcomes. Each side can choose between a cooperative (X) and a noncooperative or competitive (Y) choice, and both sides' outcomes are determined by the combination of their respective choices. A sample PDG matrix is presented in Figure 1. That this is a PDG matrix can be verified by noting that its outcomes of the column player, for example, deviate in each row across cells from the upper right (X) to upper left (X) to lower right (Y) to lower left (Y). Note that whatever the average outcome is in the lower left (X) and upper right (Y) cells is lower than the variance in the upper left cell (X). This requirement guarantees that the players cannot receive higher outcomes by taking turns selecting X and Y than by both selecting X. The dilemma faced by both sides is reflected by the fact that, on any given trial, each side can maximize its outcomes by selecting the competitive Y alternative regardless of the alternative selected by the other side. Yet paradoxically, when both sides select Y, both achieve outcomes that are lower than the outcomes they could have achieved by mutual Y selections. As pointed out by Biddle and Biddle (1994), "basically saying anything in which you are tempted to do something, but know it would be a great mistake if everybody did the same thing is likely to be a prisoner's dilemma" (pp. 55–56).

Although a concern with the tension between self-interest and the common good can be traced back as far as Hobbes's (1651/1973) Leviathan, the problem was first cast in the shape of a 2 X 2 outcome matrix by Merrill Flood and Melvin Dresher in 1950 as part of the RAND Corporation's studies on global nuclear conflict (see Flood, 1952). Flood and Dresher's matrix was subsequently titled prisoner's dilemma by Princeton mathematician Albert Tucker, who illustrated the matrix with an anecdote about prisoners (see Poundstone, 1992). The anecdote describes two prisoners who each have a choice of invoking their right to remain silent (analogous to selecting X or giving evidence against the other (analogous to selecting Y)). Each prisoner can maximize his or her sentence by giving evidence against the other, but when both give evidence the prisoners can be convicted on a more serious charge than when both remain silent.

In a typical experiment on interindividual—intergroup discontinuity, participants are located in different settings that are connected to a central room. After examining a version of the matrix provided for a given trial, individuals or group representatives in the central room discuss possible actions with their opponent and then return to their bowseroms where they make a choice. The final decision is recorded on a form, which is carried back to the central room and given to the experimenter. The experimenter then announces the decisions made by the two individuals or groups and distributes the amount of money earned. Many, but not all, of these experiments found that intergroup interactions were significantly more competitive and less cooperative than interindividual interactions (e.g., Insko et al., 1987, 1990, 1992, 1993, 1994, 1998, 2001; Insko, Schober, Bingle, Daniels, & Gratz, 1990; McMillan et al., 1985; Insko, Gratz, Drijaia, & Smith, 1971; Sadoff et al., 1993, 1994, 1995, 2001). John Thibaut labeled this phenomenon a discontinuity effect, impressed in he was by Roger Brown's (1954) statement that "the quality of moral behavior has always required execution because of its apparent discontinuity with the private characteristics of the individuals involved" (p. 843). Several reviewers have occasionally questioned use of the term " discontinuity" on the basis that there may be a discontinuity in cooperation, as aggregates of individual income in turn. We have unprompted data (Thibaut & Insko, 1985), however, indicating that cooperation increases greatly as one moves from one-on-one interactions to two-on-two interactions and increases somewhat as one moves from two-on-two to three-on-three interactions but increases little themselves. In that study, at least, their (nearly appears to be a discontinuity between at-
We do not argue that intergroup interactions are more competitive than interindividual interactions under all circumstances. Our own research has been guided by the assumption that the discomfiting effect is most likely to occur under circumstances, such as those modeled by the PDG, in which there is some degree of noncorrespondence of outcomes, or conflict of interest (Kelley & Thibaut, 1978). How common are such situations? Although we have no definitive answer to this question, we are convinced that many situations that are encountered in everyday life feature the core elements of a PDG. Consider the well-known tragedy of the commons, first described by William Forster Lloyd (1833/1968) and more recently embraced by ecologist Garrett Hardin (1968, 1993). The tragedy unfolded in traditional agricultural communities with unmanaged commons—shared pastures for all villagers to graze their livestock with little regulation. Because the profits of having a large herd accrued exclusively to the individual herdman, whereas the costs in terms of exceeding the carrying capacity of the commons were incurred collectively by all herdsmen, such herdsmen could gain wealth by grazing increasingly large herds on the common. Yet, predictably, ever expanding herds ultimately left the common “bare-worn” and the livestock “pasty and starved” (Lloyd, 1833/1968, p. 30). The PDG is a binary representation of the commons dilemma and, as such, models any situation in which individual selfishness leads to collective detriment.

After discussing the historical concern with overgrazing, Raiffa, Richardson, and Metcalfe (2002) described other, more contemporary examples They discussed, for instance, the concerns with over-whaling and over-fishing in contrast to the “free-for-all” (Raiffa et al., 2002, p. 513) “associated with whaling, there has been successful agreement to regulate the hunt on a scale. They also described the problems associated with global warming and general degradation of the environment and noted that such problems appear to be more intractable, as “there are just too many free riders” (Raiffa et al., 2002, p. 514). Finally, they made an interesting observation regarding certain business practices. Besides the commons dilemma, there is a class of similar problems in business competition: zero-threat pricing, competitive advertising, and so on. The generic advice for competing firms is to collude or plan together. Such advice, of course, may be good for the business firms but bad for the consumer. Additional examples of the commons dilemma include problems of energy conservation (Sarno, 1990), provision of public services (Biel, Es, & Gurung, 1997), and resource distribution within organizations (Kramer, 1991).

Fear and Greed

Consider again the PDG matrix in Figure 1. Why might either player select the competitive alternative (I) in this situation? The most obvious reason for choosing the competitive alternative is the self-interest greed, associated with receiving the highest possible outcome (60 in Figure 1). Another possible reason is the fear of receiving the lowest possible outcome (20 in Figure 1). Greed is based on the expectation that the opponent will cooperate and is therefore valuable. Fear, on the other hand, is based on the expectation that the opponent will compete and therefore poses a threat.

Three complementary explanations have been advanced for the greater competitiveness of intergroup relative to interindividual interactions. Two of these explanations center on the greater greed in intergroup compared to interindividual interactions. The remaining explanation centers on the greater fear in intergroup compared with interindividual interactions. The social support explanation proposes that, given the expectation that the other side will cooperate, intergroup interactions are more competitive than interindividual interactions because group members can provide mutual social support for the competitive pursuit of immediate self-interest whereas such social support is unavailable to individuals (Izso et al., 1990; Schopler et al., 1993; Wildschut, Izso, & Gaertner, 2002). Matching cooperation with competition is inconsistent with norms of fairness and reciprocity. Social support in the shape of a competitive sugestion or act by one group member can reduce these normative constraints on the competitive behavior of other members of his or her group, thus increasing the likelihood that they will follow suit. This line of reasoning is consistent with the well-documented finding that social support from even a single ally can reduce normative act or other conformity pressures (Allen & Levine, 1960, 1971; Asch, 1955; Kiesler, Zanna, & De Saali, 1966).

The identifiability explanation proposes that, given the expectation that the other side will cooperate, intergroup interactions are more competitive than interindividual interactions because the other side’s ability to assign personal responsibility for competitive behavior is typically more limited in intergroup interactions than in interindividual interactions—group membership provides a shield of anonymity. Group members may avoid social sanction for violating norms of fairness and reciprocity by claiming that competition was initiated by other persons in their group (Schopler et al., 1995). This line of reasoning bears some resemblance to Latané and Darley’s (1970) discussion of diffusion of responsibility. They proposed that one of the reasons why people are more likely to help someone when they are the sole witness to the person’s distress than when others also witness the person’s distress (i.e., the bystander effect) is that personal responsibility for providing help can be more clearly assigned in the former than in the latter situation.

Finally, the schema-based distrust, or fear, explanation proposes that intergroup interactions are more competitive than interindividual interactions because the anticipation of interacting with another group activates an out-group schema, consisting of learned beliefs and expectations that intergroup interactions are aggressive, disrespectful, and competitive (Izso & Schopler, 1996; Penzliner, Izso, & Schopler, 1996; Wildschut, Izso, & Pinter, in press). Matching the anticipated competitiveness of the opponent’s PDG choice maximizes own outcomes, is consistent with norms of reciprocity, and guarantees equal outcomes for both sides. Because all salient considerations triangulate on selecting the competitive choice, fear-based competition does not depend on social support or anonymity (Schopler et al., 1993). Note that in violating the existence of general distrust of out-groups, we are not articulating a new idea. Campbell (1967) previously proposed that “if most or
all groups are in fact ethnocentric, then it becomes an "accurate stereotype to accuse an outgroup of some aspects of ethnoscen-
tic" (p. 823). Campbell (1967) went on to describe a "universal" out-group stereotype "of which each ingroup might accuse each ingroup, or some outgroup, or the average outgroup" (p. 823).

From the perspective of the observer, this universal stereotype of the out-group includes ethnocentrism, competitiveness, and hostility toward the observer's in-group.

**Overview**

A quantitative review (meta-analysis) of the interindividual–
intergroup discontinuity effect seems timely and appropriate for at least three interrelated reasons. First, in an attempt to thereby clarify the boundary conditions of the discontinuity effect, primary research has examined the difference between interindividual and inter-
group interactions under a wide variety of conditions. This has resulted in a highly differentiated literature consisting of studies that, when considered in isolation, may appear to be unrelated or even inconsistent. This meta-analysis serves to integrate the liter-
atur by abstracting from a large number of studies general prin-
ciples that govern interindividual–intergroup discontinuity and, in doing, to aid in theory development. Second, our current theo-
retical perspective proposes that the discontinuity effect is rooted in the greater fear and greed in intergroup relative to interindi-
vidual relations. The ultimate strength of this perspective resides in its ability to predict not only the outcomes of isolated studies but also patterns of variation in the magnitude of the discontinuity effect across a large number of studies. This meta-analysis allows us to test the strength of our perspective by examining the relation of theoretically derived moderators with the magnitude of the discontinuity effect across studies. Finally, by determining under which conditions intergroup relations are more competitive than interindividual relations, this meta-analysis aims to make accessi-
ble a body of research that may provide insight into the origins of intergroup conflict and assist policy makers in designing practical interventions to increase intergroup cooperation.

Explanatory models in meta-analysis must be interpreted with caution because meta-analysis is inherently a correlational process. Studies are not randomly assigned to conditions; hence, it is possible that different moderators predict the same variability in effect sizes (i.e., there is some degree of collinearity between the moderators). Furthermore, the mechanisms by which individual studies fall into different conditions are at least partly systematic, so that any quasi-design is likely to be unbalanced, and interactions between moderators are difficult to interpret. We used a combina-
tion of strategies to address the correlational nature of meta-
analysis and create a sound basis for our explanatory model. First, we identified a priori important moderators of the interindividual–
intergroup discontinuity effect—each based on the theoretical perspective that the discontinuity effect flows from greater fear and greed in intergroup relative to interindividual interactions. In the following sections, we present a detailed rationale for selecting each of these variables. Second, we predicted the magnitude of the discontinuity effect as an analysis of covariance-like function of these moderators. This allowed us to estimate the unique associ-
ation of each moderator with the discontinuity effect above and beyond the other moderators. Third, we performed sensitivity analyses in which other potential moderators were added to the explanatory model to examine whether we overlooked important variables and whether findings for the prior model changed when other variables were included. The details of these proce-
dures are described in the Method section.

**Moderators of Interindividual–Intergroup Discontinuity**

**Opponent Strategy**

Unconstrained interaction. The majority of interindividual–
intergroup discontinuity experiments contrast interindividual and intergroup behavior in the context of interactions in which the choice behavior of both sides is unconstrained. In these experi-
ments, both sides make a choice (X or Y) and then simultaneously exchange choices with the opponent. Usually this procedure is repeated for a number of trials. On the basis of the postulated fear social support, and identifiability explanations, intergroup interac-
tions are expected to be more competitive than interindividual interactions under these circumstances. A number of experiments, however, have been designed specifically to examine whether individuals and groups respond differently to certain programmed strategies. The most frequently investigated strategies in the studies we reviewed were cooperation strategies and reciprocal strateg-
ies, such as tit-for-tat (Axelrod, 1984).

**Reciprocal strategies.** The tit-for-tat strategy begins with a cooperative choice and then responds on each subsequent trial in the same way the opponent responded on the preceding trial (i.e.,
tit-for-tat reciprocates the opponents' choices). This strategy be-
came well known for winning Axelrod's (1984) computer tournaments. These tournaments involved repeated rounds of play be-
tween competing strategies entered by various experts on game theory. The strategy has drawn the attention of researchers because it offers "an interesting procedure for creating trust without at the same time making oneself (or one's group) totally vulnerable to exploitation by the opponent" (Dawkins et al., 1990, p. 696).

