

The Extended Contact Effect: Knowledge of Cross-Group Friendships and Prejudice

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The extended contact hypothesis proposes that knowledge that an in-group member has a close relationship with an out-group member can lead to more positive intergroup attitudes. Proposed mechanisms are the in-group or out-group member serving as positive exemplars and the inclusion of the out-group member's group membership in the self. In Studies 1 and 2, respondents knowing an in-group member with an out-group friend had less negative attitudes toward that out-group, even controlling for dispositional variables and direct out-group friendships. Study 3, with constructed intergroup-conflict situations (on the robbers cave model), found reduced negative out-group attitudes after participants learned of cross-group friendships. Study 4, a minimal group experiment, showed less negative out-group attitudes for participants observing an apparent in-group-out-group friendship.

The intergroup contact hypothesis (Allport, 1954; Williams, 1947) proposes that under a given set of circumstances contact between members of different groups reduces existing negative intergroup attitudes. Some recent research (reviewed below) suggests that the effect may be most clearly associated with the specific contact of a friendship relationship. The extended contact hypothesis, which we introduce here, proposes that knowledge that an in-group member has a close relationship with an out-group member can lead to more positive intergroup attitudes. This article presents the rationale for the extended contact effect, including three mechanisms by which it may operate, and four methodologically diverse studies to demonstrate the phenomenon.

Intergroup Contact Hypothesis

The intergroup contact hypothesis has been among the most enduring theoretical perspectives in the study of intergroup relations (Amir, 1976; Miller & Brewer, 1984; Pettigrew, 1986; Stephan, 1987; Taylor & Moghaddam, 1994). Early formulations of the contact hypothesis (Allport, 1954; Williams, 1947) focused on the potential for contact between members of different groups to reduce existing negative intergroup attitudes. Even in these early statements of the hypothesis it was realized that contact in and of itself was not adequate, that societal, situational, and even personal variables could undermine or enhance the positive impact of contact (see Stephan, 1987, for a review). As evidence of the potential negative effects of contact (i.e., confirmation and strengthening of negative out-group attitudes) grew, the list of provisions and qualifications for successful contact grew with it. As this list also grew, the contact hypothesis came to resemble a "grocery list" of necessary conditions rather than a coherent model of attitude or behavior change (Pettigrew, 1986; Stephan, 1987).

However, a recent resurgence of interest in the contact hypothesis has sparked a number of advancements and raised new (and renewed) controversies (Brown, 1995; Hewstone & Brown, 1986; Mackie & Hamilton, 1993; Miller & Brewer, 1984). Three of these recent additions bear directly on the focus of this article. The first is the special importance of cross-group friendships. Several recent theoretical discussions of contact effects have focused on the role of interpersonal intimacy (Cook, 1984; Herek & Capitanio, 1996; Pettigrew, 1997). In support

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of the importance of intimacy, Pettigrew (1997), using data from a large international European sample, demonstrated that having an out-group friend predicts lower levels of both subtle and blatant prejudice, greater support for pro-out-group policies, and even generalized positive attitudes toward out-groups other than that of the friend. Similar effects were not found when the individual had an out-group coworker or neighbor (but not a friend). Pettigrew also presented analyses that used "instrument variables" suggesting that the causal direction is from friendship to lower prejudice.

The second recent addition involves a theoretical perspective on contact that emerges from the social identity approach (Hewstone & Brown, 1986). With its emphasizing the distinction between interpersonal and intergroup interactions (Brown & Turner, 1981; Tajfel, 1978), this perspective brings into focus a consistent problem for the intergroup contact hypothesis and raises a potential problem for a theory of contact based on friendship: How do the positive effects of contact with an individual out-group member generalize to attitudes about the out-group as a whole (Pettigrew, 1986)? Because interactions at the interpersonal and intergroup levels are considered to involve unique psychological processes (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987), interpersonal interactions (i.e., between individuals interacting as individuals) should have little impact on attitudes and actions toward the group as a whole (Hewstone & Brown, 1986). Although some research has shown that personalized contact can result in positive generalizations to the out-group (Marcus-Newhall, Miller, Holtz, & Brewer, 1993) and Brewer and Miller (1984) have provided a theoretical discussion of the importance of personalized contact, there is a growing consensus (see Batson et al., 1997; Brown, 1995; Fisher, 1990; Harrington & Miller, 1992; Johnston & Hewstone, 1990; Pettigrew, 1986, 1997; Rothbart & John, 1985; Wilder, 1993) that, at some point, group memberships must become salient for interpersonal interactions to affect intergroup attitudes and behavior.

The third issue arose out of work emphasizing that interactions with out-group members, especially when group memberships are highly salient, can be fraught with anxiety, discomfort, fears of appearing prejudiced or intolerant, and other negative emotions. These negative emotions increase the likelihood of self-censorship, misattribution, and stereotype confirmation (Bodenhausen, 1993; Stephan & Stephan, 1985; Wilder, 1993).

The combined impact of these three additions or controversies is a rather pessimistic outlook for intergroup contact effects. It appears that contact is likely to be most effective when the contact represents a strong affective tie and the participants' group memberships are made salient. However, the likelihood of these two conditions being simultaneously achieved is undermined by the tendency for in-group-out-group interaction to be associated with negative emotions.

Extended Contact Hypothesis

The extended contact hypothesis proposes that knowledge that an in-group member has a close relationship with an out-group member can lead to more positive intergroup attitudes. To the extent that such an effect occurs, it has very promising implications for social change, in part because it provides a

potential solution to the conundrum just described. In a cross-group friendship, group membership is more likely to be salient to an observer (who is less acquainted with the individuating features of the persons) than to participants themselves. Further, observing an in-group-out-group friendship involving others should not evoke the interaction anxiety and other negative emotions for the observer that actual participation in intergroup contact might. In addition, the extended contact hypothesis proposes a means by which widespread reductions in prejudice could occur without everyone having to have out-group friendships themselves.

