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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 4 moderators 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) conflict of interest is severe 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 banded 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, 1968). Plato's (trans. 1891) distrust 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 dismissive of democracy, they shared Plato'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 Socrates, every Athenian assembly would still have been a mob" (Publius, 1788/1948, p. 248).[1]

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 foremost in mind 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*. Succinctly 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 he faced the situation as an isolated individual" (p. 21).

Floyd Allport (1924) is well known among social scientists for his critique of the crowd-mind concept. Nevertheless, in his later writing, F. Allport (1962) referred to the relation between the individual and the collective as the "master problem of social psychology" (p. 7). Quoting F. Allport (1962) directly,

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Sadly, John Schopler died on April 30th, 2001.

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[1] This quote is often attributed to Alexander Hamilton. Indeed, G. W. Allport (1968) 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 Republican and Democratic camps over the course of the contested U.S. Presidential Election of 2000 illustrates that his concern was not completely unfounded.

> One can, of course, discount as illogical the familiar argument that because individuals behave in a crowd as they would not behave alone, the crowd is therefore a mental entity that "embraces" or "descends upon" its members. But even if we got 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 issue that was conceptualized inadequately (Insko & Schopler, 1998). Isolated individuals cannot engage in the kind of hostile behavior that concerned Le Bon, McDougall, and others. Hostile, or competitive, 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 (Triplett, 1897; Zajonc, 1965), individual versus group problem solving (Shaw, 1932), social loafing (Latané, Williams, & Harkins, 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 competitive than individuals then the comparison 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 *inter*individual interactions and *inter*group interactions. Unlike many of his contemporaries, McDougall (1920) must have recognized this when he proposed that the group's influence on its members is strongest 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 the outcomes of the column player, for example, decrease in rank order across cells from the upper right (*YX*) to upper left (*XX*) to lower right (*YY*) to lower left (*XY*). Note further that the average outcome in the lower left (*XY*) and upper right (*YX*) cells is lower than the outcome in the upper left cell (*XX*). 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 *X* selections. As pointed out by Ridley (1996), "broadly speaking any situation 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).

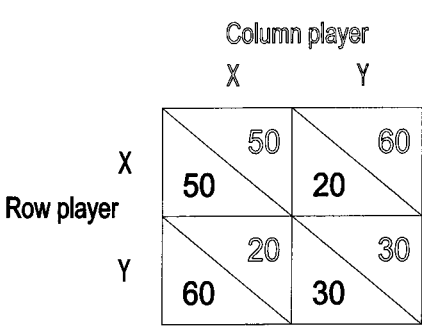

Figure 1. A prisoner's dilemma game matrix.

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 2×2 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 minimize 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 rooms that are connected to a central room. After examining a version of the matrix provided for a given trial, individuals or group representatives meet in the central room to discuss possible action with their opponent and then return to their homerooms 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, 1988, 1992, 1993, 1994, 1998, 2001; Insko, Schopler, Hoyle, Dardis, & Graetz, 1990; McCallum et al., 1985; Schopler, Insko, Graetz, Drigotas, & Smith, 1991; Schopler et al., 1993, 1994, 1995, 2001). John Thibaut labeled this phenomenon a *discontinuity effect*, impressed as he was by Roger Brown's (1954) statement that "the quality of mob behavior has always required explanation because of its apparent discontinuity with the private characters of the individuals involved" (p. 843).[2]

[2] Anonymous reviewers have occasionally questioned our use of the term *discontinuity* on the basis that there may be a continuous increase in competitiveness as aggregates of individuals increase in size. We have unpublished data (Thibaut & Insko, 1985), however, indicating that competitiveness 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 thereafter. In that study at least, there indeed appears to be a discontinuity between interindividual and intergroup interactions.

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 discontinuity 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 definite 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 Garret 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.[3] Because the profits of having a large herd accrued exclusively to the individual herdsman, whereas the costs in terms of exceeding the carrying capacity of the common were incurred collectively by all herdsmen, each herdsman 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 "puny and stunted" (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 concern with over-whaling and noted that 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 fur seals. 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: cut-throat 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 (Samuelson, 1990), provision of public services (Biel, Eek, & Garling, 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 (*Y*) in this situation? One possible reason for selecting the competitive alternative is the self-interest, or 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 vulnerable. 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 relations. Two of these explanations center on the greater greed in intergroup compared with 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 (Insko et al., 1990; Schopler et al., 1993; Wildschut, Insko, & Gaertner, 2002). Matching cooperation with competition is inconsistent with norms of fairness and reciprocity. Social support in the shape of a competitive suggestion 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 and other conformity pressures (Allen & Levine, 1969, 1971; Asch, 1955; Kiesler, Zanna, & De Salvo, 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, deceitful, and competitive (Insko & Schopler, 1998; Pemberton, Insko, & Schopler, 1996; Wildschut, Insko, & 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 postulating the existence of general distrust of out-groups, we are not articulating a new idea. Campbell (1967) previously proposed that "if most or

[3] The emphasis on unmanaged commons is important because, as Ridley (1996) pointed out, most medieval English commons were "carefully regulated communal property" (p. 232). Rights to utilize the common were privately held, and rules were implemented to avoid overexploitation by any one person (cf. Hardin, 1998).

all groups are in fact ethnocentric, then it becomes an 'accurate' stereotype to accuse an outgroup of some aspects of ethnocentrism" (p. 823). Campbell (1967) went on to describe a "universal" out-group stereotype "of which each ingroup might accuse each outgroup, or some outgroup, or the average outgroup" (p. 823). From the perspective of the observer, this universal stereotype of the out-group includes dishonesty, 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 identify the boundary conditions of the discontinuity effect, primary research has examined the difference between interindividual and intergroup 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 literature by abstracting from a large number of studies general principles that govern interindividual–intergroup discontinuity and, in so doing, to aid in theory development. Second, our current theoretical perspective proposes that the discontinuity effect is rooted in the greater fear and greed in intergroup relative to interindividual 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 accessible 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 combination 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 association 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 a priori model changed when other variables were included. The details of these procedures 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 experiments, 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 interactions 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 cooperative strategies and reciprocal strategies, 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 became well known for winning Axelrod's (1984) computer tournaments. These tournaments involved repeated rounds of play between 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" (Insko et al., 1998, 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 safe targets at certain times of the day. Axelrod (1984) suggested that the troops' behavior was caused by the permanence of the front 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" (Axelrod, 1984, p. 21). 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 maximizing outcomes across a number of trials. Thus, from the standpoint of maximizing long-term outcomes, both players should be motivated to achieve mutual cooperation and avoid mutual competition (Insko et al., 1998).

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.18). 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 cooperative 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 followed 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 competition. 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. Furthermore, 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 "eminently 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 strategy. Several discontinuity experiments have contrasted interindividual and intergroup behavior in the context of interactions with a cooperative opponent. It is clear that an opponent who cooperates consistently should reduce or eliminate distrust. This removes one basis for the discontinuity effect: the greater fear in intergroup compared with interindividual interactions. 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 toward 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 competition, with a cooperative strategy competition begets cooperation. Because the cooperative strategy does not punish short-term greed in the same way as reciprocal strategies do, it is vulnerable to exploitation. Shure, Meeker, and Hansford (1965), for instance, observed that in a bargaining context "the pacifist's tactics apparently invite exploitation and aggression, even among those who do not begin with such intentions" (p. 116). From the perspective of discontinuity reduction, reciprocal strategies should therefore be more effective 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 unending mutual recriminations, then a generous level of forgiveness is appropriate. But, if the main danger is from strategies that are good at exploiting easygoing rules, then an excess of forgiveness is costly" (p. 120; cf. Bendor, Kramer, & Stout, 1991; Kollock, 1993; Komorita et al., 1991; Van Lange, Ouwekerk, & Tazelaar, 2002). In this meta-analysis, the small number of effect sizes related to a tit-for-tat ($N = 4$), a two-for-one ($N = 2$), and a one-for-two strategy ($N = 2$) ruled out meaningful comparisons among the three reciprocal 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 predating Axelrod's (1984) *The Evolution of Cooperation*, Esser and Komorita (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 Esser (1975), "not because subjects were inhibited in exploiting the other but because they resented and refused to be intimidated by the unfairness of an opponent who did not reciprocate concessions" (p. 699). Komorita and Esser, however, obtained some evidence that was incompatible with this fairness interpretation. They interpreted their evidence as consistent with results obtained by Shure et al. (1965), indicating that "subjects who were convinced of the opponent's cooperative orientation were very likely to exploit this vulnerability" (Komorita & Esser, 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 Kimmel (1977) asserted that "a perception that the other is fair is tantamount to an expectation that he will cooperate" (p. 380). We find this assertion reasonable, and evidence obtained by Insko et al. (1998) indicates that the usual greater distrust between groups than between individuals was not present when participants 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.