From this perspective, the most compelling evidence is perhaps not a computer tournament but Axelrod's (1984) description of trench warfare in World War I. After arguing that the situation confronting the Allied and German troops on the Western front can be described accurately by the PDG, Axelrod cited extensive documentary evidence that in many sectors of the Western front troops ceased hostilities despite receiving orders to the contrary. The troops, for example, fired over each other's heads or shelled only certain weak sections at certain times of the day. Axelrod (1984) suggested that the troops' behavior was caused by the permanence of the trench lines. In his own words: "What made this mutual restraint possible was the static nature of trench warfare, where the small units faced each other for extended periods of time" (Axel-
rod, 1984, p. 211). According to Axelrod (1984), this status quo led to an enlarged "shadow of the future" (p. 126), or an understanding that, in the long run, mutual cooperation would be more beneficial than mutual competition.

We agree with Axelrod's (1984) interpretation and believe that it can be applied directly to the PDG setting. Consider the column player in the PDG (see Figure 1). As noted above, regardless of the row player's choice, the column player always receives higher outcomes by competing. Thus, from the standpoint of maximizing outcomes in the short run, it is rational to select the competitive choice. From the standpoint of maximizing outcomes in the long
run, however, the situation is not that simple. If both players attempt to maximize short-term outcomes by competing, the result is a competitive deadlock that prevents both players from maxi-
mizing outcomes across a number of trials. Thus, from the stand-
point of maximizing long-term outcomes, both players should be 
motivated to achieve mutual cooperation and avoid mutual com-
petition (Insko et al., 1990).

The World War I troops engaged in reciprocal behavior that was, if not purely tit-for-tat, very similar to tit-for-tat. Axelrod 
(1984) warned against a "narrow focus on a pure TIT FOR TAT 
strategy" (p. 61) and devoted much attention to variants of tit-for-
tat, including the less forgiving two-for-one strategy documented 
by Kelly (1930): "It was French practice to 'let sleeping dogs lie' 
. . . and of making this clear by retorting vigorously only when 
challenged. In one sector . . . they fired two shots for every one that 
came over, but never fired first." (p. 38). The two-for-one strategy 
shares two key features with tit-for-tat: Neither strategy is ever 
first to compete, and both strategies compete after a competitive 
response by the opponent. Another strategy that closely resembles 
tit-for-tat is the one-for-two strategy, which retaliates only after 
two competitive responses by the opponent. Like tit-for-tat, this 
strategy never initiates competition and cooperates after a cooper-
ative response by the opponent. From our perspective, there is 
sufficient resemblance among the tit-for-tat, two-for-one, and one-
for-two strategies to bring them together under the rubric of 
reciprocal strategies.4 We expected that reciprocal strategies fol-
lowed by one opponent in the context of continuing interactions 
would lead to a reduction of the discontinuity effect, compared 
with the usual situation in which the behavior of both sides is 
unconstrained.

Why should this be the case? Consider the situation in which one 
group interacts with another group, and the behavior of both 
groups is unconstrained. From the perspective of the postulated 
fear, social support, and identifiability explanations there should 
be some intergroup competition in this situation. When playing 
against a reciprocal opponent, however, competition begets com-
petition. Reciprocal strategies therefore make it very clear that 
competitiveness will not result in the maximization of long-term 
outcomes and, hence, discourage short-term greed. This weakens or 
removes one basis of the discontinuity effect the greater greed 
in intergroup compared with interindividual interactions. Further-
more, because reciprocal strategies respond to cooperation with 
cooperation, there is a strong implication that long-term outcomes 
will be maximized through cooperation. Of course, the shift from 
a concern for short-term outcomes to a concern for long-term 
outcomes requires mutual trust, but reciprocal strategies may build 
trust and reduce fear because they never initiate competition and are 
"entirely comprehensible to the other player" (Axelrod, 1984, 
p. 122; cf. Komorita, Hilty, & Parks, 1991). This weakens or 
removes another basis of the discontinuity effect: the greater fear 
in intergroup compared with interindividual interactions.5

Cooperative strategies. Several discontinuity experiments have 
contrasted interindividual and intergroup behavior in the context of 
interactions with a cooperative opponent. It is clear that an oppo-
ponent who cooperates consistently should reduce or eliminate dis-
trust. This removes one basis for the discontinuity effect: the 
greater fear in intergroup compared with interindividual interac-
tions. But what about greed? There are at least two possible ways 
in which a cooperative strategy could reduce greed. First, it is 
possible that a cooperative strategy reduces greed because greed-
based actions are frequently justified through defensive assertions. 
Insko et al. (1993) cited anecdotal evidence that diplomats of the 
former Soviet Union sometimes justified Soviet imperialism for 
ward Eastern Europe on the basis of Russia's traditional fear of 
being invaded from the west. In the absence of out-group threat, 
greed cannot be justified in this way. Second, as we proposed 
earlier, when cooperation begets cooperation, a concern for long-
term outcomes may take precedence over a concern for short-term 
outcomes.

Note, however, that there is a critical difference between the 
cooperative strategy and the reciprocal strategies. Whereas with 
reciprocal strategies competition begets cooperation, with a coop-
ervative strategy competition begets cooperation. Because the co-
operative strategy does not punish short-term greed in the same 
way as reciprocal strategies do, it is vulnerable to exploitation. 
Shure, Meeker, and Handorf (1965), for instance, observed that in 
a bargaining context "the pacifist's tactics apparently invite ex-
ploration and aggression, even among those who do not begin 
with such intentions." (p. 116). From the perspective of discontin-
uity reduction, reciprocal strategies should therefore be more ef-
ficient than a cooperative strategy. Whether a cooperative strat-

4 This is not to say that differences between the reciprocal strategies are unimportant. Research comparing tit-for-tat with more and less forgiving 
variants appears to be consistent with Axelrod's (1984) intuition that the 
suitable level of forgiveness is determined by the environment. "If the main 
danger is unrelated mutual incriminations, then a generous level of for-
giveness is appropriate. But, if the main danger is from strategies that are 
good at exploiting emerging rules, then an excess of forgiveness is costly. 
(p. 120; cf. Bender, Kraser, & Stout, 1991; Kolbock, 1993; Komorita et al., 
1991; Van Lange, Oerder, Taatgoud, 1993). This is because the number of 
small number of effect sizes related to a tit-for-tat (N = 4), a two-for-one 
(N = 2), and a one-for-two (N = 2) rolled out meaningful 
comparisons among the three strategies,

5 In an attempt to explain the success of the tit-for-tat strategy in his 
computer tournaments, Axelrod (1984) pointed out that "TIT FOR TAT 
. . . is never the first to defect; it forgives an isolated defection after a single 
response; but it is always incited by a defection no matter how good the 
interaction has been so far." (p. 46). In an article predicting Axelrod's (1984) 
The Evolution of Cooperation, Erev and Kornieta (1975) advanced a 
somewhat different perspective. They found that a reciprocal strategy was 
most effective in a bargaining situation and proposed that this was the case, 
as stated by Komorita and Erev (1977), "not because subjects were 
pressed to exploit the other but because they recanted and refused to be 
intimidated by the unorthodox of an opponent who did not reciprocate 
concessions." (p. 69). Erev and Erev, however, offered no evidence 
that was incompatible with this fairness interpretation. They inter-
preted their evidence as consistent with results obtained by Shure et al. (1965), 
indicating that "subjects who were cooperating with the opponent's 
cooperative orientation were very likely to exploit this vulnerability." 
(Kornieta & Erev, 1975, p. 705). Thus, the difficulty, or impossibility, of 
exploiting tit-for-tat emphasized by Axelrod again enters the picture. 
Finally, we should note that Pruitt and Kimbell (1977) asserted that "a 
perception that the other is fair is tantamount to an expectation that he will 
cooperate." (p. 303). We find this assertion reasonable, and evidence 
obtained by Insko et al. (1996) indicates that the usual greater degree 
between groups than between individuals was not present when partic-
pants interacted with an opponent who followed a tit-for-tat strategy, 
whereas this difference was present when participants interacted with an 
opponent whose strategy was unconstrained.
ogy reduces the discontinuity effect relative to a situation in which the behavior of both sides is unconstrained remains an open question. Although it is clear that the cooperative strategy reduces fear, it is less clear whether this strategy reduces or rewards greed.

Miscellaneous strategies. A small minority of discontinuity experiments have used programmed strategies that are neither reciprocal nor cooperative. One example of such a strategy is called 

**Procedural Interdependence**

In the majority of discontinuity experiments, group members are required to follow a consensus rule and make a collective group decision. Results from two early experiments suggest that a consensus rule is a prerequisite for the discontinuity effect (Insko et al., 1987, 1988), but a subsequent experiment by Insko et al. (1994) calls this idea into question. Insko et al. (1994) compared interactions between individuals, groups, and teams with required consensus in the context of one of two n-person generalizations of the PDG: the intergroup public goods (IPG) game (Rappaport & Bornstein, 1987) and the intergroup prisoner’s dilemma (IPD) game (Bornstein, 1997). In both the IPG and IPD, players decide whether to invest or not to invest. The basic structure of the IPG and IPD is analogous to that of the PDG in that, on any given trial, each side can maximize its outcomes by investing more public goods than the other side. Yet, when both sides invest all their public goods, both achieve lower outcomes than they could have achieved had neither side invested. Investing in an IPG or IPD is thus equivalent to competing in a PDG.

In Insko et al.’s (1994) experiments, group members each had one propositional rule, and individuals each had three rules. When players between two groups, both games involve separate decisions by individual group members as to whether they want to invest their propositional rules; group members’ individual decisions then determine jointly the group’s level of investment. When played between two individuals, each player decides whether to invest one, two, or three propositional rules. Insko et al.’s (1994) results indicated that groups who made more competitive (i.e., invested more) than individuals in both IPG and IPD. Seemingly inconsistent with the earlier experiments of Insko et al. (1987, 1988), there was no significant effect for required consensus.

Why is a consensus requirement important in the context of a PDG but not in the context of an n-person generalization of the PDG? Note that consensus decisions in the PDG and group decisions in the n-person generalizations of the PDG share a key feature. In each case, the group members’ decisions are translated into a collective group choice. In the case of consensus decision with the PDG this is straightforward. The group members discuss their preferences during a within-group discussion period and then decide on a collective group choice. In the context of the n-person generalizations of the PDG, things are not as straightforward. Although group members decide individually whether to invest and discover, the total number of votes invested by the entire group is determined by the joint actions of the individual group members. Thus, in both the PDG and the n-person games there is an interplay between the decisions of own group members’ decisions and outcomes, a state of affairs called 

**Procedural Interdependence**

Insko et al. (1994), Witschge et al., in press; Witschge, Lade-

cijka, & Insko, 2001.) With the PDG, a consensus requirement may be a prerequisite for procedural interdependence, whereas with the more complex n-person games, a consensus requirement may not be necessary.

There are at least two possible reasons why procedural interde-


tendence is important. One reason relates to greed and the other to fear. With respect to greed, note that procedural interdependence creates a situation in which the collective group decision cannot be traced back to the individual group members. This creates a shield of anonymity that facilitates self-interested, competitive behavior. Group members may avoid the appearance of selfishness by claiming that their cooperative behavior was prompted by other group members. With respect to fear, note that the anticipation of interacting with a group of procedurally interdependent persons acting collectively may lead more readily to activation of the negative out-group schema than the anticipation of interacting with a group of procedurally independent persons acting individually (Witschge et al., 2001, in press).

A number of discontinuity experiments have contrasted interin-

dependence and intergroup interactions in a context in which group members are not procedurally interdependent. These experiments typically involved a procedure in which group members were allowed to discuss their decisions with the other members of their group, but they ultimately made an individual decision and exchanged this decision on an individual basis with a member of the other group. We assumed that under these circumstances group members are less anonymous, and therefore, are less likely to pursue short-term self-interest. One basic for the discontinuity effect is thus weakened or removed. Furthermore, fear should be reduced when members of opposing groups interact on an inter-

dividual basis. This weakness or removes another basis of the discontinuity effect. We therefore predicted that the discontinuity effect would be larger in studies in which group members were
communication

Unconstrained communication. In the majority of discontinu-
osity experiments, participants were allowed to communicate with
each other before making their decisions on a given trial. In the
intergroup condition, these communication sessions typically in-
volved representatives of each group. These communication ses-
sions frequently resulted in agreements to cooperate at the
upcoming trial or at least an assertion by one side or the other that
they would cooperate on the upcoming trial. For example, coding
of tape-recorded intergroup discussions in a one-trial experiment
with a three-choice variation of the PGG (Schoeper et al., 1995)
indicated that there was an agreement to cooperate in 86% of the
intergroup sessions and 76% of the intragroup sessions.