Is the extended contact hypothesis plausible? We turn shortly to a discussion of mechanisms by which such an effect would operate—mechanisms that seem to make such an effect likely. However, there are at least two existing relevant studies. First, Wilner, Walkley, and Cook (1952) presented data from two large studies of White residents in integrated public housing projects. First, they found that direct intimate contact with Black residents led to more positive attitudes about Blacks in general—the direct contact effect. They also found that expectations that other Whites would approve of intimate intergroup contact affected attitudes toward Blacks. The authors claimed that these favorable expectations were in part the result of observing the intimate cross-racial interactions of others in the projects:

The women who live closer to Negroes, it appears, are not only more likely to have the more intimate types of contact with Negroes; having *more opportunity to observe other white women associating with Negroes* [italics added], they are also more likely . . . to believe that interracial activities are socially approved. (Wilner et al., 1952, p. 55)

The extended contact hypothesis also provides an alternative explanation for the findings of Hamilton and Bishop's (1976) longitudinal study of residential integration. This study compared the racial attitudes of White residents whose neighborhood was "integrated" with a single Black family to White residents whose new neighbors were White. After a year, those with Black neighbors demonstrated significantly lower anti-Black sentiment. However, the improvements in White respondents' attitudes toward Blacks was independent of the respondent's actual direct contact with Blacks. Hamilton and Bishop interpreted this finding as showing that it is the failure of the new Black neighbors to confirm previously held negative expectations that led to attitude change. However, an alternative interpretation, based on the extended contact hypothesis, is that awareness of a positive relationship between their Black neighbor and even a very few of their White neighbors accounted for the improved attitudes of the no-contact Whites.

Mechanisms

There are at least three mechanisms that we believe underlie and promote extended contact effects: positive ingroup exemplars, positive outgroup exemplars, and inclusion of other in self.

Positive In-Group Exemplar

In-group norms. One potentially important way in which the actions and apparent attitudes of the in-group partner in a

perceived cross-group friendship might influence the observer's response to the out-group arises out of the powerful influence of group norms on intergroup attitudes and behavior generally (Jetten, Spears, & Manstead, 1996; Pettigrew, 1991) and on contact effects specifically (Johnston & Hewstone, 1990). Social identity and self-categorization theorists (e.g., Abrams & Hogg, 1990; Turner, 1991) have proposed that when a self-relevant group membership is salient (i.e., the individual's social identity is salient), influence is exerted by those believed to share the relevant social identity in a process called *referent informational influence*. The in-group member influences attitudes and actions because he or she is regarded as interchangeable with the self and is able to provide information about the group's shared consensus and about the nature and content of in-group norms in the relevant social context (Haslam, McGarty, & Turner, 1996; Terry & Hogg, 1996). In other words, members of the relevant in-group can provide information about how group members understand the situation and how a group member should respond.

We propose that when the norms for interaction with the out-group are not firmly established, are ambiguous, or are in a state of change, an in-group member engaged in a close friendship with a member of the out-group should provide a salient and effective source of referent informational influence, demonstrating positive intergroup attitudes and tolerant in-group norms (Kohn & Williams, 1956; Pettigrew, 1991). This perspective is consistent with research by Schopler et al. (1993), who demonstrated that the very pervasive tendency for intergroup interactions to be more competitive than interpersonal interactions—the *discontinuity effect*—can be effectively eliminated when there are inconsistencies in the motivational and normative pressures felt by group members. These authors concluded that “such inconsistencies may create ambivalence and thus susceptibility to social influence” (Schopler et al., 1993, p. 430). Their data show that consistent influence toward cooperation by a single in-group member (combined with demonstrations of cooperation by the out-group) was the only situation that led to a high frequency of cooperative behavior. Perhaps the clearly cooperative actions demonstrated by an in-group member in a close friendship with an out-group member can provide this kind of influence and, when combined with equally salient demonstrations of cooperation by the out-group member (see our discussion of the outgroup exemplar), can lead other in-group members to pursue intergroup cooperation.

Even in intergroup contexts when norms are firmly established, an individual who violates these norms can serve to puncture the pluralistic ignorance, the adherence to a norm simply because “everyone behaves this way.” If the cross-group friendship goes unsanctioned (or, even better, results in positive outcomes for the in-group partner) the model may also serve to reduce fears of sanctions—fears that play a key role in the maintenance of norms (Cialdini, Kallgren, & Reno, 1991).

Anxiety reduction. Observing the in-group partner's actions and apparent attitudes may also reduce the observer's anxiety about the possibility of intergroup interactions. Even the anticipation of intergroup contact can be anxiety provoking (Stephan & Stephan, 1985; Stroessner & Mackie, 1993), and anxiety is associated with negative attitudes toward the out-group (Islam & Hewstone, 1993). However, comfortable interaction

demonstrated by the in-group friend may serve to reduce fears and negative expectation in the observer, leading to a more positive impression of the out-group and perhaps even to actual positive interactions with the out-group that would permit direct contact effects to operate.

Reducing ignorance. Through direct communication with the observer, the in-group member could provide information about the out-group. Ignorance of the out-group's subjective culture (Triandis, 1972) can lead to misperception, misinterpretations, misattribution, as well as fear of interacting with the out-group.