egy 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 *Pavlov* (Kraines & Kraines, 1989, 1993; Nowak & Sigmund, 1993)—a direct application of Kelley, Thibaut, Radloff, and Mundy's (1962) "win stay, lose change" strategy. Following this strategy, a player "stays" after receiving high outcomes (for the column player, one of the outcomes in the top row of Figure 1) and "changes" after receiving low outcomes (for the column player, one of the outcomes in the bottom row of Figure 1). Another example is the *X*, *Y*, *Y*, *X*, *X*, *X*, *X* strategy used by Rabbie, Visser, and Van Oostrum (1982) in a seven-trial PDG interaction. Because this fourth category captures a set of heterogeneous strategies, we decided not to formulate specific predictions regarding the magnitude of the discontinuity effect in this set of studies relative to the magnitude of the discontinuity effect in studies using other strategies (i.e., unconstrained, cooperative, and reciprocal).

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 without required consensus, and groups with required consensus in the context of one of two *n*-person generalizations of the PDG: the intergroup public goods (IPG) game (Rapoport & Bornstein, 1987) and the intergroup prisoner's dilemma (IPD) game (Bornstein, 1992). In both the IPG and IPD, players decide whether to invest promissory notes. 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 promissory notes than the other side. Yet, when both sides invest all their promissory notes, 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.[6]

In Insko et al.'s (1994) experiment, group members each had one promissory note, and individuals each had three notes. When played between two groups, both games involve separate decisions by individual group members as to whether they want to invest their promissory note; 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 promissory notes. Insko et al.'s (1994) results indicated that groups were more competitive (i.e., invested more) than individuals in both the 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 *n*-person generalizations 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 the 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 a promissory note, the total number of notes 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 interrelationship of own group members' decisions and outcomes, a state of affairs called *procedural interdependence* (Insko et al., 1994; Wildschut et al., in press; Wildschut, Lodewijkx, & 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 interdependence 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 can escape the appearance of selfishness by claiming that their competitive 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 (Wildschut et al., 2001, in press).

A number of discontinuity experiments have contrasted interindividual 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 interindividual 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 basis for the discontinuity effect is thus weakened or removed. Furthermore, fear should be reduced when members of opposing groups interact on an interindividual basis. This weakens 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

[6] The IPG and IPD differ in that the former models conflict of interest over step-level public goods, whereas the latter models conflict of interest over continuous public goods. The IPG has a step-level character because investing more notes than one's opponent is associated with a fixed increase in outcomes regardless of the extent to which own investments exceed the opponent's investments. As an example, one can think of a sports competition in which what matters is winning, regardless as to the margin of victory. Contrary to the IPG, the IPD represents a situation in which outcomes are influenced by the extent to which own investments exceed the opponent's investments. As an example, consider a nondecisive military encounter in which the margin of victory is important.

procedurally interdependent than in studies in which group members were not procedurally interdependent.

Communication

Unconstrained communication. In the majority of discontinuity experiments, participants were allowed to communicate with each other before making their decisions on a given trial. In the intergroup condition, these communication sessions usually involved representatives of each group. These communication sessions frequently resulted in an agreement to cooperate on 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 intercom discussions in a one-trial experiment with a three-choice variation of the PDG (Schopler et al., 1995) indicated that there was an agreement to cooperate in 85% of the intergroup sessions and 76% of the interindividual sessions.

There are at least two reasons why intergroup interactions are likely to be more competitive than interindividual interactions in the presence of unconstrained communication. First, the fear explanation proposes that there is greater distrust in intergroup than in interindividual interactions. The influential research program on attitudes and communication conducted at Yale University in the 1950s identified trustworthiness as an important component of source credibility (Hovland, Janis, & Kelley, 1953). Subsequent research has demonstrated that the trustworthiness of a source affects the persuasiveness of a communication (Walster, Aronson, & Abrahams, 1966). These findings suggest that a schema dictating distrust of out-groups makes intergroup communication less credible and persuasive than interindividual communication and, hence, contributes to the greater competitiveness in intergroup relative interindividual interactions (Insko et al., 1993). Second, even when communication of cooperative intent is perceived as credible and persuasive, groups should be more likely than individuals to attempt to exploit a cooperative opponent because the group context provides anonymity and social support for the pursuit of short-term self-interest (Wildschut et al., 2002).

Constrained cooperative communication. When there is unconstrained communication, the behavior of the two players is not independent. The statistical unit of analysis thus becomes the interaction between players rather than the player per se. Because unconstrained communication frequently results in an agreement to cooperate on the upcoming trial, some studies involved procedures in which the communication between players was controlled by the experimenter. For example, Lodewijkx, Wildschut, Syroit, Visser, and Rabbie (1999) used a procedure in which individuals and groups exchanged written notes as a means of communication. The experimenter collected these notes and replaced them with standard notes that indicated cooperative intent. Similarly, Buelens, Vanbeselaere, and Van Avermaet (2001) used a procedure in which individuals and groups communicated with their opponent through a telephone. In reality, participants communicated 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 independent, and the statistical unit of analysis becomes the player per se. Given the same number of participants, these procedures yield twice as many independent observations as procedures in which communication is unconstrained.

Will constrained cooperative communication influence the magnitude of the discontinuity effect as compared with situations in which communication is unconstrained? This is an open question. On the one hand, as is the case with unconstrained communication, groups should be less likely than individuals to perceive constrained cooperative communication as credible and persuasive. Furthermore, even when constrained cooperative communication is perceived as credible, groups should be more likely than individuals to exploit the cooperative other. On the basis of these considerations, one would not expect the magnitude of the discontinuity effect given constrained cooperative communication to differ significantly from the magnitude of the effect given unconstrained communication. On the other hand, it is unclear whether constrained communication in the form of somewhat impersonal handwritten notes or telephone conversations is sufficient to create trust between individuals. Relevant to this issue, Wichman (1972) examined interindividual PDG interactions under four conditions: in the isolation condition, participants could not communicate in any way; in the see-only condition, participants could use nonverbal but no verbal communication; in the hear-only condition, participants could use verbal but no nonverbal communication; and in the see-and-hear condition, participants could use both verbal and nonverbal communication. The latter condition produced the highest levels of cooperation, followed by the hear-only, see-only, and isolation conditions, respectively. Wichman's findings suggest, at a general level, that not all forms of communication are equally effective in creating trust between individuals and, at a more specific level, that unconstrained face-to-face communication may be more effective in creating trust between individuals than handwritten notes or telephone conversations. If the latter conclusion is correct then the discontinuity effect should be smaller in studies with some form of constrained cooperative communication than in studies with unconstrained communication.