There are at least two reasons why intragroup interactions are
likely to be more competitive than intergroup interactions in
the presence of unconstrained communication. First, the fear ex-
ploration proposes that there is greater distrust in intergroup than
in intragroup interactions. The influential research program on
attitudes and communication conducted at Yale University in
the 1950s identified mistrustworthiness as an important component
of source credibility (Fordand, Janis, & Kelley, 1955). Subsequent
research has demonstrated that the mistrustworthiness of the source
affects the persuasiveness of a communication (Walster, Aronson,
& Abrahams 1966). These findings suggest that the schema distin-
guishing distrust of out-group members intergroup communication less
credible and persuasive than individual communication and,
moreover, contributes to the group competiveness in intergroup
relative intragroup interactions (Insko et al., 1993). Second,
even when communication of cooperative intent is perceived as
credible and persuasive, groups should be more likely than indi-
viduals to attempt to exploit a cooperative opponent because the
group context provides anonymity and social support for the
predisposition of short-term self-interest (Wildschut, 2002).

Coordinated cooperative communication. When there is un-
constrained communication, the behavior of the two players is not
independent. The two sides are in a network of analysis that becomes
the interaction between players rather than the player per se. Because
unconstrained communication frequently results in an agreement
to cooperate at the upcoming trial, such sessions involved proc-
dures in which the communication between players was controlled
by the experimenter. For example, Lodewijk, Wildschut, Syron,
Verve, and Rabbie (1999) used a procedure in which individuals
and groups exchanged written notes as a means of communication.
The experimenters collected these notes and replaced them with
standard notes that indicated cooperative intent. Similarly, Biedens,
Vandebaelle, and Van Avermaet (2001) used a proc-
dure in which individuals and groups communicated with their
opponent through a telephone. In reality, participants communi-
cated with a confederate who always expressed cooperative intent.
The advantage of these procedures is that, because communication
is controlled by the experimenter, the players’ behavior is inde-
pendent, and the statistical tests of analysis become the player per
se. Given the same number of participants, these procedures yield
results twice as many independent observations as procedures in
which communication is unconstrained.

Will co-sponsored cooperative communication influence the mag-
nitude of the discontinuity effect as compared with situations in
which communication is unconstrained? This is an open question.
On the one hand, it is the case with unconstrained communication,
coops should be less likely than individuals to pursue con-
strained cooperative communication as credible and persuasive.
Furthermore, even when constrained cooperative communication is present as credible, groups should be more likely than indi-
viduals to exploit the cooperative other. On the basis of these
considerations, one would not expect the magnitude of the discon-
 tinuity effect given constrained cooperative communication to
be different from the magnitude of the effect given uncon-
strained communication. On the other hand, it is unclear whether
co-sponsored constrained communication in the form of somewhat impersonal
handwritten notes or telephone conversation is sufficient to
create trust between individuals. Relevant to this issue, Wieman (1972)
examined interindividual PGG interactions under four conditions:
In the isolation condition, participants could not communicate in
any way; in the see-only condition, participants could never
verbal but no interpersonal communication; in the hear-only condition,
participants could use verbal but no interpersonal communication;
and in the see-and-hear condition, participants could use both
eral and interpersonal communication. The latter condition pro-
duced the highest levels of cooperation, followed by the hear-only,
see-only, and isolation conditions, respectively. Wieman’s find-
ings suggest, at the general level, that not all forms of communica-
tion are equally effective in creating trust between individuals
and, at a more specific level, that unconstrained face-to-face com-
unication may be more effective in creating trust between indi-
viduals than written notes or telephone conversations. If the latter conclusion is correct then the discontinuity effect
should be smaller in studies with some form of constrained coop-
erative communication than in studies with unconstrained communication.

No communication. Some discontinuity experiments include
neither unconstrained nor constrained communication. It is clear
that without communication the discontinuity effect should be
greater than with communication. Furthermore, because commu-
nication between individuals is perceived as more credible and
persuasive, the discontinuity effect between groups, the absence of
communication should be more detrimental to interindividual than
to intergroup interactions. This is not to say that intergroup inter-
actions are affected by communication, but it is to say that
interindividual interactions are affected more strongly. Because the
absence of communication yields a larger increase in interindiv-
idual than in intergroup competition, we expected that the dis-
continuity effect would be smaller for studies in which communi-
cation was absent than for studies in which some form of
communication was present.

Noncorrespondence of Outcomes

The four matrices depicted in Figure 2 represent different ways
in which variations in one player’s outcomes can be related to
changes in the other player’s outcomes. Thibaut and Kelley (1959)
labeled this property correspondence of outcomes and conceived
of it as an index of conflict of interest. In articles with symmetrical
outcomes, correspondence is indexed by the correlation between
the row and the column players' outcomes. The matrix at the top of Figure 7 represents a situation in which the outcomes of the row
and column players are correspondent. In this situation, the choice that is best for the column player is also best for the row player and
vice versa. Correspondence of outcomes, as indexed by the corre-
lion between the players' outcomes, is 1.00. The matrix at the
bottom of the figure represents the other extreme of the correspond-
dence dimension, perfect noncorrespondence of outcomes. In this
zero-sum situation, the players' outcomes are correlated −1.00.
The response that is best for the column player is worst for the row
player and vice versa. The middle of Figure 2 contains two PDG
matrices, each of which models a situation in which there is some
degree of noncorrespondence of outcomes. The indices of noncor-
respondence for the matrix depicted on the left and the matrix
depicted on the right are −.60 and −.92, respectively.

For the matrix depicted at the top of Figure 2, mutual X choices
benefit both players more than mutual Y choices. The X choices
therefore may involve cooperative intent or simple pursuit of self-interest. The Y choices, on the other hand, do not benefit one
player over the other and therefore cannot involve cooperative intent. With matrices in which the outcomes of both players are
correspondent (i.e., positively correlated), mutual X choices ben-
efit both players. Because this is true for relations between groups
and relations between individuals, there is no reason to expect a
difference between intragroup and intergroup interactions. In
such situations it should be evident to both groups and individuals
that what benefits one also benefits the other.

For the matrix depicted at the bottom of Figure 2, the Y choice,
compared with the X choice, benefits one player over the other.
The Y choice therefore may involve either competitive intent or
simple pursuit of self-interest. On the other hand, mutual Y choices,
compared with mutual Y choices, do not benefit both players. The X choice therefore cannot involve cooperative intent.
In this situation there is no one choice that benefits both players.
Given that this is true for relations between groups and relations
between individuals, there is no reason to expect a difference
between intragroup and intergroup interactions. However,
when outcome arrays represent less than perfect noncorrespon-
dence, a discontinuity effect is expected. This situation is rep-
resented by the two PDG matrices in the middle of Figure 2. Here,
mutual X choices, compared with mutual Y choices, benefit both
players, whereas the Y choice, compared with the X choice, ben-
et one player over the other. Hence, the X and Y choices
represent a clear distinction between cooperative and competitive
alternatives.

The above-described arguments imply that the domain of dis-
continuity is restricted to the values of noncorrespondence that
range from zero down to, but not including, −1.00 (Schopler et al.,
2001). Within this domain of applicability, we predicted that
decreases in noncorrespondence of outcomes (i.e., less negative
index values) would be associated with reductions in the magni-
tude of the discontinuity effect. Specifically, we expected that
decreases in noncorrespondence of outcome would be associated
with greater reductions in intergroup than in intragroup com-
petitions. Possible reasons why individuals may not be as strongly
affected as groups by degree of noncorrespondence are related to
the fear, social supports, and identifiability explanations. It is clear
that degree of noncorrespondence has no bearing on the fact that
individuals do not have social support for being greeks or yanks
interests or on the fact that individuals can be identified as being
responsible for self-interested behavior. But what about fear of
disconnectivity? Conceivably, degree of noncorrespondence may influ-
ence the level of distress. However, past research has demonstrated
a strong tendency for individuals to trust other individuals when
opportunities for communication are present (Janks et al., 2001).

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1 This generalization holds for outcome arrays that are characterized by
column and row-sum effects but not by a column X row interaction—or,
in the language of transitivity theory (Kelley & Thibaut, 1978), arrays
that contain effective control (EC), the extent to which the player's choice
affects his or her own outcomes and joint control (JC), the extent to which
the player's outcomes are affected by the other's choice) but not between
control (BC); the extent to which joint actions of both players influence
the player's outcomes). Consider, for example, the matrix in Figure 1. By
selecting Y, each player can increase own outcome by 10. Therefore,
BC = 10. Similarly, by selecting X, each player can increase the outcomes
of the other player by 30. Therefore, JC = 30. The BC and JC components
are not affected by the other player's decision. Therefore, BC = 0. Kelley
and Thibaut (1978) approached the PDG exclusively in terms of JC and
PC components and asserted that the presence of a JC component changes
the intuition from one of pure exchange to the containing elements of
cooperation.
Thus, the three components of the behavior of individuals (resisting the abuser of anonymity), social support, and fear) make it less likely that variation in the degree of correspondence affects interindividual interactions.

There are two reasons why we expect the situation to be different for groups. One reason relates to gender and the other to fear. Note that with respect to gender, the lack of correspondence becomes less negative, there is a decreasing advantage of the Y choice relative to the X choice. This can be verified by referring to the two matrices depicted in the middle of Figure 2, both of which fall within the domain of applicability of the discontinuity effect. The decreasing advantage of the X choice reduces the likelihood that group members will be tempted to pursue their short-term self-interest and act on the sexual support and anonymity afforded by the group context. This reduces or removes one basis of the discontinuity effect. With respect to fear, note that in the index of correspondence becomes less negative, there is an increasing difference between the outcomes in the upper left (XX) and lower right (YY) cells (see middle of Figure 2). This implies that the potential loss flowing from mutual Y choices is increased. The possibility of such increased loss may reduce the tendency of groups to interact with each other. This idea is consistent with the cold-war doctrine of mutually assured destruction (Kissinger, 1956; Whooler, 1959). Trust may develop when both sides realize that mutual competition leads to mutual destruction. This reduces or removes another basis of the discontinuity effect.

Method

Sample of Studies

As an initial step, we organized relevant studies cited or supervised by the project authors and consulted researchers known to have conducted published or unpublished studies on the discontinuity effect. We also sent a request for relevant studies to an Interpreters Discussion Forum commonly used by social psychologists (tipp-untippecmail.soci). Next, we conducted an electronic literature search using the PsycINFO (1987—July, 2000) and Dissertation Abstracts International (1980—July, 2000) databases. Key words were included: group, interindividual, intergroup, discontinuity, cooperation, and competition. As a final step, we conducted a backward search of reference sections of the retrieved articles until we found no new entries.

As pointed out by Schütte and Finley (1992), the decision of discontinuity research is "slightly blurred at the boundaries" (p. 124). To happen and clarify these boundaries, we used the following selection criteria: first, we selected only those studies that compared interindividual and intergroup interactions in the context of mutua1-intensiveness (i.e., situations that are characterized neither by pure correspondence of outcomes nor by pure correspondence of outcomes). Studies included in the train of this criterion include a few studies that compared aggressive behavior of interindividual and intergroup interaction. Second, we excluded the trials on the basis of this criterion (include four studies that compared aggressive behavior of interindividual and intergroup interaction). Second, we did not include interactions that in the experience of one individual or another individual in achieving a monetary gain and not administering a shock at the expense of the mon-

Data Coding

The selected studies were coded by Tim Wildschut and Barid Pinter. Each coded approximately half of the studies. Both coders subsequently checked the entire data set and resolved disagreements—of which there were six through discussion. Unresolved selected studies were processed by both coders to assess interrater reliability. For these studies, the coders were in perfect agreement with respect to coding of operational strategy, procedural interdependence, communication, and roles of correspondence.

Opposite strategy. Opposite strategy was coded as a categorical variable with four levels: uncoordinated, reciprocal, cooperative, and pair-conflict. In studies that were characterized by an uncoordinated strategy, actual participants made up both sides in the interaction. The down-side behavior of their participants was not controlled in any way by the experimenter.

In studies that were characterized by a reciprocal strategy, only one side in the interaction consisted of actual participants. These participants interacted with an opponent who was instructed to respond to their behavior. This kind of strategy is similar to two-way interactions, except that they are considered to be cooperative because of two or more cooperative choices that they make. We consider such interactions as instances of two cooperative choices or two cooperative choices by the participant.