Positive Out-Group Exemplar

It seems plausible that observation of friendly behaviors of an out-group member interacting with an in-group member may serve as a basis for modification of a negative prototypic image (or stereotype) of the out-group. This effect is likely to occur when group memberships are salient and when the out-group is perceived to be relatively homogeneous. Extensive research on the perceptions of group variability has shown that in general, although the in-group is perceived to be fairly heterogeneous, the out-group is perceived to be relatively homogeneous (see Linville, Salovey, & Fischer, 1986; Mullen & Hu, 1989; Park & Judd, 1990). The concept of group variability is at the center of considerable recent research and debate (e.g., Brewer, 1993; Linville & Fischer, 1993; Simon, 1992), and the relation between person perception and group perceptions is indeed complex (Hamilton & Sherman, 1996). However, under some circumstances the out-group homogeneity effect can support greater member-to-group inferences (Nisbett, Krantz, Jepson, & Kunda, 1983; Quattrone & Jones, 1980). This is often considered with reference to negative stereotypes resulting from inferences made from negative interactions or negative information about a particular out-group member (e.g., Henderson-King, 1994). However, in the present context it could serve to enhance the effect of a positive out-group exemplar. When social identities are salient, an out-group member who is observed interacting with an in-group member may provide information about the nature of relevant intergroup relations and about the attitudes and norms of the relevant out-group (Wright, in press). Thus, when this interaction demonstrates a close friendship, the observer may conclude that the out-group feels positively toward the in-group and is interested in harmonious intergroup relations.

The effectiveness of both the positive in-group exemplar and the positive out-group exemplar mechanisms are likely dependent on the level to which group memberships are salient. Only when the individual's relevant social identity is salient will he or she recognize the cross-group friends as exemplars of their social categories, and only then will the in-group member be regarded as interchangeable with the self and the out-group member as a reflection of the out-group as a whole (Turner et al., 1987).

Including Other in the Self

This mechanism is based on recent work by Aron and his colleagues (for a review, see Aron & Aron, 1996) focusing on

ways in which, in a close relationship, *other is included in self*. Extending earlier work showing modification of self–other differences in memory and other cognitive tasks (e.g., Bower & Gilligan, 1979; Keenan & Bailett, 1980), Aron, Aron, Tudor, and Nelson (1991) demonstrated that close others function cognitively like the self in a variety of tasks including money allocations, memory for imaged words, and reaction time, whereas nonclose others do not. In a subsequent study, Aron, Aron, and Smollan (1992) showed that individuals spontaneously ascribe self–other overlap with close others and that the extent to which they do so is strongly associated with other measures of intimacy and density of social interaction.

Smith and Henry (1996), using Aron et al.'s (1991) reaction time procedure, recently demonstrated that individuals spontaneously include in-group members (but not out-group members) in the self. Furthermore, Sedikides, Olsen, and Reis (1993) found that observers treat partners in a close relationship as a single cognitive unit in a manner the authors explicitly associated with self–other overlap as described by Aron and his colleagues. Putting these three findings together leads to the following logic: In an observed in-group–out-group friendship, the in-group member is part of the self, the out-group member is part of that in-group member's self, and hence part of myself. Presuming that the out-group member's group membership is part of what one has included of that out-group member in myself, then to some extent the out-group is part of myself.

To put this another way, ordinarily, in self's conception of the world, the in-group is part of self and the out-group is not. Thus self spontaneously treats in-group members, to some extent, like self, including feeling empathy with their troubles, taking pride in their successes, generously sharing resources with them, and so forth. However, out-group members, because they are not part of self, receive none of these advantages. Literally, self could not care less about them. (Note that from this perspective the prejudice against the out-group is not so much directly negative as it is the lack of the usual positives self would feel for members of the in-group. At the extreme, out-group members are simply objects in the environment to be ignored or treated according to their usefulness for self's purposes.) We argue, however, that this changes when someone who is part of the in-group, and thus part of self, is known to have an out-group person as part of their self. In this case, the out-group friend—and hence to some extent the out-group itself—becomes part of self. The effect is that, to some extent, self begins to see members of that group as part of self. This means that self's response to members of that particular group is likely to be more positive, more like the way self would treat self. The result is that self's in-group–out-group distinctions are directly undermined, as are negative attitudes self may have held toward the out-group.

This logic is actually closely related to Heider's (1958) balance theory, if one considers there are unit relations between self and in-group, between in-group member and his or her out-group friend, and between out-group member and out-group member's group membership. In essence, this is an extension of "my friend's friend is my friend," as demonstrated by Aronson and Cope (1968), to "my group member's friend's group is my friend." Although this route is rather remote, if such links are either very salient or are numerous (if there are a number

of known examples of in-group friends having out-group friends), the effect might nevertheless be substantial.

Qualifications and Limitations

At this point, an important qualification is necessary. All the mechanisms we have considered can fall prey to *subtyping* or *refencing* (Allport, 1954). Observations of a single case that violates one's expectation about the nature of the intergroup relationships can lead to the creation of subcategories that allow attitudes about the out-group to remain unaffected by the action of the individual. Subcategorization or refencing can be applied to either partner in the cross-group friendship. When used to individuate the out-group member, this process can explain stereotype maintenance in the face of disconfirming examples (Kunda & Oleson, 1995), as well as why pleasant contact with a single out-group member does not generalize to the group as a whole (Rothbart & John, 1985; Weber & Crocker, 1983). When applied to the in-group member, his or her status as part of the group can be questioned or dismissed (an idea consistent with Marques, Yzerbyt, & Leyens', 1988, *black sheep effect*). In either case, the in-group or out-group member (or both) who violates expectations is dismissed as an exception to the rule, and no change to the general view of the out-group is necessary.

Thus it is very likely that the extended contact effect, like the direct contact effect, will be qualified by a set of conditions. However, because the present research was primarily a first attempt to establish the possibility of the phenomenon, we will not initiate the lengthy discussion of potential qualifying circumstances at this time.

The Present Research

The main purpose of these studies was to provide a multimethod demonstration of the relation between knowledge of cross-group friendships and intergroup attitudes, as predicted by the extended contact hypothesis. Consistent with the oft-stated concern for the value of method variance (e.g., Campbell & Fiske, 1959), we conducted four studies with three distinct research methods: two questionnaire studies, a laboratory constructed group conflict study, and a minimal group experiment.