No communication. Some discontinuity experiments include neither unconstrained nor constrained communication. It is clear that without communication, distrust between players should be greater than with communication. Furthermore, because communication between individuals is perceived as more credible and persuasive than is communication between groups, the absence of communication should be more detrimental to interindividual than to intergroup interactions. This is not to say that intergroup interactions are unaffected by communication, but it is to say that interindividual interactions are affected more strongly. Because the absence of communication entails a larger increase in interindividual than in intergroup competition, we expected that the discontinuity effect would be smaller for studies in which communication 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 arrays with symmetrical outcomes, correspondence is indexed by the correlation between

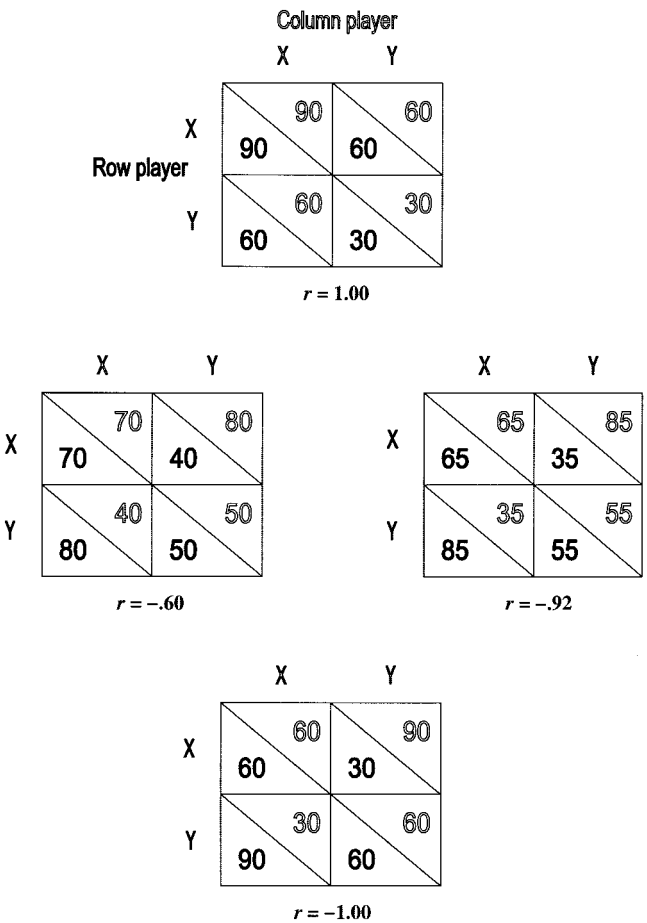

Figure 2. Four matrices with different indices of noncorrespondence.

the row and the column players' outcomes. The matrix at the top of Figure 2 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 correlation between the players' outcomes, is 1.00. The matrix at the bottom of the figure represents the other extreme of the correspondence 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 noncorrespondence 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* choice therefore may involve cooperative intent or simple pursuit of self-interest. The *Y* choice, on the other hand, does not benefit one player over the other and therefore cannot involve competitive intent. With matrices in which the outcomes of both players are correspondent (i.e., positively correlated), mutual *X* choices benefit both players.[7] Because this is true for relations between groups and relations between individuals, there is no reason to expect a difference between interindividual 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 *X* 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 interindividual and intergroup interactions. However, when outcome arrays represent less than perfect noncorrespondence, a discontinuity effect is expected. This situation is represented 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, benefits 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 discontinuity 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 magnitude of the discontinuity effect. Specifically, we expected that decreases in noncorrespondence of outcomes would be associated with greater reductions in intergroup than in interindividual competition. Possible reasons why individuals may not be as strongly affected as groups by degree of noncorrespondence are related to the fear, social support, 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 greedy or self-interested or on the fact that individuals can be identified as being responsible for self-interested behavior. But what about fear or distrust? Conceivably, degree of noncorrespondence may influence the level of distrust. However, past research has demonstrated a strong tendency for individuals to trust other individuals when opportunities for communication are present (Insko et al., 2001).

[7] This generalization holds for outcome arrays that are characterized by column and row main effects but not by a Column × Row interaction—or, in the language of interdependence theory (Kelley & Thibaut, 1978), arrays that contain *reflexive control* (RC; the extent to which the player's choice affects his or her own outcomes) and *fate control* (FC; the extent to which the player's outcomes are affected by the other's choice) but not *behavior control* (BC; the extent to which the joint actions of both players influence the player's outcome). Consider, for example, the matrix in Figure 1. By selecting *Y*, each player can increase own outcomes by 10. Therefore, RC = 10. Similarly, by selecting *X*, each player can increase the outcomes of the other player by 30. Therefore, FC = 30. The RC and FC components are not affected by the other player's decision. Therefore, BC = 0. Kelley and Thibaut (1978) approached the PDG exclusively in terms of RC and FC components and asserted that the presence of a BC component changes the situation from one of pure exchange to one containing elements of coordination.

Thus, the three constraints on the behavior of individuals (relating to the absence of anonymity, social support, and fear) make it less likely that variation in the degree of noncorrespondence affects interindividual interactions.

There are two reasons why we expect the situation to be different for groups. One reason relates to greed and the other to fear. Note that with respect to greed, as the index of noncorrespondence 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 *Y* choice reduces the likelihood that group members will be tempted to pursue their short-term self-interest and act on the social support and anonymity afforded by the group context. This reduces or removes one basis of the discontinuity effect. With respect to fear, note that as the index of noncorrespondence 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 distrust each other. This idea is consistent with the cold-war doctrine of mutually assured destruction (Kissinger, 1956; Wholstetter, 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 conducted or supervised by the present authors and contacted researchers known to have conducted published or unpublished studies on the discontinuity effect. We also sent a request for relevant studies to an Internet discussion forum commonly used by social psychologists (spsp-discuss@stolaf.edu). Next, we conducted an electronic literature search using the PsycINFO (1887–July 2000) and *Dissertation Abstracts International* (1861–July 2000) databases. Key words were *individual, 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 Schopler and Insko (1992), the domain of discontinuity research is "slightly blurred at the boundaries" (p. 124). To sharpen 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 mixed-motive interdependence (i.e., settings that are characterized neither by pure noncorrespondence of outcomes nor by pure correspondence of outcomes). Studies excluded on the basis of this criterion include five studies that compared the punitive or aggressive behavior of individuals and groups toward individual targets (Diener, 1976; Mathes & Kahn, 1975; Ratner & Miller, 2000, Experiment 1; Wolosin, Sherman, & Mynatt, 1975; Yinon, Jaffe, & Feshbach, 1975). These studies were excluded for two reasons. First, they compared individual and group behavior toward individual targets rather than interindividual and intergroup behavior. Second, they did not involve interdependence, as the behavior of one side completely determined the outcomes received by both sides. For example, Wolosin et al. (1975) offered individuals and groups a choice between administering a shock to another individual to achieve a monetary gain and not administering a shock at the expense of the monetary gain. Group decisions were more self-oriented than individual decisions and, thus, more punitive to the target.

Next, we selected only those remaining studies in which the interindividual interaction was not confounded with intragroup interaction. We excluded studies in which participants were first involved in an intergroup interaction and subsequently interacted within their group to divide the proceeds of the intergroup interaction (Doise, 1969, 1978; Wilson & Kayatani, 1968). Finally, we selected only those remaining studies in which the dependent variable was a behavioral measure of competition, such as choice behavior in a mixed-motive situation. We excluded a study in which the dependent variable was evaluative bias (Dustin & Davis, 1970). The search strategy resulted in the selection of 48 studies conducted at 11 different laboratories. All selected studies were laboratory experiments. From these experiments we extracted 134 independent effect sizes (i.e., each effect size was based on a unique sample of participants).[8]

Data Coding

The retrieved studies were coded by Tim Wildschut and Brad Pinter. Each coded approximately half of the studies. Both coders subsequently checked the entire data set and resolved disagreements—of which there were few—through discussion. Five randomly 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 opponent strategy, procedural interdependence, communication, and index of noncorrespondence.

Opponent strategy. Opponent strategy was coded as a categorical variable with four levels: unconstrained, reciprocal, cooperative, and miscellaneous. In studies that were characterized by an unconstrained strategy, actual participants made up both sides in the interaction. The choice behavior of these participants was not constrained 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 (programmed) to follow a reciprocal strategy. Programmed strategies that were coded as reciprocal include tit-for-tat, which starts with a cooperative choice and reciprocates the participants' behavior on the following trials; the two-for-one strategy, which is similar to tit-for-tat except that it responds to a competitive choice with two consecutive competitive choices; and the one-for-two strategy, which is similar to tit-for-tat except that it competes only in response to two consecutive competitive 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 programmed to select the cooperative choice regardless of the participants' choice behavior. Finally, studies that did not involve either an unconstrained, reciprocal, or cooperative strategy were assigned to the miscellaneous strategies category.