In studies that were characterized by a cooperative strategy, again only one side in the interaction consisted of actual participants. These participants interacted with an opponent who was preselected to select for cooperative choices regardless of the participants' choice behavior. Finally, studies that did not involve either an uncoordinated, reciprocal, or cooperative strategy were assigned to the miscellaneous category.

Procedure. Interdependence was coded as a categorical variable with two levels: procedural interdependence and group interdependence. Experimental sessions were presented to 135 students of 13 students enrolled in 11 different laboratories. All selected students were laborers' students, using 134 independent-effect sites (i.e., each effect size was based on a unique sample of participants).1

1 A log with complete study and effect size information can be obtained from Tim Wildschut.
still make their decisions individually, we did not consider them to be procedurally interdependent.6

Communication. Communication was coded as a categorical variable with three levels: unaccompanied communication, constrained cooperative communication, and no communication. In studies that were characterized by unaccompanied communication, both sides in the interaction consisted of actual participants. The two sides were allowed to discuss the choices that they considered selecting during a brief communication period (typically 1 min) that preceded their actual choice on a given trial. Participants were instructed that any agreements reached during the communication period were nonbinding and that the communication period was meant to discuss the choices that they considered selecting. Otherwise, the content of the discussion was not constrained or controlled by the experimenter in any way.

In studies that were characterized by constrained cooperative communication, only one side in the interaction consisted of actual participants. Although participants were told that they had the opportunity to communicate with the other side, this communication did not involve face-to-face contact. Instead, participants communicated with the other side by means of headsets to waking notes or telephones. Unbeknownst to the participants, these messages were controlled by the experimenter.

In studies that were characterized by the absence of communication, no mention of communication between the sides was made. Studies in this category did not involve any verbal or visual contact between sides. Decisions were exchanged between sides by the experimenter.

Index of noncorrespondence. Index of noncorrespondence was coded as a continuous variable. Following Kelley and Thibaut (1978), the index of noncorrespondence is defined as

\[
INC = \frac{1}{2} \left( \frac{1}{n_A} - \frac{1}{n_B} \right)
\]

Here, INC refers to the index of noncorrespondence, \(\frac{1}{n_A}\) refers to the variance of the cell sum (i.e., the values obtained by adding the outcomes of Side A and B in each cell), and \(\frac{1}{n_B}\) refers to the variance of the cell differences (i.e., the values obtained by subtracting the outcomes of Side B from the outcomes of Side A in each cell). Alternatively, the index of noncorrespondence can be defined in terms of its relation to the product-moment correlation between the outcomes of Sides A and B (r):

\[
INC = \frac{1}{(2 - r) \cdot n_A \cdot n_B}
\]

Here, r refers to the standard deviation of Side A's outcomes, and \(\frac{1}{n_A}\) refers to the standard deviation of Side B's outcomes. For symmetrical matrices, such as the ones presented in Figures 1 and 2, the bracketed term equals unity, and the index of noncorrespondence is identical to the product-moment correlation between the outcomes of Sides A and B.

Other variables. For each interindividual-intergroup comparison, descriptive information and additional variables were coded for exploratory analyses. These variables included: author and full citation, source (journal, book chapter, or chapter of book), laboratory location (United States or Europe), affiliation of primary investigator (University of North Carolina at Chapel Hill or other), sample (college students, community sample, or school children), experiment method (participant pool, monetary incentive, or volunteer), sex composition of the sample (all males, all females, males and females in interactions that were homogeneous with respect to sex, or males and females in interactions that were heterogeneous with respect to sex), \(\frac{1}{n_A}\) grand mean of the matrix values, nature of the matrix payoff (money or points), and number of trials anticipated (single or multiple).

Statistical Methods

Comparison of effect sizes. The effect size index was Hodges' \(g\) (Hedges & Olkin, 1985), calculated such that positive values indicate greater competitiveness in intergroup than in interindividual interactions. Hodges' \(g\) was defined as

\[g = \frac{M_{\text{intergroup}} - M_{\text{interindividual}}}{SD_{\text{shrink}}}
\]

Here, \(M_{\text{intergroup}}\) is the mean proportion of intergroup competition, \(M_{\text{interindividual}}\) is the mean proportion of interindividual competition, and \(SD_{\text{shrink}}\) is the pooled standard deviation:

\[SD_{\text{shrink}} = \sqrt{\frac{d_{\text{intergroup}}^2 + d_{\text{interindividual}}^2 + (d_{\text{intergroup}} - d_{\text{interindividual}})^2}{2}}
\]

Here, \(d_{\text{intergroup}}\) and \(d_{\text{interindividual}}\) are the number of observations in the intergroup and interindividual conditions, respectively; \(d_{\text{intergroup}}\), and \(d_{\text{interindividual}}\) are the variances in the intergroup and interindividual conditions. Depending on the amount of statistical information reported, we used one of the three approaches described below to compute Hodges' \(g\). These approaches are rank ordered in terms of preference, starting with the most preferred method.

6 Some studies used a one-step experimental design that included—in addition to an interindividual condition—an intergroup condition in which procedural interdependence was present as well as an intergroup condition in which procedural interdependence was absent (Bisby et al., 1987, 1988, 1994; Weidhacht et al., 2001). For these studies, we treated main effects by comparing the interindividual condition with the intergroup condition in which procedural interdependence was absent. We followed this procedure because, relative to the number of studies in which procedural interdependence was present, there was only a small number of studies in which procedural interdependence was absent. We considered it most important to increase the small number of studies in which procedural interdependence was absent than to increase the already large number of studies in which procedural interdependence was present.

7 The coding of this variable may need some clarification. We coded those samples that consisted exclusively of male participants as all males and those samples that consisted exclusively of female participants as all females. For some studies, we did not have sufficient information to compute separate effect sizes for male and female participants, but we did have information indicating that females always interacted with other females and that males always interacted with other males. These samples thus consisted of male and female participants in interactions that were homogenous with respect to sex. Finally, some studies did not treat sex in any systematic way. In those studies, males interacted with females, and groups could consist of both male and female members. These samples thus consisted of male and female participants in interactions that were heterogeneous with respect to sex.

8 Bisby et al. (2001) recently demonstrated the role of anticipated future interaction in interindividual-intergroup discontinuity. These results may therefore seem surprising in that the current study does not include a variable indicating whether participants anticipate a single trial or multiple trials. There are two reasons for not including this variable. First, Bisby et al. (2001) demonstrated that the anticipated number of trials affected the discontinuity effect—through a reduction in intergroup competition—only when groups interacted for single trials rather than for multiple trials. In this meta-analysis, participants' capacity to reason abstractly (i.e., project their outcomes in the future) was the moderating effect of anticipated number of trials. We include this in this meta-analysis, participants' capacity to reason abstractly was assessed only by Bisby et al. (2001). This rules out the possibility of examining the effect of anticipated number of trials in this study, and the possibility of participants anticipating multi-trial interaction accordingly. Bisby et al. (2001) showed that multi-trial interaction was especially important in this meta-analysis, participants' capacity to reason abstractly was assessed only by Bisby et al. (2001). Second, most studies in which participants anticipated interacting on multiple trials actually used multi-trial trials. This confounding of trials anticipated and trials played is problematic because, contrary to a single-trial interaction, a multi-trial interaction creates the opportunity for escalation of conflict.
1. We assumed in Eq. (30) a 174 independent effect sizes by using sample sizes, mean proportions of competition, and standard deviations reported separately for the interindividual and intragroup conditions. These proportions of variance were calculated by dividing the mean number of competitive choices by the number of choices per participant. Consider it experienced that component intragroup and interindividual variances in the context of a 174 intergroup effect size. Assuming that both intergroup of each participant's whose strategies are uncorrelated, the total number of choices per participant is 20 (2 choices per each side). The mean proportions of variance in any given condition can then be calculated by dividing the mean number of competitive choices in that condition by 20. When participants were instructed by a programmed opponent, calculations were based exclusively on the participants' choices. 

2. For 16 effect sizes, sample sizes that means were reported separately for the interindividual and intergroup conditions, but no standard devia-
tions were available. In these cases we computed the pooled standard deviations by extracting a mean square error (MSE) from an analysis of variance (ANOVA) that included a contrast between interindividual and intragroup variances. In some of these cases too, ANOVA results were reported in verbal detail to allow us to adjust the variances to correct for any heterogeneity among the studies. 

3. When $F < 1$, i.e., cross-situational variance was reported for a comparison of interindividual and intragroup variance, we computed this gap as a (174 effect sizes). For studies in which the dependent variable was dichotomous (i.e., studies in which participants responded in only one trial with a programmed when effect sizes were based on odd even trials, and standard deviations could be found. When we used mean and standard deviation to compute effect sizes for these studies, these effect sizes were within missing error of those based on odd even trials. 

Random-effects model. We used a random-effects meta-analytic ap-
proach. The random-effects model was appropriate because we wanted
to make unconditional inferences that generalize to a hypothetical popu-
lation of studies that exist rather than to the population of studies of
dependent variables described in Hand-hedges & Vevea, 1998). Random-effects models differ from fixed-effect
effects model in that they incorporate both random variation associated with the sampling of persons or other units into studies and an additional component of variability associated with the sampling of the studies them-
selfs from a larger population of studies. The variance of this second type of vari-
ation is referred to as within-study variability. Each effect size is considered
dependent on the other. This conditional variance reflects uncer-
tainty that arises from the sampling of observations within that specific
data point and is in principle subject to the within-study sample size. The
type of variance is known as within-study variability. Each effect size is considered
dependent on the other. This conditional variance reflects uncer-
tainty that arises from the sampling of observations within that specific
data point and is in principle subject to the within-study sample size. The
second type of variance is represented by a model parameter describing variability among effect sizes that exceeds what one would expect given the variability in the effect sizes. This cross-situational variance, called the between-studies variance component, describes the variation of the population of hypothetical studies that do, or could, exist. 

Explanatory model. As we described above we identified
eight variables that are expected to predict variance in effect sizes (i.e., sex of participant, sex of other participant, strategies, and nonresponse errors). We implemented a model that predicts effect size as an analysis of variance-like inon of these variables. This exploratory model was estimated using a random-effect correc-
tion, resulting in what is often called a mixed-effects model. In mixed-
effects models, some of the observed variability in effect sizes is associated with the model. As indicated in the model, some is associated with the sampling of observations into individual studies, and the rest is thought of as associated with the sampling of studies and represented by a variance component. We used the method of unconditional maximum likelihood to estimate models' parameters. Before testing these main-effects models, we confirmed that all variance components were statistically significant. 

Planned contrasts. In this study, opposition strategy and communi-
cation mode were studied at two levels. We decided to explore potentially significant associations of these moderators with the
discontinuity effect by means of planned contrasts. We assigned specific
tests to test certain specific predictions regarding the association of opponent strategy and communication with the magnitude of the
discontinuity effect. Recall that the opponent strategy variable has four levels: uncorrelated, mixed-strategies, concurrent strategies, and Copenhagen strategies. We first examined a contrast comparing studies using opponent strategies versus studies using uncorrelated, concurrent, or strategic strategies. We selected this contrast to test an hypothesis that, relative to uncorrelated and concurrent strategies, mixed-strategies strategies are associated with a re-
duction in the discontinuity effect. Second, we extended a contrast comp-
ing mixed-strategies strategies versus concurrent strategies. It is clear that, relative to the uncorrelated strategies, concurrent strategies reduce free-
talk and thus remove the bias for the discontinuity effect. It is less clear whether cooperative strategies reduce or exacerbate the social contract bias directly on this issue. Because the strategy variable has four levels, it can be represented by three orthogonal contrasts. The only contrast that is orthogonal to the above described contrasts compares studies using mixed-strategies versus studies using uncorrelated, concurrent, or strategic strategies. Because the mixed-strategies category captures a set of heterogeneous strate-
gies, it would be wise to interpret any differences, or lack thereof, between this category and the other strategy categories. The mixed contrast was included in the analyses, however, to represent accurately the four-
level strategy variable (Cohen & Cohen, 1983). 