Study 1, a questionnaire study, tested the predicted associations of knowledge of cross-group friendships with less prejudice. Respondents were majority group members (Whites/Anglos) responding to attitude and cross-group friendship questions about Asian Americans, African Americans, and Latinos/Latinas. Although this was a correlational study, we were able to control statistically for direct out-group friendships. In addition, we used a within-subjects design (comparing each person's friendships and prejudice across different ethnic out-groups), which automatically controls for general dispositional prejudice, dispositional friendliness, or their association. Study 2, a second questionnaire study, cross-validated the results of Study 1 with another White/Anglo sample and replicated the results of Study 1 with a minority sample.

Study 3, the laboratory constructed group conflict study, focused on the predicted causal direction from knowledge of the cross-group friendship to reduced prejudice. This study used a

procedure based on the robbers cave studies (Sherif, Harvey, White, Hood, & Sherif, 1961). Over a 1-day period, we first created strong intergroup conflict between individuals randomly assigned to one of two 7-person groups. Late in the day, two pairs (each pair consisted of 1 participant from each group) were excused to take part in what they believed was another study, in which they went through a procedure designed to create strong interpersonal closeness (Aron, Melinat, Aron, Vallone, & Bator, 1997). The groups were reassembled and participated in an in-group meeting and a final intergroup competitive task. We predicted reduced negative attitudes toward the out-group following learning about in-group members having formed out-group friendships. This entire procedure was repeated, with different participants, in four sessions.

Study 4 used a modified minimal group paradigm (Tajfel, Billig, Bundy, & Flament, 1971) to create a fully experimental test of the extended contact hypothesis. Participants believed they were in one of two groups based on a series of estimation tasks. They then observed a member of their in-group and the out-group (confederates) interacting. By using verbal and non-verbal cues, the two confederates represented their existing relationship as that of close friends, neutral strangers, or disliked acquaintances. We predicted less negative out-group attitudes in the observed close-friend interaction condition.

Study 1

As noted above, this was a questionnaire study that tested the predicted associations of knowledge of cross-group friendships with less prejudice. This initial study focused on Whites/Anglos (the majority group in our samples) because majority prejudice toward minorities has been the traditional emphasis in intergroup research in general and in contact hypothesis studies in particular. The study included items targeting three ethnic minority out-groups, permitting within-subject comparisons investigating whether the respondent reported less prejudice toward those groups in which the respondent knew of a cross-ethnic friendship. This within-subject procedure automatically holds constant dispositional variables, such as overall tendency to have friends in out-groups and overall tendency to be prejudiced toward out-groups. If not controlled for, these might provide third-variable alternative explanations for any obtained correlations.

On the basis of the extended contact hypothesis we made the following three predictions: (a) Respondents who perceive an in-group member to have a friend in a particular out-group will express less prejudice toward that out-group; (b) the greater the number of in-group members perceived by a respondent to have a friend in a particular out-group, the lower his or her prejudice toward that out-group; (c) the greater the perceived degree of overlap of selves between the in-group member and his or her out-group friend, the lower the prejudice expressed toward that out-group.

Method

Respondents. Questionnaires were completed by 125 students, including 84 Whites/Anglos, during a regular class session of a personality course at the State University of New York at Stony Brook; nearly everyone present volunteered to participate. Because of the very limited

sample of each minority group and our primary interest in majority respondents' attitudes, data from the 41 respondents from other ethnic groups were not analyzed. The university at which the study was conducted is moderately diverse over the ethnic groups of African American, Latino/Latina, and Asian American; therefore it was likely that many Whites/Anglos would know of at least some in-group members who had formed friendships with members of each of these groups.

Questionnaire. The questionnaire included a separate page for each ethnic group, in the following order: Asian Americans, African Americans, Latinos/Latinas, and Whites/Anglos. Respondents were instructed to skip the page for their own ethnic group (our measure of own ethnicity was which page was skipped). Each page consisted of several parts. The Affective Prejudice Scale assessed "whether you have ever felt the following ways about [ethnic group]" by using a 7-point scale that ranged from 1 (*never*) to 7 (*often*) for "felt sympathy" and "felt admiration." The two-item scale (sympathy and admiration) had an alpha of .71. This measure is part of a Subtle Prejudice Scale developed by Pettigrew and Meertens (1995). Inspired by Dovidio, Mann, and Gaertner's (1989) findings that the lack of positive emotion, rather than negative emotions, best predicts subtle prejudice, this scale is intended to provide a measure of emotional response to the target out-group. Following Pettigrew and Meertens' research, the Affective Prejudice Scale is reverse-scored such that higher values indicate greater prejudice. In addition, the General Evaluation Scale asked respondents to "describe how you feel about [ethnic group] in general" by using the following bipolar adjective pairs separated by a 7-point scale: warm-cold, negative-positive, friendly-hostile, suspicious-trusting, respect-contempt, admiration-disgust. (In all cases, results were scored such that the more positive adjective received the higher score.) This six-item semantic differential scale had an alpha of .90. Semantic differentials and other forms of trait-characteristics judgments are commonly used as measures of intergroup attitude and, in this case, were intended to provide a general positive-negative evaluation of the out-group. As noted earlier, the items on the General Evaluation Scale were scored such that higher scores indicated a more positive evaluation. The Affective Prejudice and General Evaluation Scales correlated $-.31$. Finally, as a summary indicator of overall prejudice, we computed the average of the two scales (reversing scores on the General Evaluation Scale).

The questions on friendships for each target ethnic group first asked about the number of the respondent's own friendships in the target ethnic group. Respondents who had at least one such friend also completed Aron et al.'s (1992) Inclusion of Other in the Self (IOS) Scale for the respondent's relationship with this person. The IOS Scale is a series of seven pairs of overlapping circles of increasing degrees of overlap. (Aron et al. found this scale to have high levels of test-retest and alternative form reliability, to correlate strongly with a variety of longer measures of closeness, and to predict relationship stability as well as or better than these longer measures.) The respondents select the pair of overlapping circles that best describe their relationship with the out-group member.