Procedural interdependence. Procedural interdependence was coded as a categorical variable with two levels: procedural interdependence present and procedural interdependence absent. Procedural interdependence was present when group members' individual decisions were combined into a collective group decision. In most experiments procedural interdependence was created by imposing a consensus requirement. Procedural interdependence was absent when group members' individual decisions were not combined into a collective group decision. For example, when group members were allowed to engage in intragroup discussion but

[8] A list with complete study and effect size information can be obtained from Tim Wildschut.

still made their decisions individually, we did not consider them to be procedurally interdependent.[9]

Communication. Communication was coded as a categorical variable with three levels: unconstrained communication, constrained cooperative communication, and no communication. In studies that were characterized by unconstrained 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 handwritten 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 mathematically as

$$\mathrm{INC} = \frac{\sigma^2_{\mathrm{sums}} - \sigma^2_{\mathrm{diffs}}}{\sigma^2_{\mathrm{sums}} + \sigma^2_{\mathrm{diffs}}}.$$

Here, INC refers to the index of noncorrespondence, σ^2_{sums} refers to the variance of the cell sums (i.e., the values obtained by adding the outcomes of Sides A and B in each cell), and $\sigma^2_{\mathrm{diffs}}$ 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):

$$\mathrm{INC} = r\left[\frac{2\sigma_{\mathrm{A}}\sigma_{\mathrm{B}}}{\sigma_{\mathrm{A}}{}^2 + \sigma_{\mathrm{B}}{}^2}\right].$$

Here, σ_{A} refers to the standard deviation of Side A's outcomes, and σ_{B} 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 1.00, 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 include: authors and full citation, source (journal, edited volume, thesis or dissertation, or unpublished manuscript), 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), recruitment method (participant pool, monetary incentive, or volunteer), sex composition of the sample (all males, all females, males and females in interactions that were homogenous with respect to sex, or males and females in interactions that were heterogeneous with respect to sex),[10] grand mean of the matrix values, nature of the matrix payoffs (money or points), and number of trials anticipated (single or multiple).[11]

Statistical Methods

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

$$g = \frac{M_{\mathrm{intergroup}} - M_{\mathrm{interindividual}}}{S_{\mathrm{pooled}}}.$$

Here, $M_{\mathrm{intergroup}}$ is the mean proportion of intergroup competition, $M_{\mathrm{interindividual}}$ is the mean proportion of interindividual competition, and S_{pooled} is the pooled standard deviation:

$$S_{\mathrm{pooled}} = \sqrt{\frac{(n_{\mathrm{intergroup}} - 1)s^2_{\mathrm{intergroup}} + (n_{\mathrm{interindividual}} - 1)s^2_{\mathrm{interindividual}}}{n_{\mathrm{intergroup}} + n_{\mathrm{interindividual}} - 2}}.$$

Here, $n_{\mathrm{intergroup}}$ and $n_{\mathrm{interindividual}}$ are the number of observations in the intergroup and interindividual conditions, respectively; $s^2_{\mathrm{intergroup}}$ and $s^2{}_{\mathrm{interindividual}}$ 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 Hedges's g. These approaches are rank ordered in terms of preference, starting with the most preferred method.

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[9] Some studies used a one-way 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 (Insko et al., 1987, 1988, 1994; Wildschut et al., 2001). For these studies, we computed effect sizes 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 more 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.

[10] 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 these 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.

[11] Insko et al. (2001) recently demonstrated the role of anticipated future interaction in interindividual–intergroup discontinuity reduction. It may therefore seem surprising that the explanatory model 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, Insko et al. (2001) demonstrated that the anticipation of future interaction reduces the discontinuity effect—through a reduction in intergroup competition—only when groups trust each other and are composed of participants who have the capacity to reason abstractly (i.e., project their outcomes in the future). Thus, the moderating effect of anticipated future interaction occurs only under specific circumstances (i.e., high trust and high abstractness). Although trust of the opponent is assessed in many of the studies included in this meta-analysis, participants' capacity to reason abstractly was assessed only by Insko et al. (2001). This rules out the possibility of examining across multiple studies the complex pattern of results obtained by Insko et al. (2001). Second, most studies in which participants anticipated interacting on multiple trials actually involved multiple trials. This confounding of trials anticipated and trials played is problematic because, contrary to a single-trial interaction, a multiple-trial interaction creates the opportunity for escalation of conflict.

1. We computed most *g*s (101 of 134 independent effect sizes) by using sample sizes, mean proportions of competition, and standard deviations reported separately for the interindividual and intergroup conditions. Mean proportions of competition were calculated by dividing the mean number of competitive choices by the number of choices per interaction. Consider an experiment that compares interindividual and intergroup interactions in the context of a 10-trial PDG. Assuming that both sides consist of actual participants whose strategies are unconstrained, the total number of choices per interaction is 20 (10 choices by each side). The mean proportion of competition in any given condition can then be calculated by dividing the mean number of competitive choices in that condition by 20. When participants interacted with a programmed opponent, calculations were based exclusively on the participants' choices.

2. For 16 effect sizes, sample sizes and means were reported separately for the interindividual and intergroup conditions, but no standard deviations were available. In these cases we computed the pooled standard deviation by extracting a mean square error (MS_{error}) term from an analysis of variance (ANOVA) that included a contrast between interindividual and intergroup interactions. In none of these cases were ANOVA results reported in sufficient detail to allow us to adjust the extracted MS_{error} by returning other between-groups sources of variability to the error term.

3. When a *t*, *F*, or chi-square statistic was reported for a comparison of interindividual and intergroup interactions, we converted this test statistic to *g* (17 effect sizes). For studies in which the dependent variable was dichotomous (i.e., studies in which participants interacted on only one trial with a programmed other), effect sizes were based on chi-squares even when means and standard deviations could be retrieved. When we used means and standard deviations to compute effect sizes for these studies, these effect sizes were within rounding error of those based on chi-squares.

Random-effects model. We used a random-effects meta-analytic approach. The random-effects procedure was appropriate because we wanted to make unconditional inferences that generalize to a hypothetical population of studies that could exist rather than to the population of studies at hand (Hedges & Vevea, 1998). Random-effects models differ from fixed-effects models in that they incorporate both random variation associated with the sampling of persons or other units into studies and an additional component of variation associated with the sampling of the studies themselves from a hyperpopulation of possible studies. The first type of variation is regarded as known in meta-analysis. Each effect size has a variance conditional on the true effect. This conditional variance represents uncertainty that arises from the sampling of observations within that individual study and is primarily a function of the within-study sample size. The second type of variation is represented by a model parameter describing variability among effect sizes that exceeds what one would expect given the conditional variances. This excess variability, called the *between-studies variance component*, describes the variance of the population of hypothetical studies that do, or could, exist.

Explanatory model. As we already described in some detail, we identified four variables that we expected to predict variation in effect sizes (i.e., opponent strategy, procedural interdependence, communication, and noncorrespondence of outcomes). We implemented a model that predicts effect size as an analysis of covariance-like function of these variables. This explanatory model was estimated using a random-effects conceptualization, 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 moderators included in the model, some is associated with the sampling of observations into individual studies, and the rest is thought of as variation associated with the sampling of studies and represented by a variance component. We used the method of unconditional maximum likelihood to estimate model parameters. Before testing these main-effects models, we confirmed that all estimable interactions were nonsignificant.

Planned contrasts. In this analysis, opponent strategy and communication are categorical variables with more than two levels. We decided to explore potentially significant associations of these moderators with the discontinuity effect by means of planned contrasts. We designed these contrasts to test certain specific predictions regarding the associations of opponent strategy and communication with the magnitude of the discontinuity effect.