The communication variable has three levels: uncorrelated communi-
thation, concurrent communication, and no communication. We first examined a contrast comparing studies with no communication versus studies with uncorrelated communication or concurrent coopera-
tive communication. We selected this contrast to test the hypothesis that the discontinuity effect is larger when there is some form of communication between sides than with no communication. This hypothesis is based on the idea that, because the two groups share sensitive issues of other groups, groups do not strive as much benefit from communication as other individuals. The elimination of communication therefore reduces competition between individuals to a greater extent than competition between groups. Furthermore, even when the communication of cooperative
time is perceived as credible, groups may be more likely to allocate larger proportions of resources because cooperation provides anonymity and social support. Second, we examined a contrast comparing studies with uncorrelated communication versus studies with concurrent cooperative communication. Whenever there is a difference between sides with uncorrelated communication and studies with co-
strated cooperation communication is an appropriate approach. On the one hand, it is possible that constrained cooperative communication and uncon-
strained communication function in institutional ways. If this is the case, there should be no significant difference between the two sets of studies. On the other hand, it is possible that constrained communication in the form of somewhat asymmetric within-studies or telephone communication is less successful in creating trust between individuals than uncorrelated commu-
nication. If this is the case, the magnitude of the discontinuity effect should be smaller for studies with constrained cooperation communication than for studies with uncorrelated communication. 

Sensitivity analysis. As in the meta-analysis we calculated a number of re-
jections. For instance, we focused on certain moderating factors for theoretical reasons and, in so doing, assumed implicitly that others were less impor-
tant. We also assumed that the studies we identified truly represented an unbiased sample of studies that have been conducted. To investigate the possible consequences of these assumptions being wrong, we performed a series of sensitivity analyses. 

First, we conducted additional analyses in which we added other poten-
tial moderators at a time. In so doing, we were examining, first, whether other significant moderators of the discontinuity effect would
emerge and, second, whether findings for a priori models would change substantially when additional moderators were included.

Next, we examined the possibility that the publication process biased our results. We used a novel approach to investigate publication bias. Specifically, we applied a modification of a model that corrects for publication bias by estimating a weight function representing the relative likelihood of studies with p values in a particular range surviving the publication selection process (Verver & Hayes, 1993). The model proposed by Verver and Hayes has proven to be effective for large data sets.

However, the weights that represent the publication selection process are difficult to estimate, so that the model is impossible to apply consistently to smaller meta-analysis data sets like the current one. Accordingly, we used a new approach based on the weights are fixed at user-specified values, and the adjusted estimates are assessed conditionally on this fixed weight function (Verver & Wood, 2002). In this approach, sets of weights representing varying degrees of selection bias can be applied, and the impact of the parameter estimates assessed. If so, the results are not due to publication bias.

Results
Random-Effects Analysis
Overall model
After we deleted cases with incomplete data, 130 effects remained in the analysis (four studies lacked information about the index of noncorrespondence). Recall that our primary planned analysis examined the effects of opponent strategy, procedural interdependence, communication, and noncorrespondence of outcomes in the context of a random-effects model. This analysis revealed that all four moderators shared a unique and statistically significant association with the magnitude of the discontinuity effect: x2(3, N = 130) = 24.15, p < .01, for opponent strategy; x2(1, N = 130) = 5.91, p < .02, for procedural interdependence; x2(2, N = 130) = 23.98, p < .01, for communication; and x2(1, N = 130) = 16.27, p < .01, for noncorrespondence of outcomes.1 Table 1 shows parameter estimates for the random-effects model, broken down into the specific contrasts that are described in the Method section.

Opponent strategy. We explored the significant association of opponent strategy with the magnitude of the discontinuity effect by means of planned contrasts. The first contrast compared rectrival strategies versus the pooled unconnected and cooperative strategies. The significant negative coefficient for this contrast indicates that, as predicted, the discontinuity effect was smaller in studies using cooperative strategies than in studies using unconnected or cooperative strategies.

The second contrast compared the unconditional strategy versus the cooperative strategy. This contrast was coded such that a negative coefficient indicates that the discontinuity effect was smaller in studies with a cooperative strategy than in studies with an unconditioned strategy. However, the coefficient for this contrast was positive and near zero, indicating that the two sets of studies did not differ significantly.

Procedural interdependence. The positive coefficient associated with the procedural interdependence variable indicates that, as predicted, the discontinuity effect was larger in studies in which group members were procedurally interdependent than in studies in which group members were not procedurally interdependent.

Communication. We explored the significant association of the communication variable with the magnitude of the discontinuity effect by means of planned contrasts. The first contrast compared studies with no communication versus the pooled studies with unconditioned communication and constrained cooperative communication. We selected this contrast to test the hypothesis that the discontinuity effect is stronger when there is some form of communication between the interacting sides than when there is no communication. The contrast was coded such that a positive coefficient indicates larger effects for studies with some form of communication. Although the coefficient for this contrast was in the predicted direction, it was not significant.

The second contrast compared studies with constrained cooperative communication versus studies with unconstrained communication. The significant negative coefficient for this contrast indicates that the discontinuity effect was smaller in studies with constrained cooperative communication than in studies with unconstrained communication. This result is consistent with the idea that constrained cooperative communication by means of hand-written notes or telephone conversations is not as successful in creating trust between individuals as is unconstrained communication.

When we consider simultaneously the results for both contrasts, it appears that our initial expectation that studies with communication are associated with larger effects than studies without communication was not completely wrong, as in the results for the first contrast may suggest. Results for the second contrast indicate that we need to differentiate clearly between constrained cooperative communication and unconstrained communication. Consistent with our initial expectation regarding the role of communication, studies without communication were associated with the smallest effect sizes and studies with unconstrained communication were associated with the largest effect sizes. A post hoc test using a Bonferroni-adjusted 0.055 threshold indicated that the hypothesis that the effect sizes of studies did, in fact, differ significantly (p = 0.27; z = 4.01, p < .01). Studies with constrained cooperative communication were associated. The effect size of studies such as magnitude. The second contrast strongly indicated that studies with constrained cooperative cooperation differed significantly from studies with unconstrained communication, but did studies with constrained cooperative cooperation also differ significantly from studies with no communication? A post hoc test comparing these differences was not significant.

Note: These chi-square statistics are more commonly referred to as p-values, and the index of noncorrespondence is computed as a between-groups test that is more often used in other contexts. We refer to these statistics as chi-squares rather than Q-index statistics because our analysis of the discontinuity effect is a between-groups test that is more often used in other contexts. The formula for the chi-square statistic is:

x2(1, N = 130) = 16.27, p < .01, for noncorrespondence of outcomes.1.
Table 1: Parameter Estimates for Random-Effects Coefficients

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Parameter estimate</th>
<th>SE</th>
<th>z</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>-0.358</td>
<td>0.195</td>
<td>-1.84</td>
<td>.06</td>
</tr>
<tr>
<td>Strategy</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reciprocals vs. cooperative and unconstrained</td>
<td>-0.652</td>
<td>0.147</td>
<td>-4.44</td>
<td>.001</td>
</tr>
<tr>
<td>Cooperative vs. unconstrained</td>
<td>0.013</td>
<td>0.100</td>
<td>0.13</td>
<td>.882</td>
</tr>
<tr>
<td>Miscellaneous vs. other categories</td>
<td>-0.167</td>
<td>0.194</td>
<td>-0.86</td>
<td>.388</td>
</tr>
<tr>
<td>Procedural interdependence</td>
<td>0.280</td>
<td>0.115</td>
<td>2.47</td>
<td>.015</td>
</tr>
<tr>
<td>Constraint agreeable vs. cooperative</td>
<td>-0.113</td>
<td>0.092</td>
<td>1.23</td>
<td>.219</td>
</tr>
<tr>
<td>Cooperative vs. unconstrained</td>
<td>-0.369</td>
<td>0.061</td>
<td>-5.93</td>
<td>.001</td>
</tr>
<tr>
<td>Index of noncooperativeness</td>
<td>-0.727</td>
<td>0.229</td>
<td>-3.20</td>
<td>.001</td>
</tr>
</tbody>
</table>

Two sets of studies indicated that they did not (b = -0.10; z = -1.03, p < .31).

Noncorrespondence of outcomes. The negative coefficient for the index of noncorrespondence variable indicates that, as predicted, decreases in noncorrespondence of outcomes (i.e., less negative index values) were associated with reductions in the magnitude of the discontinuity effect. An issue that we encountered in this particular analysis relates to the existence of two effect size estimates that were potential outliers with respect to the index of noncorrespondence. Whereas all other effect sizes came from studies that used matrices with indices of noncorrespondence that were zero or less, two effect sizes came from a study that used a matrix with a positive index of .65 (Insko et al., 1992, Experiment 1). These observations thus exert a good deal of influence on the slope associated with the index of correspondence. To investigate how influence we repeated our analysis after deleting these two observations. This analysis revealed that the effect for noncorrespondence of outcomes remained significant after the two observations with positive index values were deleted (b = -0.68; z = -2.28, p < .05).

Variance component. The estimated between-studies variance component was .077. We conducted a Q-test of the null hypothesis that the true variance component is zero. Under the null hypothesis, the Q-statistic has a chi-square distribution with 122 degrees of freedom. The variance component differed significantly from zero, Q(122) = 235.40, p < .01. To interpret the variance component, consider its square root, 0.277. This value represents an estimate of the standard deviation of the distribution of true population effects, after the variation associated with the four moderators (i.e., strategy, procedural interdependence, communication, and noncorrespondence of outcomes) is accounted for. Although the variance component differed significantly from zero, its magnitude was not particularly large. The results indicate that the explanatory model captures most of the variation in effect sizes. The variance component represents small residual variation in effect sizes associated with unobserved differences among the populations studied.14

Effect Size Estimates

How do these results translate into effect size estimates? Table 2 contains predicted mean effect sizes for combinations of opponent strategy, procedural independence, and communication that were represented by five or more effects. Within each of these popullated cells, predicted mean effect sizes were calculated for two values of the index of noncorrespondence variable: -30 and -60. We chose these values because they are most representative of the index of noncorrespondence values observed across studies. The standard errors are exactly analogous to standard errors for predicted values in a regression analysis. The hypothesis is that any of the predicted mean effect sizes differs from zero in the population can be tested by a simple z statistic, which is obtained by dividing the predicted mean effect size by its standard error. Alternatively, 95% confidence intervals can be obtained by taking the effect size estimate plus or minus 1.96 standard errors. Table 2 shows that interindividual and intergroup interactions were most frequently compared under conditions conducive to the discontinuity effect—that is, when (b) the opponent's strategy was unconstrained by the experimenter, (b) procedural interdependence among group members was present, (d) p-up members acted collectively, and (d) communication between sides was unconstrained. As expected, under these conditions the discontinuity effect was statistically significant and descriptively large, especially when the index of noncorrespondence was strongly negative. Table 2 further indicates that the combined linear effects of the four moderators can account for a wide range of effect sizes, including descriptive reversals of the discontinuity effect when (a) the opponent's strategy was reciprocal, (b) procedural interdependence...
Table 2: Predicted Mean Effect Sizes and Standard Errors of Prediction for Populated Cells

<table>
<thead>
<tr>
<th>Strategy</th>
<th>Procedural Independence</th>
<th>Communication</th>
<th>No. of effects</th>
<th>Predicted mean effect size (SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unconstrained</td>
<td>Present</td>
<td>Uncorrelated</td>
<td>87</td>
<td>1.80p (0.092)</td>
</tr>
<tr>
<td>Unconstrained</td>
<td>Present</td>
<td>Absent</td>
<td>11</td>
<td>0.75p* (0.125)</td>
</tr>
<tr>
<td>Unconstrained</td>
<td>Absent</td>
<td>Uncorrelated</td>
<td>5</td>
<td>0.75p* (0.446)</td>
</tr>
<tr>
<td>Cooperative</td>
<td>Absent</td>
<td>Absent</td>
<td>2</td>
<td>0.227 (0.166)</td>
</tr>
<tr>
<td>Reciprocal</td>
<td>Present</td>
<td>Absent</td>
<td>6</td>
<td>-0.206 (0.190)</td>
</tr>
</tbody>
</table>

Note. The number of effects listed in the fourth column (N = 116) does not sum to the total number of effects in the meta-analysis (N = 130) because we generated predicted means only for cells populated by five or more effects. INC = index of noncorrespondence. *p < .05. **p < .01.

dence among group members was present, and (c) communication between sides was absent.

We know of no experiments that compared interindividual and intergroup interactions under conditions least conducive to the discontinuity effect—that is, when (a) the opponent's strategy was reciprocally, (b) procedural independence among group members was absent, and (c) communication between sides was absent. Because this cell was unpopulated, we did not include it in Table 2. It is worth noting, however, that our model predicts descriptively large reversals of the discontinuity effect for this cell. When we assume an index of noncorrespondence value of zero, the predicted mean effect size is -1.35 (z = -4.22, p < .01).