Next, respondents indicated the number of members of their own ethnic group that they know who have friends in the target ethnic group. If the respondent knew at least one in-group member with a friend in the target ethnic group, the respondent was to select the in-group person with whom he or she had the closest relationship and complete the IOS Scale describing the closeness of that person's friendship with the out-group member.

The questionnaire also included a number of personality items to cloud to some extent the true purpose of the study.

Results

Analysis strategy. In all analyses, we used a within-subjects (repeated measures) strategy in which the independent variable

was a measure of perceived cross-group friendships involving the target out-group, and the dependent variable was a measure of attitude toward that out-group. These analyses yielded within-subject correlations. For example, a positive within-subject correlation between X and Y means that when a respondent scores particularly high on X for one ethnic group compared to the way that respondent scores for the other ethnic groups, that respondent is also likely to score high on Y for that ethnic group compared to the way that respondent scores for Y on other ethnic groups. Thus, how the respondent scores in general (across all out-groups) on X or Y , when compared to other respondents, is eliminated from the analysis. In this sense, individual differences in overall tendencies on X or Y are removed from the analysis in the same sense that between-subjects variance is removed from any other within-subjects design. In the present case, the variables of interest were knowledge of cross-group friendships (and the number and closeness of the relationship) and prejudice, variables on which overall individual differences in tendencies have traditionally served to create ambiguities in interpreting correlational results in contact hypothesis studies. Thus the present approach undermined such alternative explanations.

The within-subject aspect was carried out by following procedures described in Cohen and Cohen (1983, chapter 11). The responses for each target ethnic group were treated as a separate respondent (creating three times the number of respondents as actually existed); however, the effect of nonindependence of scores was then removed by partialing the between-subjects differences out of the analysis. This was done by partialing out a set of 83 dichotomous dummy variables representing the 84 respondents. (This is actually the precise way in which the regression equivalent of a repeated measures analysis of variance is conducted.) This approach has long been used in social psychology (e.g., Cutrona, 1986), particularly in the study of relationships (e.g., Aron et al., 1992; Rusbult, Verette, Whitney, Slovick, & Lipkus, 1991).¹

It should be noted, however, that even with this type of analysis it is still possible that either X could be causing Y or Y causing X in the sense that it could be that an individual's tendency to score higher on X for a particular target ethnic group compared to other target groups might be the cause of scoring high on Y for that target group compared to other ethnic groups, or vice versa. It is only general (across all ethnic groups) tendencies on X and Y that are controlled with the within-subject design and analysis.

To further reduce the ambiguity of results of this correlational study, we conducted, for each analysis, an analysis in which a parallel measure of the respondent's own, direct friendships with the target out-group served as a control variable. Thus, when testing whether there was a link between prejudice and knowing an in-group person with a friend in a particular out-group, we conducted a parallel analysis controlling for one's own friendships with members of that out-group. (Similarly, for analyses involving number of in-group persons known with friends in a target out-group, we controlled for number of own friends in that out-group; for analyses involving closeness of in-group person to his or her friend in a target out-group, we controlled for closeness to own friend in that out-group.) If correlations between extended contact and prejudice remain

after removing parallel direct-contact variables, this would seem to dramatically undermine a direction of causality from prejudice to extended contact. For example, if prejudice was affecting whether or not one knows in-group members with a friend in a particular out-group, it seems that prejudice would even more strongly affect whether or not one has one's own friend in that target out-group.

In sum, the present study, involving real-life attitudes and social relations involving real-life out-groups, was necessarily correlational. However, the design we used (ratings by each respondent of several target out-groups and including measures of direct contact as control variables) permitted us to conduct analyses that minimized the most obvious alternative explanations to findings appearing to support the extended contact hypothesis.

Prediction 1: Knowledge of an in-group-out-group friendship will be associated with less prejudice. As shown by the regression adjusted means in the top section of Table 1, respondents who knew an in-group member with a friend in the target out-group showed significantly less affective prejudice and less overall prejudice toward that target out-group. These differences remained significant and of about the same magnitude even after partialing out whether respondents had their own friends in that out-group. The difference for the General Evaluation Scale was not significant.

Prediction 2: The greater the number of in-group-out-group friendships known, the lower the prejudice. This analysis applied only to respondents who knew of, and to target out-groups in which there was, at least one cross-group friendship. Also, because number of in-group members known to have out-group friends was highly positively skewed, we applied a log transformation to this variable prior to analysis. (We made the same transformation for number of own direct friendships with the target out-group.) As shown by the correlations in the second section of Table 1, the more in-group members known to have friends in a target out-group, the lower the reported affective prejudice and overall prejudice. These correlations remained significant and of about the same magnitude even after partialing out the number of respondent's own friends in that out-group. The predicted pattern was also found for the General Evaluation Scale, but was significant only before partialing out the number of own direct friendships with the target out-group.

Prediction 3: The greater the perceived overlap of selves in the in-group-out-group friendship, the lower the prejudice. This analysis also applied only to respondents who knew of, and to target outgroups in which there was, at least one cross-group friendship. As shown by the correlations in the bottom section of Table 1, the greater the perceived overlap of the in-

¹ It has recently been recognized (Kenny, Kashy, & Bolger, in press) that, strictly speaking, significance tests using this approach are accurate only when there is no interaction between the between-subject variance and the predictor variable. That is, there should not be significant variation in the within-subject correlations across respondents. In the present study (and also in Study 2), there were no significant interactions of this kind in any of the analyses involving the key combined prejudice measure. Among the other 12 analyses, there was one case in which the interaction just barely attained significance—about what one would expect by chance. Thus we felt justified in using the traditional approach.