Recall that the opponent strategy variable has four levels: unconstrained interaction, reciprocal strategies, cooperative strategies, and miscellaneous strategies. We first examined a contrast comparing studies using reciprocal strategies versus studies using unconstrained or cooperative strategies. We selected this contrast to test the hypothesis that, relative to unconstrained and cooperative strategies, reciprocal strategies are associated with a reduction in the discontinuity effect. Second, we examined a contrast comparing unconstrained strategies versus cooperative strategies. It is clear that, relative to unconstrained strategies, cooperative strategies reduce fear and thus remove one basis for the discontinuity effect. It is less clear whether cooperative strategies reduce or reward greed. The second contrast bears 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 miscellaneous strategies versus studies using unconstrained, cooperative, or reciprocal strategies. Because the miscellaneous category captures a set of heterogeneous strategies, it would be unwise to interpret any difference, or lack thereof, between this category and the other strategy categories. The third contrast was included in the analyses, however, to represent accurately the four-level strategy variable (Cohen & Cohen, 1983).

The communication variable has three levels: unconstrained communication, constrained cooperative communication, and no communication. We first examined a contrast comparing studies with no communication versus studies with unconstrained communication or constrained cooperative 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 when there is no communication. This hypothesis is based on the idea that, because the out-group schema dictates distrust of other groups, groups do not derive as much benefit from communication as do individuals. The elimination of communication therefore increases competition between individuals to a greater extent than competition between groups. Furthermore, even when the communication of cooperative intent is perceived as credible, groups should be more likely than individuals to exploit the cooperative opponent because the group context provides anonymity and social support. Second, we examined a contrast comparing studies with unconstrained communication versus studies with constrained cooperative communication. Whether there is a difference between studies with unconstrained communication and studies with constrained cooperative communication is an open question. On the one hand, it is possible that constrained cooperative communication and unconstrained communication function in identical 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 impersonal handwritten notes or telephone conversations is less successful in creating trust between individuals than unconstrained communication. If this is the case, the magnitude of the discontinuity effect should be smaller for studies with constrained cooperative communication than for studies with unconstrained communication.

Sensitivity analyses. In this meta-analysis we made a number of assumptions. For instance, we focused on certain moderators for theoretical reasons and, in so doing, assumed implicitly that others were less important. 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 potential moderators one at a time. Our interest here was in determining, first, whether other significant moderators of the discontinuity effect would

emerge and, second, whether findings for the a priori model 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 compensates for publication bias by estimating a weight function representing the relative likelihood of studies with p values in particular ranges surviving the publication selection process (Vevea & Hedges, 1995). The model presented by Vevea and Hedges 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-analytic data sets like the current one. Accordingly, we used a new approach in which the weights are fixed at user-specified values, and the adjusted parameters are estimated conditionally on that fixed weight function (Vevea & Woods, 2002). In this approach, sets of weights representing varying degrees of selection bias can be applied, and the impact on the parameter estimates assessed. If the results are not overturned or too severely attenuated even by weights that represent quite extreme selection processes, we can assert with confidence that our findings 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: $\chi^2(3, N = 130) = 24.15$, $p < .01$, for opponent strategy; $\chi^2(1, N = 130) = 5.91$, $p < .02$, for procedural interdependence; $\chi^2(2, N = 130) = 23.89$, $p < .01$, for communication; and $\chi^2(1, N = 130) = 10.27$, $p < .01$, for noncorrespondence of outcomes.[12] Table 1 shows parameter estimates for the random-effects model, broken down into the specific contrasts that are described in the Method section.[13]

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 reciprocal strategies versus the pooled unconstrained and cooperative strategies. The significant negative coefficient for this contrast indicates that, as predicted, the discontinuity effect was smaller in studies using reciprocal strategies than in studies using an unconstrained or cooperative strategy.

The second contrast compared the unconstrained 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 unconstrained 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 unconstrained 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 handwritten 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 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 alpha of .017 (.05/3) revealed that these two sets of studies did, in fact, differ significantly ($b = 0.27$, $z = 4.01$, $p < .01$). Studies with constrained cooperative communication were associated with effect sizes of intermediate magnitude. The second contrast already indicated that studies with constrained cooperative communication differed significantly from studies with unconstrained communication, but did studies with constrained cooperative communication also differ significantly from studies with no communication? A post hoc test comparing these

[12] These chi-square statistics are more commonly referred to as *Q-between* statistics and are analogous to *F*s in a conventional ANOVA or regression analysis. We refer to these statistics as chi-squares rather than Q-between statistics because the presence of a continuous moderator (i.e., index of noncorrespondence) makes the concept of a between-groups test somewhat inapplicable here.

[13] The first contrast on the strategy variable was coded as −1/2 for unconstrained strategies, −1/2 for cooperative strategies, 1 for reciprocal strategies, and 0 for miscellaneous strategies. The second contrast was coded as −1 for unconstrained strategies, 1 for cooperative strategies, 0 for reciprocal strategies, and 0 for miscellaneous strategies. The third contrast was coded as −1/3 for unconstrained strategies, −1/3 for cooperative strategies, −1/3 for reciprocal strategies, and 1 for miscellaneous strategies. The first contrast on the communication variable was coded as 1/2 for unconstrained communication, 1/2 for constrained cooperative communication, and −1 for no communication. The second contrast was coded as −1 for unconstrained communication, 1 for constrained cooperative communication, and 0 for no communication.

Table 1
Parameter Estimates for Random-Effects Contrasts

Parameter	Parameter estimate	*SE*	*z*	*p* <
Intercept	−0.358	0.195	−1.84	.066
Strategy				
Reciprocal vs. cooperative and unconstrained	−0.652	0.147	−4.44	.001
Cooperative vs. unconstrained	0.015	0.100	0.15	.882
Miscellaneous vs. other categories	−0.167	0.194	−0.86	.388
Procedural interdependence	0.280	0.115	2.43	.015
Communication				
Constrained cooperative and unconstrained vs. absent	0.113	0.092	1.23	.219
Constrained cooperative vs. unconstrained	−0.369	0.094	−3.93	.001
Index of noncorrespondence	−0.727	0.227	−3.20	.001

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 that 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 0.077. 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. Our 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 populated cells, predicted mean effect sizes were calculated for two values of the index of noncorrespondence variable: −.80 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 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 (a) the opponent's strategy was unconstrained by the experimenter, (b) procedural interdependence among group members was present (i.e., group members acted collectively), and (c) 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 interdepen-

[14] One consequence of applying random-effects models is that parameter estimates use less information from the largest studies compared with fixed-effects analyses. This occurs because random-effects estimation weights by the inverse of the sum of the conditional variances and the between-studies variance component. The addition of the variance component evens out the weights; thus, a large study that receives three times as much weight as a small study in a fixed-effects analysis might receive only twice as much weight in the random-effects analysis. Ideally, parameter estimates in each approach are similar. If this is not the case, the implication is that the largest studies represent populations for which the true mean effect size is different from that of the smaller studies. That could be a disturbing finding. Accordingly, we compared the parameter estimates for the random-effects model presented in Table 1 with the same estimates for a fixed-effects model. The results were reassuring. The average absolute difference between the parameter estimates for the random- and fixed-effects models was only .020, and in no case did the estimates differ by more than .032.

Table 2
Predicted Mean Effect Sizes and Standard Errors of Prediction for Populated Cells

Strategy	Procedural interdependence	Communication	No. of effects	Predicted mean effect size (*SE*) INC = −.80	Predicted mean effect size (*SE*) INC = −.60
Unconstrained	Present	Unconstrained	87	1.300** (0.092)	1.150** (0.070)
Unconstrained	Present	Absent	11	0.757** (0.125)	0.612** (0.119)
Unconstrained	Absent	Unconstrained	5	0.736** (0.244)	0.591* (0.236)
Cooperative	Absent	Absent	7	0.227 (0.156)	0.082 (0.145)
Reciprocal	Present	Absent	6	−0.206 (0.190)	−0.351 (0.186)

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 reciprocal, (b) procedural interdependence 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 moderators of the discontinuity effect in our a priori 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, $\chi^2(3, N = 130) = 28.81$, $p < .01$. We explored the sex composition effect with three planned contrasts. The first contrast compared studies with male and female participants in heterogeneous interactions versus all studies with homogeneous interactions (i.e., all males, all females, and males and females in homogeneous interactions). The second contrast compared studies with male and female participants in homogeneous interactions versus studies with all males or all females. The third contrast compared studies with all male participants versus studies with all female participants.