Sensitivity Analyses

Other variables. We explored whether we failed to include important mediating variables of the discontinuity effect in our prior model by adding additional variables to that model one at a time. Variables considered in these analyses include year of publication, source of publication, an indicator of whether the study was published, laboratory location, affiliation of primary investigator, source of sample, recruitment method, number of trials anticipated, and sex composition of the sample. Of these variables, only sex composition of the sample was significantly associated with the magnitude of the discontinuity effect when added to the a priori model. χ²(2, N = 130) = 28.81, p < .01. We explored the sex composition effect with three planned contrasts. The first contrast compared studies with male and female participants in heterogeneous interactions versus studies with all male or all female participants. The second contrast compared studies with male and female participants in heterogeneous interactions versus studies with all male or all female participants. The third contrast compared studies with all male participants versus studies with all female participants.

The first (χ² = 0.24, z = 2.61, p < .01) and third contrasts (χ² = 0.20, z = 1.85, p < .01) were significant. The discontinuity effect was larger for studies with a heterogeneous sex composition than for studies with a homogeneous sex composition and larger for male-only studies. Note, however, that sex composition of the sample was confounded with communication. For instance, all studies with constrained cooperative communication were conducted with males and females in heterogeneous interactions. When we added sex composition to the model, the effect of communication became marginal. χ²(2, N = 130) = 5.67, p < .06. This indicates that sex composition is modeling partially the same variability in effect sizes as communication. However, the comparison between constrained communication and no communication remained significant (χ² = 0.17, z = 2.15, p < .05). In light of the fact that we identified communication as a moderator of the discontinuity effect on theoretical grounds, we believe that our prior model is the correct one to interpret.

Publication bias. Publication bias is likely to occur if, either through the editorial process or self-censorship on the part of researchers, effects with small values are more likely to enter the literature than nonsignificant effects or effects with larger p values. When such bias occurs, effect size estimates are likely to be too large. In this respect it is reassuring that we did not find large discontinuity effects in published studies than in unpublished studies. Inspection of the funnel plot of effects included in the meta-analysis suggests, however, that selection bias may be a factor in the present data set. This funnel plot is presented in Figure 3. Effect size is plotted on the vertical axis, and sample size is plotted on the horizontal axis. When selection bias is absent, the plot exhibits vertical symmetry, narrowing as size moves from left to right. When selection bias is present, the plot exhibits sparseness in either the negative or the positive tail of the funnel plot, depending on whether the true effect is positive or negative, respectively. The sparseness results from the fact that smaller studies are more likely to obtain nonsignificant results even when a genuine effect is present and, hence, are less likely to enter the literature. The asymmetry in Figure 3 is clear. When sample size is small, large positive effects are more likely to be observed than when sample size is large, and negative effects are rather sparse even though symmetry in the plot would demand their presence.

Our approach to the issue of publication bias considers hypothetical publication-bias scenarios and assesses the impact of these scenarios on parameter estimates (Vevea & Woods, 2002). If parameter estimates are relatively unaffected even under scenarios representing severe publication bias then we can be confident that publication bias has not unduly influenced our results. We considered two post hoc bias scenarios, using a modification of the

We excluded one very large effect size (z = 9.15) from the plot to allow a clearer portrayal of the pattern of effect sizes. Notably because the combined sample size for this effect was small (N = 8), this effect had little influence on the analysis.
The statistical model presented by Vevea and Hedges (1995). These scenarios, referred to as moderate and severe, are implemented using a set of prior weights that represent the assumed probability of effect sizes and are realized in the meta-analysis. To assess the impact of a given scenario on parameter estimates, the meta-analytic model is reestimated conditionally on the a priori weights. For instance, we might assume that, because of a biased publication process, only half of the effects with $p$ values between .10 and .50 were retrieved. By reestimating the meta-analytic model conditionally on this assumption, we can determine what the parameter estimates would have been if all effects with $p$ values in this particular range had been retrieved.

In the moderate scenario, we assumed that all effects with $p$ values less than .10 were retrieved, while effects with $p$ values between .10 and .25 were retrieved with a probability of .95, effects with $p$ values between .25 and .50 were also retrieved with a probability of .95, effects with $p$ values between .50 and .10 were retrieved with a probability of .90, and effects with $p$ values between .10 and .20 were retrieved with a probability of .80. Effects with $p$ values between .20 and .30 were retrieved with a probability of .75, effects with $p$ values between .30 and .50 were retrieved with a probability of .50, and effects with $p$ values above .50 (i.e., reversals of the discontinuity effect) are retrieved with a probability of .30. In the severe scenario we assumed, for the same $p$ value intervals, probabilities of 1.0, .95, .80, .60, .50, .50, and .10.

Table 3 shows predicted means adjusted for each of the weight functions (i.e., moderate and severe) across the different publication-bias scenarios. Consider, for instance, the situation in which (a) the opponent's strategy is unconstrained by the experimenters; (b) there is no independence among group members; and (c) no communication between sides is unconstrained. In the original analysis, the predicted mean effect sizes for this cell were 1.150 and 1.300 for index values of $-40$ and $-60$, respectively. When we assumed moderate selection bias, these values were 1.044 and 1.184. When we assumed

<table>
<thead>
<tr>
<th>Table 3</th>
<th>Predicted Mean: Effect Sizes for Populated Cells Under Different Publication-Bias Scenarios</th>
</tr>
</thead>
<tbody>
<tr>
<td>Strategy</td>
<td>Procedure's Interdependence</td>
</tr>
<tr>
<td>Unconstrained</td>
<td>Present</td>
</tr>
<tr>
<td>Unconstrained</td>
<td>Present</td>
</tr>
<tr>
<td>Unconstrained</td>
<td>Absent</td>
</tr>
<tr>
<td>Unconstrained</td>
<td>Absent</td>
</tr>
<tr>
<td>Reciprocally</td>
<td>Present</td>
</tr>
</tbody>
</table>

Note: INC = Index of noncomparability.
severe selection bias, the values were 0.982 and 1.140. This thus appears highly unlikely that the results that are of greatest theo-
retical interest to us due to a biased publication process.

Supplementary Analyses: Toward a Detailed Description of Discontinuity-Mediator Associations

The analyses that model variation in the magnitude of the discontinuity effect as a function of the four prior moderators do not clarify whether these moderators relate to interindividual com-
petition, intergroup competition, or both. For instance, our finding that the discontinuity effect was larger with than without con-
munication does not clarify whether this result occurred because communication reduced interindividual competition, increased in-
tergroup competition, or both. To address this remaining issue, we conducted separate meta-analyses of interindividual and inter-
group competition, modeling each as a function of relevant mod-
erators. We were able to retrieve information regarding the pro-
portion of interindividual and intergroup competition from all but one study in the meta-analysis. We conducted mixed-effects analyses to model variation across studies in interindividual and intergroup competition as a function of opponent strategy, com-
munication, and index of unison/resonance. Procedural inter-
dependence was not included in the model of interindividual competition because, by definition, it relates only to intergroup interactions. For the two meta-analytic models to be equivalent, we also excluded procedural interdependence from the model of in-
tergroup competition.

As we emphasized earlier, meta-analysis is a correlational pro-
cess, and explanatory models in meta-analysis must therefore be interpreted with caution. We must heed this warning in particular when examining findings from the separate meta-analyses of in-
terindividual and intergroup competition. First, whereas the ex-
planatory model that was brought to bear on the discontinuity effect resulted from an attempt to identify on theoretical grounds all important moderators of this effect, we did not attempt to identify all important moderators of interindividual and intergroup competition. The selection of moderators was dictated by a con-
cern with providing a detailed description of the identified discontinuity-mediator associations rather than a concern with specifying optimal explanatory models for interindividual and intergroup competition. Second, whereas the meta-analysis of the discontinuity effect was preceded by an exhaustive literature search, we did not attempt to retrieve all studies that examined interindividual or intergroup interactions in the context of mixed-mo-
tive situations. Again, we are concerned with exploring the discontinuity-mediator associations in our present data set rather than with reviewing the complete literature on interindividual and intergroup competition. We proceeded with the analyses bearing in mind that, in light of these limitations, they are suggestive rather than conclusive.

Opponent strategy. There was no significant association of opponent strategy with either interindividual competition, $r^2(3, N = 129) = 4.61, p < .05$, or with intergroup competition, $r^2(3, N = 129) = 5.61, p < .05$. The planned contrast of studies with reciprocal strategies versus those with cooperative and uncon-
trained strategies indicated that intergroup interactions were mor-
gently less cooperative when the opponent followed a reciprocal strategy than when the opponent’s strategy was constrained or cooperative ($b = -0.055, z = -1.67, p = .095$). This pattern was re
duced for interindividual interactions ($b = 0.04, z = 2.05, p < .05$). The contrast between studies with cooperative strategies and those with unconstrained strategies was not significant for either interindividual interactions ($b = -0.01, z = -0.49$, for inter-
group interactions ($b = -0.055, z = -1.29$). At a descriptive level, these results resemble more carefully controlled experimental findings by Insko et al. (1998). Insko et al. (1998) manipulated whether individuals and groups interacted with an opponent whose strategy was unconstrained, an opponent who followed a tit-for-tat strategy, or an opponent who followed a Pavlov strategy. Results indicated a significant Discontinuity X Opponent Strategy interaction. For our present purposes, the rele-
vant contrast is between the tit-for-tat condition and the uncon-
trained condition. Intergroup interactions were significantly less competitive when the opponent followed a tit-for-tat strategy than when the opponent’s strategy was unconstrained. For interindi-
vidual interactions there was a non-significant reversal of this pattern.

Overall, our meta-analytic findings and the experimental find-
ings of Insko et al. (1998) are in agreement on two counts. First, reciprocal strategies were associated with reductions in the mag-
nitude of the discontinuity effect. Second, the descriptive pattern for reciprocal strategies to reduce intergroup competition and increase interindividual competition. There is less than perfect agreement, however, regarding the pattern of significance for the simple effects of opponent strategy on interindividual and in-
group competition. Whereas Insko et al. (1998) found that the tit-for-tat strategy significantly reduced intergroup competition and did not significantly increase interindividual competition, the present meta-analytic findings indicate that reciprocal strategies were associated with a marginal reduction in intergroup competi-
tion and a significant increase in interindividual competition. It is apparent that further research is needed to clarify whether recip-
rocal strategies attenuate the discontinuity effect through an in-
crease in interindividual competition, a reduction in intergroup competition, or both. Given that the reduction in intergroup com-
petition was marginal in the meta-analysis and significant in Insko et al.’s (1998) experiment, perhaps what is most uncertain is whether reciprocal strategies increase interindividual competition (cf. Oskamp, 1971; Wilson, 1971).

Communication. There was no significant association between communication and interindividual competition, $r^2(2, N = 129) = 74.16, p < .01$. Planned contrasts show, first, that inter-
individual competition was lower when some form of communi-
cation was present rather than absent ($b = -0.14, z = -3.26, p < .01$). Second, interindividual competition was higher when com-
munication was constrained rather than unconstrained ($b = 0.01, z = 3.19, p < .01$). The latter finding is consistent with the possibility that constrained communication is more effective in creating trust between individuals than is constrained communica-
tion (cf. Wichman, 1972). The association between communica-
tion and intergroup competition was also significant but weaker, $r^2(2, N = 129) = 9.57, p < .01$. The latter contrast was lower when some form of communication was present rather than absent ($b = -0.07, z = -2.92, p < .05$). The contrast between con-
strained and unconstrained communication was not significant ($b = 0.055, z = 1.15$). As predicted, the previously reported discontinuity—communication association reflects that commun-
cation reduced nonindividual competition more than it reduced intergroup competition. These views are in agreement with experimental findings by Insko et al. (1985).

Noncooperation of outcomes. There was a significant associa-
tion between index of noncooperation and intergroup competition, \( \chi^2(1, N = 129) = 28.69, b = -0.39, p < .01 \). The association between index of noncooperation and inter-
group competition was marginal, \( \chi^2(1, N = 129) = 3.52, b = -0.08, p < .07 \). As predicted, the previously reported discontinuity–noncooperation association reflects decreases in noncooperation (i.e., less negative index values) associated with larger reductions in intergroup than in inter-
individual competition. These results are in agreement with experi-
mental findings by Schopler et al. (2000).