Table 1
Means and Within-Subjects Correlations for Three Predictions in Study 1

Scales	Overall differences ^a			Direct friendships controlled ^b		
	Affective prejudice	General evaluation	Overall prejudice	Affective prejudice	General evaluation	Overall prejudice
Prediction 1 ^c						
Friendship known						
No	5.06	3.79	4.13	5.01	3.88	4.08
Yes	4.09	4.07	3.51	4.10	4.06	3.52
<i>r</i>	-.20**	.08	-.21**	-.18**	.05	-.17*
Prediction 2 ^d						
<i>r</i>	-.27**	.21**	-.30	-.18*	.10	-.18*
Prediction 3 ^e						
<i>r</i>	-.18*	.28***	-.30***	-.18*	.25**	-.28***

Note. Values for yes and no in Prediction 1 are means.

^a For overall differences, within-subject correlations are reported.

^b For direct friendship controlled, within-subject partial correlations are reported.

^c Prediction 1: Knowledge of an in-group-out-group friendship will be associated with lower prejudice.

^d Prediction 2: The greater the number of in-group-out-group friendships known, the lower the prejudice.

^e Prediction 3: The greater the perceived overlap of selves (score on the Inclusion of Other in Self Scale) in the in-group-out-group friendship, the lower the prejudice.

* $p < .05$. ** $p < .01$. *** $p < .001$.

group member's self with that of his or her out-group friend, the less affective prejudice, the more positive the general evaluation, and the less overall prejudice toward that out-group. All three results remained significant and of about the same magnitude after partialing out overlap of self with self's closest out-group friend.

Discussion

Overall, these data confirm the three predictions and provide the first direct evidence in support of the extended contact hypothesis. The within-subjects design rules out as explanations for these findings any general dispositional tendencies to make friends (or to make friends with out-groups), general dispositional tendencies toward prejudice, or any links between these dispositional tendencies. That the findings remained significant even after controlling for direct friendships with the out-group is particularly important, because it makes it somewhat less likely that the association of extended contact and less prejudice was due to the causal direction of less prejudice to extended contact. This causal direction seems less likely in light of the partial correlation findings because it would be reasonable that being less prejudiced toward an out-group would show up most directly in one's being more likely to have friends in that out-group—and any such tendency was partialled out of this analysis.

That several of the results were significant for the Affective Prejudice Scale but not for the General Evaluation Scale is consistent with Pettigrew's (1997) work on the direct effects of out-group friendships. Pettigrew found that out-group friendship was most associated with lower levels of affective prejudice. More generally, research by Dovidio and his colleagues (see

Dovidio & Gaertner, 1993) has shown that Whites in the United States, in an effort not to appear prejudiced and to confirm their own egalitarian perceptions of themselves, will respond in a nonprejudiced fashion in response to negative items but will be less nonprejudiced on positive affect items.

Overall, Study 1 provides initial support for the extended contact hypothesis. It demonstrated the predicted association and did so using a methodology that held constant individual differences and controlled for the respondent's own direct intergroup friendships. However, there are at least three major limitations to Study 1. First, although most results were unambiguously significant, the failure to find many significant effects on the General Evaluation Scale may still leave some doubt about the reliability of the overall pattern of associations. Second, the present findings were limited to White/Anglo respondents. Third, in spite of strong design and statistical controls for many of the most plausible third-variable explanations, the study was fundamentally correlational and could not unambiguously sort out direction of causality. Study 2 addressed the first two limitations; Studies 3 and 4 addressed the issue of causality.

Study 2

Study 2, our second questionnaire study, attempted to cross-validate the results of Study 1 with another sample of White/Anglo respondents and to extend the Study 1 findings by attempting to replicate them with a minority sample.

Method

Respondents completed questionnaires as part of a special mass testing session for introductory psychology students (for which they received

research credit for their course) at the State University of New York at Stony Brook. Our questionnaire was completed by 252 respondents, including 57 Asian Americans, 28 African Americans, 22 Latinos/Latinas, 132 Whites/Anglos, and 13 others. The questionnaire was the same as that used in Study 1, except it contained some different filler items.

Results

All analyses were conducted by following the same procedures that we used in Study 1. However, we were unable to test Prediction 1 regarding the dichotomous difference between knowing and not knowing about an in-group-out-group friendship with a particular out-group because nearly all respondents in this study knew at least one in-group member with a friend in the target out-group.

Cross-validation of Study 1: White/Anglo respondents. As shown in Table 2 (top half), results were all in the predicted directions (the same directions as the results of Study 1), with significant differences for the Affective Prejudice Scale and the Overall Prejudice Scale, as in Study 1.

Extension of Study 1: Ethnic minority group respondents. None of the analyses were qualified by an interaction with respondent's own particular ethnic group. Therefore, these analyses combined respondents from all three of the ethnic minority groups. Nevertheless, to be conservative, respondent's own ethnicity was included as a covariate in all analyses. As shown in Table 2 (bottom half), results for minority group respondents were similar to those for Whites/Anglos. Indeed, for ethnic minority group respondents, effects for the General Evaluation Scale were also significant.

Discussion

Study 2 largely cross-validated the findings from Study 1 with Whites/Anglos and extended those findings to an ethnic

minority sample. The replication of the basic pattern with minority group respondents is of particular interest. Most research on intergroup relations and on intergroup contact in particular, when it has involved real groups, has focused primarily on majority prejudice toward minority groups. This is because majority prejudice to minority groups has been the major practical concern (and also the most convenient because majority group respondents are more readily available to most researchers). Nevertheless, it is clearly important for both practical and theoretical reasons to examine intergroup phenomena from both the majority and minority perspective (see Lalonde & Cameron, 1994; see also Ellemers, 1993, and Turner & Brown, 1978, for discussions of the role of group status in intergroup relations).