The first ($b = 0.24$, $z = 2.61$, $p < .01$) and third contrasts ($b = 0.20$, $z = 3.85$, $p < .01$) 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 males than for females. 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, $\chi^2(2, N = 130) = 5.67$, $p < .06$. This indicates that sex composition is modeling partially the same variability in effect sizes as communication. However, the comparison between unconstrained communication and no communication remained significant ($b = 0.17$, $z = 2.15$, $p < .05$). In light of the fact that we identified communication as a moderator of the discontinuity effect on theoretical grounds, we believe that our a priori 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 p 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 larger 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.[15] When selection bias is absent, the plot exhibits vertical symmetry, narrowing as one 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 publication-bias scenarios, using a modification of the

[15] We excluded one very large effect size ($g = 9.15$) from the plot to allow a clearer portrayal of the pattern of effect sizes. Note that because the combined sample size for this effect was small ($N = 8$), this effect had little influence on the analyses.

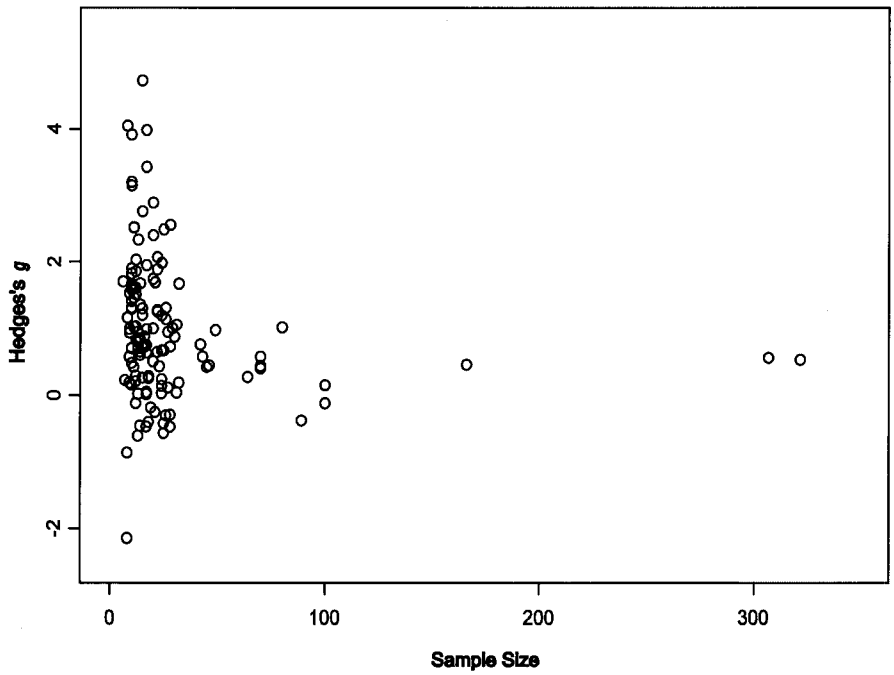

Figure 3. Funnel plot of effect size (Hedges's *g*) as a function of sample size.

statistical model presented by Vevea and Hedges (1995). These scenarios, referred to as *moderate* and *severe*, are implemented using a set of a priori weights that represent the assumed probability of effects with one-tailed *p* values in various ranges being included 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 .30 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 .01 were retrieved, effects with *p* values between .01 and .025 were retrieved with a probability of .95, effects with *p* values between .025 and .05 were also retrieved with a probability of .95, effects with *p* values between .05 and .10 were retrieved with a probability of .90, 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 .70, 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, .90, .80, .60, .50, .30, and .10.

Table 3 shows predicted means adjusted for each of the weight functions (i.e., moderate and severe) beside a repetition of the predicted means listed in Table 2 (i.e., no bias assumed). It is evident that the predicted mean effect sizes do not vary strongly across the different publication-bias scenarios. Consider, for instance, the situation in which (a) the opponent's strategy is unconstrained by the experimenter; (b) procedural interdependence among group members is present; and (c) 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 −.60, and −.80, respectively. When we assumed moderate selection bias, those values were 1.044 and 1.194. When we assumed

Table 3
Predicted Mean Effect Sizes for Populated Cells Under Different Publication-Bias Scenarios

			No bias		Moderate bias		Severe bias	
Strategy	Procedural interdependence	Communication	INC = −.80	INC = −.60	INC = −.80	INC = −.60	INC = −.80	INC = −.60
Unconstrained	Present	Unconstrained	1.300	1.150	1.194	1.044	1.140	0.982
Unconstrained	Present	Absent	0.757	0.612	0.694	0.544	0.660	0.503
Unconstrained	Absent	Unconstrained	0.736	0.591	0.602	0.451	0.495	0.337
Cooperative	Absent	Absent	0.227	0.082	0.155	0.005	0.099	−0.059
Reciprocal	Present	Absent	−0.206	−0.351	−0.300	−0.450	−0.394	−0.552

Note. INC = index of noncorrespondence.

severe selection bias, the values were 0.982 and 1.140. It thus appears highly unlikely that the results that are of greatest theoretical interest to us are due to a biased publication process.

Supplementary Analyses: Toward a Detailed Description of Discontinuity–Moderator Associations

The analyses that model variation in the magnitude of the discontinuity effect as a function of the four a priori moderators do not clarify whether these moderators relate to interindividual competition, intergroup competition, or both. For instance, our finding that the discontinuity effect was larger with than without communication does not clarify whether this result occurred because communication reduces interindividual competition, increases intergroup competition, or both. To address this remaining issue, we conducted separate meta-analyses of interindividual and intergroup competition, modeling each as a function of relevant moderators. We were able to retrieve information regarding the proportion 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, communication, and index of noncorrespondence. Procedural interdependence 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 intergroup competition.

As we emphasized earlier, meta-analysis is a correlational process, 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 interindividual and intergroup competition. First, whereas the explanatory 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 concern with providing a detailed description of the identified discontinuity–moderator 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-motive situations. Again, we are concerned with exploring the discontinuity–moderator associations in our present data set rather than with reviewing the complete literatures 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, $\chi^2(3, N = 129) = 4.61$, $p < .21$, or with intergroup competition, $\chi^2(3, N = 129) = 5.47$, $p < .15$. The planned contrast of studies with reciprocal strategies versus those with cooperative and unconstrained strategies indicated that intergroup interactions were marginally less competitive when the opponent followed a reciprocal strategy than when the opponent's strategy was unconstrained or cooperative ($b = -0.05$, $z = -1.67$, $p = .10$). This pattern was reversed 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 ($b = -0.01$, $z = -0.40$) or for intergroup interactions ($b = -0.05$, $z = -1.29$).

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 × Opponent Strategy interaction. For our present purposes, the relevant contrast is between the tit-for-tat condition and the unconstrained 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 interindividual interactions there was a nonsignificant reversal of this pattern.

Overall, our meta-analytic findings and the experimental findings of Insko et al. (1998) are in agreement on two counts. First, reciprocal strategies were associated with reductions in the magnitude of the discontinuity effect. Second, the descriptive pattern is 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 intergroup 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 competition and a significant increase in interindividual competition. It is apparent that further research is needed to clarify whether reciprocal strategies attenuate the discontinuity effect through an increase in interindividual competition, a reduction in intergroup competition, or both. Given that the reduction in intergroup competition 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 a significant association between communication and interindividual competition, $\chi^2(2, N = 129) = 74.16$, $p < .01$. Planned contrasts show, first, that interindividual competition was lower when some form of communication was present rather than absent ($b = -0.14$, $z = -6.26$, $p < .01$). Second, interindividual competition was higher when communication was constrained rather than unconstrained ($b = 0.08$, $z = 3.19$, $p < .01$). The latter finding is consistent with the possibility that unconstrained communication is more effective in creating trust between individuals than is constrained communication (cf. Wichman, 1972). The association between communication and intergroup competition was also significant but weaker, $\chi^2(2, N = 129) = 9.57$, $p < .01$. Intergroup competition was lower when some form of communication was present rather than absent ($b = -0.07$, $z = -2.92$, $p < .05$). The contrast between constrained and unconstrained communication was not significant ($b = 0.05$, $z = 1.18$). As predicted, the previously reported discontinuity–communication association reflects that communi-

cation reduced interindividual competition more than it reduced intergroup competition. These results are in agreement with experimental findings by Insko et al. (1993).