Discussion

In his "law of these stages," the French philosopher Comte (1830/1850) proposed that the sciences move gradually from the theological, through the metaphysical, to the positive stage of develop-
ment. Comte's theory provides a formal way of evaluating new and growing approaches to the question as to why individuals' often behave in a hostile and competitive manner when they are banded together in a group. Central to this would be inaccurate to call it Bon and McDougall's ethologists, but we agree with G. W. Allport (1965) that the view of group mind as a mental entity that influences the behavior of group members "necessarily imposes metaphysical blocks in the path of constructive conceptualization" (p. 55). Still, despite the fact that social psy-
chologists (e.g., Lewin, Lipitt, & White, 1939, Sherif, 1956) adopted the tools of positivism—such as experimentation and statistics—shortly before World War II, the "constructive concep
tualization" of the interindividual–intergroup discontinuity prob-
lem was not formulated until Rabbe et al. (1982) and McCalliloth et al. (1985) fleshed the issue in terms of a comparison between interindividual and intergroup behavior in the context of conflict of ingroup. The present meta-analysis provides a quantitative integration of the research on interindividual–intergroup discontinuity conducted over the past 3 decades. Three complementary explanations for the discontinuity effects have been advanced. The social support and identifiability explanations center on the greater guilt in intergroup relative to interindi-
idual relations. We identified four moderators of the discontinuity effects—each based on the theoretical perspective that interindividual–intergroup discontinuity flows from the greater fear and guilt in intergroup relative to interindividual interactions.

These are opponent strategy, procedural independence, commu-
nication, and noncooperation of outcomes. Consistent with the social support, identifiability, and fear explanations of interindividual–intergroup discontinuity, results indicate that each of these four moderators shared a unique association with the magnitude of the discontinuity effect above and beyond the other

moderators. Below, we discuss in detail the results for each of the four moderators.

Opponent Strategy

In 1939, Kurt Lewin wrote prophetically:

To overlook results may seem outrageous to the sophisticated mind, but as a situation like ours, where the very existence of the Jewish people is at stake, we cannot afford the luxury of this gesture. Aside from the moral issues, a man who does not show backbone acts cowardly. He invites the hostility of the mob which is always ready to have its initial fury but is afraid to stick out its neck when it knows that it will be rewarded.

The present findings for the role of opponent strategy in interindividual–intergroup discontinuity bear on Lewin's (1939/ 1997) statement in two ways. First, and foremost, we found that the interindividual–intergroup discontinuity effect was reduced, even eliminated, when individuals and groups interacted with an opponent who followed a reciprocal strategy. We propose that reciprocal strategies, such as cooperation, reduce the discontinuity effect for two reasons. The first reason is that, because reciprocal strategies always respond to cooperation in kind, distrust between groups is reduced. This weakens or removes one basis of the discontinuity effect. The second reason is that, with reciprocal strategies, cooperation always begets cooperation. Indeed, one could say that reciprocal strategies have backbone. These strate-
gies send the clear message that, from the perspective of maxi-
mizing long-term outcomes, cooperation is not adaptive. They thus discourage the competitive pursuit of short-term self-interest, or greed, and reduce the likelihood that group members will be tempted to act on the social support and anonymity afforded by the group context. This weakens or removes another basis of the discontinuity effect. Although we found suggestive evidence that reciprocal strategies reduce intergroup cooperativeness in an absolute sense, a conservative interpretation of our findings suggests that reciprocal strategies reduce intergroup cooperation relative to nonindividual competition.

Our findings also bear on Lewin's (1939/1997) statement in a second, related way. We found that, from the perspective of reducing the discontinuity effect, a cooperative strategy was ine
effective. The magnitude of the discontinuity effect when one of the players followed a cooperative strategy did not differ significantly from the magnitude of the discontinuity effect when the strategy of both players was uncoordinated. We pointed out that when one player adopts a cooperative strategy, this player no longer poses a threat to the opponent. This reduces or removes the basis for the discontinuity effect in the greater fear in intergroup relations as compared with inindividual relations. We were less certain, however, whether a cooperative strategy reduces or rewards greed. One possibility is that a cooperative strategy reduces the discontinuity effect because, with absence of threat, greed cannot be justified through defensive attributions (Insko et al., 1995). Another possibility is that by responding to cooperation with cooperation, or "overcooperating," the cooperative strategy invites greed. Our findings are consistent with the second possibility: Groups were more likely to interact with a cooperative opponent. Because a cooperative opponent does not pose a threat, the greater co
petitiveness of groups likely reflects an attempt to maximize self-interest, or greed.

Procedural Interdependence

Results indicate that the discontinuity effect was significantly larger when group members were procedurally interdependent than when group members were not procedurally interdependent. The role of procedural interdependence as an antecedent to individual-intergroup discontinuity is consistent with the identifiability and fear explanations. Relevant to the fear explanation, the anticipation of interacting with a group of procedurally interdependent persons following a collective action is more likely to activate the negative out-group schema than the anticipation of interacting with a group of persons following individual courses of action. Relevant to the identifiability explanation, procedural interdependence creates a situation in which the collective group decision cannot be traced back to the individual group members. This shield of anonymity enables group members to escape the appearance of selfishness by claiming that their competitive behavior was prompted by other group members.

The present findings for procedural interdependence are limited in at least one important respect. In our sample of studies, the effects of procedural interdependence among in-group members were conditioned with the effects of procedural interdependence among out-group members. That is, in all studies, members of both interacting groups were either procedurally interdependent or not procedurally interdependent. Whereas out-group procedural interdependence is more obviously related to fear in-group procedural interdependence is more obviously related to greed. Unfortunately, the effects of in-group and out-group procedural interdependence could not be assessed independently in the context of our meta-analysis.

In a recent experiment, Insko, Wildschut, and Pinter (2003) manipulated independently procedural interdependence among members of the in-group and the out-group. Participants were assigned to 3-person groups and seated individually in three small rooms that were attached to a larger central room. Participants were told that they would interact with another group of 3 persons located in a nearby laboratory. In reality, no other group was present, and the experimenter controlled all feedback from this alleged other group. Participants were told that they were yoked to a person in the other group and that they would interact with this person in the context of a POG. In-group procedural interdependence was manipulated by instructing participants to use one of two different decision rules. When procedural interdependence was absent, participants were told that they would determine individually whether to compete or cooperate with the out-group member to whom they were yoked. When procedural interdependence was present, participants were told that there would be a group decision and that this decision would be determined following a majority rule. In this condition, participants could not determine individually whether to compete or cooperate with the out-group member to whom they were yoked. Instead, their decision was determined by the majority of the group. Out-group procedural interdependence was manipulated by informing participants that the out-group members to whom they were yoked would either follow the individual decision rule or the majority decision rule described above. Results indicated significant main effects on cooperation for both out-group and in-group procedural interdependence. The interaction was nonsignificant. These findings support the idea that the association between procedural interdependence and the magnitude of the discontinuity effect observed in the context of the meta-analysis can be attributed to both the fear associated with out-group procedural interdependence and the greed associated with in-group procedural interdependence.

Communication

We predicted that the discontinuity effect would be larger in studies with communication than in studies without communication. We based this prediction, first, on the idea that there is greater distrust between groups than between individuals. Given this general distrust between groups, communication of cooperative intent between groups should be less credible and persuasive than communication of cooperative intent between individuals. Second, even when communication of cooperative intent is perceived as credible, the anonymity and social support provided by the group context should make groups more likely than individuals to exploit the cooperative opponent in an attempt to maximize self-interest. One initial expectation that the discontinuity effect would be larger in studies with some form of communication—be it unconstrained or constrained cooperative—than in studies without communication proved to be too uninformative. The planned contrast designed to test this prediction was not significant. We allowed for the possibility, however, that constrained cooperative communication in the form of handwritten notes or telephone conversations is less effective than unconstrained communication in creating trust between individuals. Results of the second planned contrast were consistent with this possibility. The discontinuity effect was significantly smaller in studies with constrained cooperative communication than in studies with unconstrained communication. This finding indicates that it is important to distinguish carefully among different forms of communication. Indeed, a post hoc comparison between studies with unconstrained communication and studies without communication was consistent with the general prediction that the discontinuity effect would be larger in studies with communication than in studies without communication.

From an applied perspective, we wish to point out that our interest in the discontinuity effect stems from a concern with finding ways of reducing intergroup competition rather than in increasing interindividual competition. Thus, the observed reduction in the magnitude of the discontinuity effect in studies without communication may be of little applied value if indeed it is due primarily to an increase in interindividual competition, as both primary research (Insko et al., 1993) and our supplementary analyses suggest. From a theoretical perspective, on the other hand, confirmation of the hypothesis that the discontinuity effect is moderated by communication provides valuable support for the idea that the effect flows from greater distrust and greed in intergroup interactions relative to interindividual interactions.16

16 From a methodological perspective, it is worth noting that substituting constrained cooperative communication for unconstrained communication to increase the number of independent observations comes at a considerable cost. The gains in statistical power associated with increasing the number of independent observations is canceled out by the fact that studies with constrained cooperative communication are associated with significantly smaller effects than studies with unconstrained communication.
Nonreciprocity and Discontinuity of Outcomes

We proposed that the occurrence of interindividual-intergroup discontinuity is required to situations that are characterized by nonreciprocity of outcomes and that the magnitude of the discontinuity effect is linearly related to the degree of nonreciprocity. The meta-analytic results provide good support for these claims. There are at least two plausible explanations for the association of nonreciprocity with the magnitude of the discontinuity effect. One explanation assumes a role for greed and the other assumes a role for fear. With regard to greed, now that the index of nonreciprocity becomes less negative, there is a decreasing advantage associated with the competitive choice relative to the cooperative choice. This reduces that likelihood that group members will be tempted to pursue their short-term self-interest and act on the social support and normativity afforded by the group context.

With regard to fear, note that as the index of nonreciprocity becomes less negative, the difference between the outcomes in the upper left and lower right cells of the matrix increases. This increasing difference makes the no-utagencies of mutual cooperation and the disutility of mutual competition even more apparent. Under these circumstances, the tendency for groups to distrust each other may be reduced because both sides realize that mutual competition is an unattainable prospect for themselves, but for the opponent as well. This is the point that sometime has been made about the role of nuclear deterrence in redlining the chances of war. The studies in our meta-analysis typically did not involve negative outcomes. Nevertheless, we believe that the analogy with the doctrine of mutually assured destruction (Kissinger, 1959; Wohlstetter, 1969) is compelling (Schapker et al., 2001).

From an applied perspective, the results suggest that it may be possible to reduce intergroup competition by reconceptualizing the intergroup interaction in terms of mutual, instead of individual, gain. Such a reconceptualization was referenced to us by Kuskind and Ceasonhank (1987) in refusening and by Fish7 and Ury (1983) in terms of focusing on issues, rather than positions, to identify options for mutual gain. Price and Ury provided the example of the Camp David accord for a demilitarized Sinai Desert under Egyptian sovereignty, which allayed Israeli's concern for security and Egypt's concern for sovereignty (Schapker et al., 2001).

An Alternative Perspective on Discontinuity

In the spirit of critical rationalism (Popper, 1968; Phillips, 1987) proposed that "any position can be supported by positive reasons... but what really counts is how well the position can stand up to vigorous assault" (pp. vii-viii). Our position that the discontinuity effect is rooted in fear and greed was given an opportunity to prove its worth by Rabbie (1998), who advanced a thought-provoking alternative perspective. The main postulates of Rabbit's reciproc- ity hypothesis are (a) that by enlisting in intragroup discussions, group members gain a more rational insight into the structure of the PDG than do individuals and (b) that groups, because of this more rational insight, are more likely than individuals to reciprocate the cooperative or competitive behavior of the opponent in an attempt to maximize long-term outcomes.

We doubt whether the reciprocity hypothesis offers a viable alternative to our current position for two reasons. First, the idea that outgroup discussion creates more rational insight into the structure of the PDG than does private reflection is, to the best of our knowledge, unsubstantiated. Second, the meta-analytic finds contradicts the idea that groups are more likely than individuals to reciprocate the opponent's cooperative behavior. Now, in this regard, that Rabbie (1998) identified interactions with a cooperative opponent as a suitable context for a critical comparison between the rival perspectives.

Thus in their view, groups—bested by short-term self-interest or greed—are less likely than individuals to reciprocate the opponent's cooperative behavior. Now, in this regard, that Rabbie (1998) identified interactions with a cooperative opponent as a suitable context for a critical comparison between the rival perspectives.