Overall, Studies 1 and 2 together lend clear initial support to the positive effects of extended contact on out-group prejudice. The predicted patterns were found for the difference between groups for which respondents knew of a cross-group friendship versus groups for which they did not (Study 1), for the number of such friendships known about (Studies 1 and 2), and for the perceived overlap of selves of those friendships (Studies 1 and 2). The predicted patterns remained even after partialing out direct contact effects. Finally, the predicted patterns were consistent for both majority and minority group members.

Still, as noted earlier, there is at least one major limitation to the results of Studies 1 and 2. Like all correlational research, the direction of causality remains ambiguous. We argued in our discussion of Study 1 that some otherwise plausible third-variable explanations are undermined by holding constant individual differences and that reverse direction of causality explanations are somewhat undermined by the fact that the basic finding held up even after partialing out respondents' own direct friendships. Nevertheless, a more convincing case for a causal interpretation requires an experimental manipulation of the independent variable. Hence, Studies 3 and 4.

Table 2
Means and Within-Subjects Correlations for Two Predictions for White/Anglo and Minority Respondents in Study 2

Scales	Overall differences ^a			Direct friendships controlled ^b		
	Affective prejudice	General evaluation	Overall prejudice	Affective prejudice	General evaluation	Overall prejudice
White/Anglo respondents						
Prediction 1 ^c						
<i>r</i>	-.20**	.15*	-.22***	-.10*	.03	-.08
Prediction 2 ^d						
<i>r</i>	-.24***	.08	-.21***	-.22*	-.02	-.15*
Minority respondents						
Prediction 1 ^c						
<i>r</i>	-.26***	.30***	-.34***	.08	.15*	-.13*
Prediction 2 ^d						
<i>r</i>	-.23***	.32***	-.32***	-.15*	.18**	-.20**

^a For overall differences, within-subject correlations are reported.

^b For direct friendship controlled, within-subject partial correlations are reported.

^c Prediction 1: The greater the number of in-group-out-group friendships known, the lower the prejudice.

^d Prediction 2: The greater the perceived overlap of selves (score on the Inclusion of Other in Self Scale) in the in-group-out-group friendship, the lower the prejudice.

* $p < .05$. ** $p < .01$. *** $p < .001$.

Study 3

This study focused on the predicted causal direction from knowledge of the in-group–out-group friendship to reduced prejudice. This study used a laboratory constructed intergroup conflict procedure, inspired by the robbers cave studies (Sherif et al., 1961) and the series of similarly constructed experiments with adults by Blake and Mouton (see Blake, Shepard, & Mouton, 1964, for a review). We actually conducted four such studies, each involving the construction of an intergroup conflict, over a 1-day period, between two interacting 7-person groups. We first introduced manipulations designed to induce strong intergroup conflict and then systematically introduced an intervention involving the creation of cross-group friendships for a small subset of the group members. Measures of intergroup differentiation, intergroup bias, and participants' evaluations of the intergroup relations were taken at regular intervals throughout the study.

The value of this procedure was that it created high levels of experimental realism in a controlled setting. However, studies of this kind involving extensive construction of intergroup conflict pose difficulties for formal statistical hypothesis testing (only very minimal statistical tests were conducted in Sherif et al.'s 1961, robbers cave studies). The problem is that there are usually too few individuals within a single experimental session and it is inappropriate to combine individuals across sessions because scores from within each are nonindependent. (To put this another way, participants are nested within session.) Thus, the only entirely safe unit of analysis for any dependent variable is the mean of the entire set of participants taking part in a particular session. The practical constraints of conducting each session also mean that there are likely to be relatively few sessions. In the present study, four sessions were conducted, making the effective sample size only 4 (that is, $df = 3$). This makes statistical comparisons very difficult because of the extremely low power of the test. In addition, it is impractically costly to conduct a series of control sessions in which there are no interventions. Thus, we must rely on a quasi-experimental time-series design comparing levels of the dependent variable before and after the intervention. Given these constraints, and following the methods of Sherif et al., the data are presented with only descriptive statistics.

The key prediction, based on the extended contact hypothesis, was that measures taken after the friendship intervention would show more positive evaluations of the intergroup relations, less negative intergroup differentiation, and less in-group bias than the measures taken in the period just before the intervention. Of course, consistent with the extended contact hypothesis, the data presented here exclude the participants who actually formed the cross-group friendships.

Method

Four 1-day sessions were conducted. Each session included 12 to 14 participants divided into two groups. The phases of the experiment are outlined in Table 3. In Phase 1, in-group familiarity, solidarity, and liking were created. In Phases 2 and 3, intergroup competition tasks were used to create rivalry between the two groups. In Phase 4 (the intervention phase), feelings of friendship were developed between two members of

Table 3
Schedule/Phases of Study 3

Phase	Duration (min)
1: In-group solidarity building	60
2: Intergroup competition	90
Snack	20
First preintervention testing	10
3: Intergroup competition	90
Lunch	40
Second preintervention testing	10
4: Intervention/friendship formation	45
Group meeting (Describing friendship)	10
5: Final intergroup competition	90
Postintervention testing	10
Debriefing	30

the rival groups. Phase 5 involved a final group competition task. Dependent measures were obtained following Phases 2, 3, and 5.

Participants. Participants were 53 undergraduates (37 women and 16 men). All were enrolled in the same large psychology course at the University of California, Santa Cruz, and received course credit for research participation.

Pretest. A pretest, administered 1 week before the beginning of the experiment, served three purposes: (a) It included a number of measures of attitudes, values, and personality traits that were ostensibly to be used to create groups of people who were similar in these dimensions; (b) the emphasis on personality traits and attitudes misled participants about the true purpose of the study; and (c) it provided information about preexisting friendships among potential participants (pairs of participants, either of whom checked knowing the other even slightly on a list of all students in the course, were assigned to different sessions or, where this was not possible, were assigned to the same group in a given session).