Noncorrespondence of outcomes. There was a significant association between index of noncorrespondence and intergroup competition, $\chi^2(1, N = 129) = 28.69$, $b = -0.39$, $p < .01$. The association between index of noncorrespondence and interindividual competition was marginal, $\chi^2(1, N = 129) = 3.52$, $b = -0.08$, $p < .07$. As predicted, the previously reported discontinuity–noncorrespondence association reflects that decreases in noncorrespondence (i.e., less negative index values) were associated with larger reductions in intergroup than in interindividual competition. These results are in agreement with experimental findings by Schopler et al. (2001).

Variance components. The respective variance component estimates for the analyses of interindividual and intergroup competition are .008, $Q(122) = 310.53$, $p < .01$, and .026, $Q(122) = 421.31$, $p < .01$.

Discussion

In his "law of three stages," the French philosopher Comte (1830/1854) proposed that the sciences move gradually from the theological, through the metaphysical, to the positive stage of development. Comte's theory provides a formal way of evaluating past and present approaches to the question as to why individuals often behave in a hostile and competitive manner when they are banded together in a group. Certainly, it would be inaccurate to call Le Bon and McDougall metaphysicians, but we agree with G. W. Allport (1968) that their view of the group mind as a mental entity that influences the behavior of group members "unnecessarily imposed metaphysical blocks in the path of constructive conceptualization" (p. 55). Still, despite the fact that social psychologists (e.g., Lewin, Lippitt, & White, 1939; Sherif, 1936) adopted the tools of positivism—such as experimentation and statistics—shortly before World War II, the "constructive conceptualization" of the interindividual–intergroup discontinuity problem was not formulated until Rabbie et al. (1982) and McCallum et al. (1985) phrased the issue in terms of a comparison between interindividual and intergroup behavior in the context of conflict of interest.

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 effect have been advanced. The social support and identifiability explanations center on the greater greed in intergroup relative to interindividual relations. The fear explanation centers on the greater distrust in intergroup relative to interindividual relations. We identified four moderators of the discontinuity effect—each based on the theoretical perspective that interindividual–intergroup discontinuity flows from the greater fear and greed in intergroup relative to interindividual interactions. These are opponent strategy, procedural interdependence, communication, and noncorrespondence 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 insult may seem generous to the sophisticated mind. But in 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 issue, a man who does not show backbone acts unwisely. He invites the bestiality of the mob which is always ready to have its brutal fun but is afraid to stick out its neck when it knows that it will be resisted. (p. 121)

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 tit-for-tat, 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, competition always begets competition. Indeed, one could say that reciprocal strategies have backbone. These strategies send the clear message that, from the perspective of maximizing long-term outcomes, competition 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 competition in an absolute sense, a conservative interpretation of our findings suggests that reciprocal strategies reduce intergroup competition relative to interindividual 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 ineffective. 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 unconstrained. 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 one basis for the discontinuity effect: the greater fear in intergroup relations as compared with interindividual relations. We were less certain, however, whether a cooperative strategy reduces or rewards greed. One possibility is that a cooperative strategy reduces greed because, in the absence of threat, greed cannot be justified through defensive assertions (Insko et al., 1993). Another possibility is that by responding to competition with cooperation, or "overlooking insult," the cooperative strategy invites greed. Our findings are consistent with the second possibility: Groups were more likely than individuals to compete with a cooperative opponent. Because a cooperative opponent does not pose a threat, the greater com-

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 interindividual–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 course of 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 confounded 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 PDG. 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 competition 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.

Our 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 undifferentiated. 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 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 gain 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.

Noncorrespondence of Outcomes

We proposed that the occurrence of interindividual–intergroup discontinuity is restricted to situations that are characterized by noncorrespondence of outcomes and that the magnitude of the discontinuity effect is linearly related to the degree of noncorrespondence. The meta-analytic results provide good support for these claims. There are at least two plausible explanations for the association of noncorrespondence 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, note that as the index of noncorrespondence 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 anonymity afforded by the group context.

With regard to fear, note that as the index of noncorrespondence 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 advantages of mutual cooperation and the disadvantages of mutual competition ever 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 unattractive prospect not just for themselves, but for the opponent as well. This is the point that sometimes has been made about the role of nuclear deterrence in reducing 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, 1956; Wholstetter, 1959) is compelling (Schopler 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 referred to by Susskind and Cruikshank (1987) as *reframing* and by Fisher and Ury (1983) in terms of focusing on interests rather than positions to identify options for mutual gain. Fisher and Ury provided the example of the Camp David accord for a demilitarized Sinai Desert under Egyptian sovereignty, which satisfied Israel's concern for security and Egypt's concern for sovereignty (Schopler et al., 2001).

An Alternative Perspective on Discontinuity

In the spirit of critical rationalism (Popper, 1968), Philips (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. viii–ix). 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 Rabbie's reciprocity hypothesis are (a) that by engaging 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 intragroup 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 findings contradict the idea that groups are more likely than individuals to reciprocate the opponent's cooperative behavior. Note, in this respect, 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—driven by short-term self-interest or greed—are less likely than individuals to reciprocate the expected cooperative behavior of the outgroup. This hypothesis goes against our reciprocity hypothesis which assumes that groups are more likely than individuals to match the competitive or cooperative behavior of the other party. (p. 488)

Our meta-analysis found no evidence for the reciprocity hypothesis. On the contrary, we found that the discontinuity effect was descriptively larger when participants interacted with a cooperative rather than an unconstrained opponent.

In support of his reciprocity hypothesis, Rabbie (1998) cited two studies that used a cooperative strategy but did not find a discontinuity effect—namely, Lodewijkx and Rabbie (1992) and Rabbie and Lodewijkx (1991). How can we account for the results of these studies given the present meta-analytic findings? There are three possible explanations, relating to procedural interdependence, communication, and noncorrespondence 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 was smaller when procedural interdependence was absent rather than present. Second, in the two studies in question, there was no unconstrained communication between sides. The meta-analysis shows that the discontinuity effect was smaller when unconstrained communication was absent rather than present. Third, Lodewijkx and Rabbie (1992) and Rabbie and Lodewijkx (1991) used matrices with an index of noncorrespondence equal to $-.50$. Seventy-seven percent of all effect sizes in our meta-analysis, however, were associated with a more negative index of noncorrespondence. The meta-analysis indicates that the discontinuity effect became smaller as the index of noncorrespondence became less negative. When we used our explanatory model to generate a predicted effect size for studies in which (a) opponent strategy is cooperative, (b) procedural interdependence is absent, (c) communication is absent, and (d) index of noncorrespondence is $-.50$, we obtained a $\hat{g}$ of .009, *ns*. 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-motive matrix games. This raises two interrelated questions. First, is the discontinuity effect confined to laboratory settings? Second, can a matrix representation of outcomes model 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-one interactions, and group-on-group interactions. One-on-group and group-on-one interactions 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 (group-on-one). Participants classified and then rated their interactions. The mean rated 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 rated 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 results in both studies remained unchanged after all recorded interactions relating to sports and games were removed from the data set and can therefore not be attributed to the tendency for competitive intramural sports to be between groups rather than between individuals. These studies suggest that the greater competitiveness of intergroup relative to interindividual interactions also occurs in nonlaboratory contexts.

What about the second question? To examine whether the discontinuity effect is dependent on a matrix representation of outcomes, Schopler et al. (2001, Experiment 1) devised a nonmatrix interaction involving outcomes that paralleled exactly the outcomes in a PDG matrix. Participants made a standard set of origami figures (yellow rabbits for Side A and pink swans for Side B) and then had to decide whether to give half of their figures to their opponent. On each trial, individuals made 4 figures, and groups made 12 figures. A pair of figures composed of own figures, a primary set, was worth 10 cents, and a pair of figures composed of different figures, a secondary set, was worth 15 cents. By instructing participants to either give half of their figures to their opponent (equivalent to selecting the cooperative *X* choice) or to keep all of their figures (equivalent to selecting the competitive *Y* choice), the outcomes represented in a 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 (*YY*), each group would have six primary sets and receive 60 cents. Finally, if a group gave away half of its figures but received 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 (*YX* or *XY*).