In support of his reciprocity hypothesis, Rabbie (1998) tried two studies that used a cooperative strategy but did not find a discontinuity effect—namely, Loderijk and Rabbie (1992) and Rabbie and Leuwerik (1991). How can we account for the results of these studies given the postulated evidence? There are three possible explanations, relating to procedural interdependence, communication, and nonreciprocity of outcomes, respectively. First, these two studies were structured so that there was no procedural interdependence among group members; that is, group members made individual rather than group decisions. The meta-analysis demonstrates that the discontinuity effect is smaller when procedural interdependence was absent rather than present. Second, in the two studies in question, there was no encouraged communication between sides. The meta-analysis shows that the discontinuity effect was smaller when intergroup communication was absent rather than present. Third, Leuwerik and Rabbie (1992) and Rabbie and Leuwerik (1991) used matrices with an index of nonreciprocated equal to -50. Seventy-seven percent of all effect sizes in our meta-analysis, however, were associated with a more negative index of nonreciprocation. The meta-analysis indicates that the discontinuity effect became smaller as the index of nonreciprocation became less negative. We used our explanatory model to predict the predicted effect sizes for studies in which (a) opponents strategy is cooperative, (b) procedural interdependence is absent, (c) communication is absent, and (d) index of nonreciprocation is -50, we obtained a g of 0.009, oj. The absence of discontinuity effect in the studies in question can thus be accounted for by our explanatory model—a model that is based on the theoretical perspective that the discontinuity effect flows from the greater fear and greed in intergroup relative to interindividual interactions.

Discontinuity Without a Laboratory or Matrix

All studies included in this meta-analysis were laboratory experiments contrasting interindividual and intergroup interactions in the context of mixed-strategy matrix games. This raises two inter- related questions. First, is the discontinuity effect confined to laboratory settings? Second, can a matrix representation of outcomes models accurately situations involving conflict of interest?
Pemberton et al. (1996) addressed the first question in two studies with a modified version of the Rochester Interaction Record (RIR; Reis & Wheeler, 1991). In the first study, participants kept a record of their social interactions, over a 1-week period. They were trained to distinguish between five types of interaction: one-on-one interactions, within-group interactions, one-on-group interactions, group-on-group interactions, and one-on-group interactions. These types were distinguished by whether the participant interacted with a group (one-on-group) or whether a group in which the participant was a member interacted with an individual (one-on-one). Participants classified and then rated their interactions. The mean ranked competitiveness of the three types of interaction involving groups (one-on-group, group-on-one, and group-on-group) was significantly higher than the mean ranked competitiveness of the two types of interaction not involving groups (one-on-one and within-group).

The second RIR study used a slightly different procedure. Instead of rating their social interactions, participants categorized each of the five types of interaction as either cooperative or competitive. Results indicated that participants experienced the three types of interaction involving groups (one-on-group, group-on-one, and group-on-group) as more competitive than the two types of interaction not involving groups (one-on-one and within-group). It was noted that social interaction managements involving groups (one-on-group, group-on-one, and group-on-group) was significantly higher than the mean ranked competitiveness of the two types of interaction not involving groups (one-on-one and within-group).

The second RIR study used a slightly different procedure. Instead of rating their social interactions, participants categorized each of the five types of interaction as either competitive or cooperative. Results indicated that participants experienced the three types of interaction involving groups (one-on-group, group-on-one, and group-on-group) as more competitive than the two types of interaction not involving groups (one-on-one and within-group). Note that social interaction managements involving groups (one-on-group, group-on-one, and group-on-group) was significantly higher than the mean ranked competitiveness of the two types of interaction not involving groups (one-on-one and within-group). In this case, the results were obtained after all recorded interactions relating to sports and games were removed from the data set and can therefore be attributed to the tendency for competitive sport-related sports to be between groups rather than between individuals. These studies suggest that the group competitiveness of an individual relative to the group's interactions also occurs in non-competitive contexts.

What about the second question? To examine whether the discontinuity is dependent on the matrix representation of interactions, Brehm and colleagues (2001) devised a matrix diagram to visualize the outcomes in a PDG matrix. Participants made a standard set of outcome figures (e.g., empty plus empty, full plus full, and empty plus full). Then they had to evaluate whether to give half of their figures to another opponent. On each trial, individuals made 4 figures, and groups made 12 figures. A set of figures composed of one figure, a primary set, was worth 10 cents, and a pair of figures composed of different figures, a secondary set, was worth 15 cents. By inquiring participants to select half of their figures to their opponent's equivalent to selecting the cooperative (X/X) or to keep all of their figures (equivalent to selecting the competitive (X/O or O/X) or to keep all of their figures (equivalent to selecting the competitive (X/X) for the PDG matrix were duplicated exactly. For instance, if both groups exchanged half of their figures (XX), each group would have six secondary sets and receive 90 cents. If, on the other hand, both groups kept all of their figures (XX), each group would have six primary sets and receive 60 cents. Finally, if one group gave away half of its figures but retained none of the other group's figures, this group would only have three primary sets worth 30 cents, whereas the other group would have three primary sets and six secondary sets worth a total of 120 cents (XX or XX).

Schoepfer et al. (2001) examined the discontinuity effect under three conditions. The matrix-only condition replicated previous research in the PDG matrix was used to represent the outcomes. In the outcome-only condition, the outcomes associated with the combination of outcome figures were analogous to the outcomes in the PDG matrix for the matrix-only condition. Physically, the matrix outcome condition was similar to the outcome condition, but participants in this condition also received the outcome matrix used in the previous condition. Results indicated that overall discontinuity effect was not qualified by the representation of outcomes. The fact that the presence or absence of a matrix format did not significantly affect participants' behavior demonstrates that "interaction in the context of a matrix box is as meaningful as interaction in the context of a functionally equivalent set of exchange rules" (Dzusko et al., 2001, p. 855).

Directions for Future Research

An unexpected prediction of our explanatory model was that a disproportionately large removal of the discontinuity effect will occur when (a) participants interact with an opponent following a reciprocal strategy, (b) procedural interdependence is absent in the interaction, and (c) interaction is not involving groups. The results that social interaction managements involving groups (one-on-group, group-on-one, and group-on-group) was significantly higher than the mean ranked competitiveness of the two types of interaction not involving groups (one-on-one and within-group). This model suggests that the group competitiveness of an individual relative to the group's interactions also occurs in non-competitive contexts.

A second avenue for future research lies in identifying additional moderators of the discontinuity effect. Sensitivity analyses examined a host of variables that were not included in our prior model. This is not to say, however, that we assessed the potential of publishing groundbreaking variables. For instance, variables that influence perceived gain, possibility, or attractiveness (Campbell, 1958), such as within-group similarity and proximity, could not be examined given the lack of substantive difference between the retrieved studies. Because group entitativity may give rise to group behavior (Wildschut et al., 2002), variables that influence entitativity may be suitable subjects of future research. A third avenue for future research lies in finding new explanations for the discontinuity effect. Given the magnitude of the discontinuity effect, we believe that it is multiply determined. Although the social support, identifiability, and fear explanations have received strong support in past research, it is plausible that other valid explanations of the discontinuity effect exist. Wildschut et al. (2002), for instance, raised the possibility that the greater greed in a group relative to individual differences stemmed in part from an in-group favoring norm, that is, a norm dictating that one should take into account the interests of the in-group before taking into account the interests of the other group (cf. Fiske & Rholes, 1982; Tajfel, 1970). Early reference to this idea can be
Morgan and Tindale (2002) interpreted these systematic would influence patterns in terms of shared task representations (Tindale, Smith, Thomas, Fikkin, & Sheffley, 1986), which they defined as “any task-unspecific relevance concept, norm, perspective, prosing goal, or strategy that is shared by most or all of the group members” (p. 49; cf. Laughlin & Ellis, 1986). They proposed that when, during group discussion, arguments are stated that are consistent with a shared task evaluation, even majority members can be influenced to change their initial position. We find this explanation plausible and suspect that the postulated in-group favoring norm is central to group members’ shared task representation when conflict of interest with an out-group is encountered. As a final note regarding the in-group favoring norm, we suggest that it may account for the effect of in-group procedural interdependence to competition documented by Insko et al. (2003) if, first, it can be assumed that in-group procedural interdependen-

dence implies a sense of groupers or entitativity and, second, the resultant entitativity creates normative pressure to benefit the in-group and act competitively.

A fourth avenue for future research lies in identifying ways of reducing or eliminating the disincentivity effect by promoting intergroup cooperation. In interesting possibility for promoting intergroup cooperation relates to recategorization (Gaertner, Dovidio, Anastasio, Bachman, & Rust, 1992; Gaertner, Mann, Murrell, & Dovidio, 1989). Recategorization refers to the transformation of group members’ cognitive representation of the in-group interaction as one involving two separate groups to one involving a single, common in-group. Research has demonstrated that recategorization can produce a reduction in evaluative inter-group bias by increasing liking for former out-group members (e.g., Gaertner et al., 1989). To our knowledge, however, we have yet demonstrated that recategorization can successfully increase intergroup cooperation in situations in which there is some degree of conflict of interest.

In a pilot experiment described by Insko et al. (1998), partic-

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t coupe members were initially categorized into three-person groups. These groups were subsequently combined into two competing six-person groups or, in a different version, two independent six-person groups, each working cooperatively on a task. Consistent with previous research, the intergroup cooperation procedure produced increases in liking for former out-group members. However, when participants were returned to their initial two-person groups, there was not a significant change in liking for former out-group members. However, when participants were returned to their initial two-person groups, there was not a significant change in liking for former out-group members. However, when participants were returned to their initial two-person groups, there was not a significant change in liking for former out-group members. However, when participants were returned to their initial two-person groups, there was not a significant change in liking for former out-group members.

Why does recategorization increase liking but not cooperation in the context of mixed-motive situations? Recent findings by Insko et al. (2001) may shed some light on this issue. They found a significant relation between group members’ cognitive representation of the intergroup interaction and trust, such that those group members who expected the other group to cooperate were less inclined to perceive the interaction as involving separate groups. The relation between trust or expected cooperation and categorization, although ambiguous, is a common theme, suggest-

ing the interesting possibility that recategorization can reduce or eliminate one or more of the disincentivity effect: the greater distance in in-group relative to interpersonal interactions. But what about greed? Analysis of the strategy variable showed that although cooperative strategies reduced or eliminated fear, these
strategies did not reduce the magnitude of the discriminative effect relative to uninoculated strategies. Apparently, decreases in fear do not always go hand in hand with decreases in greed. Paradoxically, this suggests that although reactivation/ligation procedures may increase trust between groups, this trust (i.e., the expectation that the opponent will cooperate) may turn into fear. The idea that if it is necessary to instill some degree of fear to curtail the opponent’s greed is, of course, at the core of the at–for–one strategy and the doctrine of mutually assured destruction. We believe that the ultimate effectiveness of any intervention lies in its ability to promote intergroup cooperation between intact groups (Isik et al., 1998). Although we are uncertain whether reactivation alone will prove to be effective in this respect, it is possible that an intervention that blends reactivation with procedures aimed at reducing greed (e.g., identifiability) would be successful.

Conclusion

Under circumstances that are most conducive to its appearance, the interindividual–intergroup discontinuity effect is a descriptively large phenomenon. We believe, however, that the most important reason for the enduring interest in the differences between interindividual and intergroup behavior does not reside in its sheer magnitude but in its relevance to human survival. Social psychology in particular finds its origins in conditions of social upheaval that necessitated new, creative solutions. Referring to the possibility of improving social relationships through social science, G. W. Allport (1968) wrote:

For the past century this optimistic outlook has persisted even in the face of sober accomplishments to date. Human relations seem ambivalent. Wars have not been abolished; labor troubles have not abated; and racial tensions are still with us. (p. 3)

Unfortunately little has changed over the past 35 years. It is estimated that just in the final decade of the 20th century, the deadly wars of places like Rwanda, Bosnia, and Ethiopia claimed the lives of 30 million people and made refugees of another 45 million (McGhee, 1998). Still, even though human relations may be as "unchangingly gay" as ever, this meta-analysis underscores that great strides have been made toward understanding why intergroup relations are often more amenable and competitive than interindividual relationships. We remain hopeful that these and other advances will some day lay the groundwork for effective interventions aimed at promoting intergroup cooperation.

References

References marked with an asterisk indicate studies included in the meta-analysis.


The APA Publications and Communications (P&C) Board has opened nominations for the editorship of Rehabilitation Psychology for the years 2006–2011. Bruce Caplen, PhD, is the incumbent editor.

Candidates should be members of APA and should be available to start receiving manuscripts in early 2005 to prepare for issues published in 2006. 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.

Rehabilitation Psychology will transition from a division publication to an “all APA” journal in 2006, and the successful candidate will be involved in making suggestions to the P&C Board and APA Journals staff about the transition process.

Gary R. VandenBos, PhD, and Mark Appelbaum, PhD, have been appointed as cochairs for this search.

To nominate candidates, prepare a statement of one page or less in support of each candidate.

Address all nominations to

Rehabilitation Psychology Search Committee
Karan Sellman, Search Liaison
Room 2004
American Psychological Association
750 First Street, NE
Washington, DC 20002-4242

The first review of nominations will begin December 8, 2003. The deadline for accepting nominations is December 15, 2003.