Schopler et al. (2001) examined the discontinuity effect under three conditions. The matrix-only condition replicated previous research in that a PDG matrix was used to represent the outcomes. In the origami-only condition, the outcomes associated with combinations of origami figures were analogous to the outcomes in the PDG matrix for the matrix-only condition. Finally, the matrix-origami condition was similar to the origami condition, but participants in this condition also received the outcome matrix used in the matrix-only condition. Results indicated an overall discontinuity effect that 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 can be just as meaningful as interaction in the context of a functionally equivalent set of exchange rules" (Schopler et al., 2001, p. 635).

Directions for Future Research

An unexpected prediction of our explanatory model was that a descriptively large reversal of the discontinuity effect will occur when (a) participants interact with an opponent following a reciprocal strategy, (b) procedural interdependence is absent in the intergroup condition, (c) communication is absent, and (d) the index of noncorrespondence equals zero. We know of no studies, however, that have compared interindividual and intergroup interactions under these specific circumstances. This prediction thus presents a specific avenue for future research: Can the anticipated reversal of the discontinuity effect be confirmed, and if so, why does it occur? At this point we can only speculate as to the reasons for such a reversal. One possibility, originally suggested by Insko et al. (1998), is that individuals may feel that the quick-to-retaliate reciprocal strategies are particularly unforgiving and harsh in the context of generally trusting interindividual relations (Van Lange et al., 2002). Group members, on the other hand, may feel that a retaliatory strategy is legitimate in the context of an interaction that is expected to be competitive and deceitful. Differential expectations regarding the nature of interindividual and intergroup interactions may thus lead people to form unfavorable impressions of reciprocal individuals as compared with reciprocal groups.

A second avenue for future research lies in identifying additional moderators of the discontinuity effect. The sensitivity analyses examined a host of variables that were not included in our a priori model. This is not to say, however, that we exhausted the pool of potentially important variables. For instance, variables that influence perceived groupness or entitativity (Campbell, 1958), such as within-group similarity and proximity, could not be examined given the lack of substantive differences between the retrieved studies in terms of those variables. Because out-group entitativity may give rise to out-group fear (Wildschut et al., in press), 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 intergroup relative to interindividual relations stems 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 other groups (cf. Horwitz & Rabbie, 1982; Tajfel, 1970). Early reference to this idea can be

found in Plato's (trans. 1891) *The Republic*, in which Polemarchus defends a traditional maxim of Greek morality that "justice is the art which gives good to friends and evil to enemies" (p. 7).

Consistent with the existence of an in-group-favoring norm, Wildschut et al. (2002) demonstrated that, in the context of a PDG, group members who expected that their decision would be made public to the other members of their group were more competitive toward an opposing group than were group members who expected that their decision would remain private. Data were further consistent with the possibility that the difference between public and private responding was mediated by a concern with maximizing own group outcomes. First, there was greater concern for own group outcomes in the public than in the private condition, and second, when concern with own group outcomes was statistically controlled, the effect of public versus private responding on competition was no longer significant. Although these findings provide evidence for the postulated in-group-favoring norm, one could argue that when a person's behavior is public and influences the welfare of other group members, it is simply rational to take the interests of these other group members into account to gain their approval and avoid their disapproval. Regarding this issue, Thibaut and Kelley (1959) asserted that norms arise from rationality—or, in the language of interdependence theory, from an attempt by both parties in a dyad to maximize outcomes over time. They illustrated this idea with an example of a husband and wife who like to go out together on weekends. Unfortunately, the wife prefers to go dancing, whereas the husband prefers to go to the movies. Thibaut and Kelley suggested that the couple might resolve this conflict of interest and maximize joint outcomes over time by alternating between jointly going to the movies on one weekend and jointly going dancing on the following weekend. What is a rational solution at first may then become normative over time, and hence, rationality and normative processes may become confounded—at least at some point in time. Although Thibaut and Kelley did not mention this, their general argument is compatible with Bentham's (1789/1879) and Mill's (1863) concept of utilitarianism—that norms arise from what is the greatest good for the greatest number (cf. Russell, 1945, pp. 773–782).

The concept of an in-group-favoring norm may shed light on some provocative findings reported recently by Morgan and Tindale (2002). Using a social decision scheme approach, they examined social influence processes within 3-person groups by asking group members to make individual PDG choices before engaging in a discussion to reach consensus regarding a group decision. Morgan and Tindale found that when the individual decisions indicated unanimity among the 3 group members, the final group decision almost always corresponded to these individual decisions. When the group members' individual decisions were not unanimous, however, things were not that simple. Whereas a competitive group decision was reached in 91% of cases when all but 1 group member had initially selected the competitive choice, a cooperative group choice was only reached in 48% of cases when all but 1 group member had initially selected the cooperative choice. On the basis of these findings, Morgan and Tindale (2002) concluded that "defection [competition] was an easier position to defend during group discussion than was cooperation. Not only were majorities that favored defection rarely overridden, but minorities favoring defection were successful more often than not" (p. 60).

Morgan and Tindale (2002) interpreted these asymmetric social influence patterns in terms of shared task representations (Tindale, Smith, Thomas, Filkins, & Sheffey, 1996), which they defined as "any task/situation relevant concept, norm, perspective, processing 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 representation, 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 on competition documented by Insko et al. (2003) if, first, it can be assumed that in-group procedural interdependence implies a sense of groupness or entitativity and if, 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 discontinuity effect by promoting intergroup cooperation. An interesting possibility for promoting intergroup cooperation relates to recategorization (Gaertner, Dovidio, Anastasio, Bachman, & Rust, 1993; Gaertner, Mann, Murrell, & Dovidio, 1989). Recategorization refers to the transformation of group members' cognitive representation of the intergroup 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 intergroup bias by increasing liking for former out-group members (e.g., Gaertner et al., 1989). To our knowledge, however, no one has 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), participants were initially categorized into four 3-person groups. These groups were subsequently combined into two competing 6-person groups or, in a different version, two independent 6-person groups, each working cooperatively on a task. Consistent with previous research, the recategorization procedure successfully increased liking for former out-group members. However, when participants were returned to their initial 3-person groups and interacted with former in-group members in the context of a PDG, they were as competitive as groups whose members were not exposed to the recategorization procedure.

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 two separate groups. This relation between trust or expected cooperation and categorization, although ambiguous as to causal sequence, suggests the interesting possibility that recategorization can reduce or eliminate one basis of the discontinuity effect: the greater distrust in intergroup relative to interindividual 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 discontinuity effect relative to unconstrained strategies. Apparently, decreases in fear do not always go hand in hand with decreases in greed. Paradoxically, this suggests that although recategorization procedures may increase trust between groups, this trust (i.e., the expectation that the opponent will cooperate) may in turn fuel greed. The idea that it is necessary to instill some degree of fear to curtail the opponent's greed is, of course, at the core of the tit-for-tat 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 (Insko et al., 1998). Although we are uncertain whether recategorization alone will prove to be effective in this respect, it is possible that an intervention that blends recategorization 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 difference 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 slender accomplishments to date. Human relations seem stubbornly set. 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 twentieth 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 (McGuire, 1998). Still, even though human relations may be as "stubbornly set" as ever, this meta-analysis underlines that great strides have been made toward understanding why intergroup relations are often more antagonistic and competitive than interindividual relations. We remain hopeful that these and other advances will some day lay the groundwork for effective interventions aimed at promoting intergroup cooperation.

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Call for Nominations: *Rehabilitation Psychology*

The APA Publications and Communications (P&C) Board has opened nominations for the editorship of ***Rehabilitation Psychology*** for the years 2006–2011. Bruce Caplan, 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
Karen 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**.