Phase 1: Creating in-group solidarity. On arrival, participants were informed that they had been assigned to either the blue or the green group and were immediately escorted to their appropriate group meeting room. Each group was informed that they had been matched on the basis of similar responses on the pretest questionnaire, and participants were then issued a green or blue "team T-shirt," which they then wore throughout the day. For the next hour, the two groups worked in their separate group meeting rooms on a series of tasks designed to create in-group familiarity and build feelings of solidarity and liking. For example, the group played "ice-breaker" introduction games, designed a team logo and team name, and did several simple cooperative problem-solving tasks.

Phases 2 and 3: Creating intergroup rivalry. Phases 2 and 3 took 90 min each. During these phases, the groups engaged in a series of intergroup competitive tasks involving analytical and creative skills (e.g., designing a visual presentation encouraging young people to practice safe sex and writing a solution to a psychological case study). In addition, the groups competed in several physical problem-solving games (e.g., untangling while blindfolded a badly tangled rope that was tied to each person's wrist and balancing their entire group on a small platform).

Following the analytical and creative tasks, opposing teams were encouraged to provide a critical evaluation of the other team's work that was ostensibly used to assist the judges in their decision about a winner. These critiques were surreptitiously edited by the experimenters to leave only negative statements and were then given to the target groups.

At the end of each of these two phases, participants from both groups were brought together for lunch (after Phase 3) and a coffee break (after Phase 2). At these breaks, the groups were seated at separate tables on

either side of a large common room, and the "winning" team for the preceding activity was given rewards, such as candy bars or pastries (each team "won" once; order of victory was determined randomly). Participants then returned to their group meeting rooms, were separated into individual cubicles, and completed questionnaires assessing the dependent variables.

Phase 4: Friendship formation. In this 90-min phase, 2 participants were randomly selected from each group, and each was paired with a member of the other group for the friendship-making procedure. During this time, other group members were separated in individual cubicles and engaged in a filler task: personality questionnaires unrelated to the activities of the study. Using a cover story, including a second professor familiar to many of the participants as a relationships researcher (Arthur Aron), we led participants to believe that this closeness-building procedure was part of a separate experiment. They were told that only 2 participants were taken from each team to create as little disruption in the ongoing study as possible and that this particular time was chosen because it did not involve any group activities. The experimenters in the main study appeared surprised and indicated that they had no knowledge of this second study. This deception also allowed the new experimenter to provide all participants with information about the nature of the closeness-building procedure. The closeness-building task itself was a procedure developed by Aron et al. (1997) that has been shown to create high levels of interpersonal closeness in stranger dyads over a fairly short period of time. The procedure involves a series of gradually escalating mutual self-disclosure and relationship-building tasks.

After completing the closeness-building task, the 2 members from each team were reunited with their groups. The experimenter, "in order to bring everyone up to date," then asked 1 member of the group to describe the surveys they had just completed and encouraged the participants in the closeness-building task to describe their experience. (We hoped that the wording of the experimenter's instructions here would not serve to provide authority support for intergroup contact [Allport, 1954], although this possibility cannot be entirely ruled out.) All experimenters reported later that, in all cases, participants described having engaged in tasks designed to make them feel close to their out-group partner.

Phase 5: Final group competition. In the final phase, group competition was reestablished with another competitive task. Then, before full debriefing, participants completed the final administration of the dependent measures.

Dependent measures. Participants completed three different measures, each administered three times, once after each of the competition sessions (Phases 2, 3, and 5). The first measure assessed the perceived quality of the intergroup relations. This measure was an eight-item semantic differential scale (e.g., friendly–hostile, warm–cold; alphas for the three testings = .94, .85, .93). The second measure asked participants to evaluate both the in-group and the out-group by using another eight-item semantic differential scale (e.g., tolerant–intolerant, intelligent–stupid). A participant's evaluation of the out-group was subtracted from his or her evaluation of the in-group to produce a measure of intergroup differentiation (alphas for the three testings = .61, .75, .72). The third scale measured intergroup bias in resource allocation. In a single question, participants were asked to divide \$500 between the in-group and the out-group. The difference between the amount allocated to the two groups represented the magnitude of the intergroup bias in allocation.

Across the three testings in each session, the three dependent measures were correlated as follows: perceived quality of the intergroup relations and intergroup differentiation, $-.25$, $-.46$, $-.49$; perceived quality of the intergroup relations and intergroup bias in allocation, $-.36$, $-.25$, $-.26$; and intergroup differentiation and intergroup bias in allocation, $.21$, $.29$, $.42$.

Results

Table 4 shows the means for the three dependent variables for each session over the three test occasions. (Consistent with

Table 4
Means and Standard Deviations for Each of Three Dependent Variables for Each of Four Sessions

Session	After Phase 2 (preintervention)		After Phase 3 (preintervention)		After Phase 5 (postintervention)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Differential evaluation of in-group versus out-group ^a						
A	0.60	0.81	1.00	1.08	0.78	1.01
B	0.37	0.66	0.23	0.89	0.47	0.60
C	0.38	0.56	0.50	0.87	0.26	0.55
D	0.71	0.52	0.85	0.88	0.59	0.61
Intergroup bias ^b						
A	179.80	109.42	207.80	115.14	140.00	107.76
B	112.50	110.01	112.50	110.02	75.00	115.25
C	55.56	99.67	55.56	99.67	55.56	99.67
D	10.00	61.63	80.00	95.12	30.00	94.86
Quality of intergroup relations ^c						
A	4.34	0.88	3.60	0.85	5.20	0.76
B	4.70	0.82	4.78	1.16	5.16	1.05
C	5.42	0.73	5.35	1.09	6.11	0.73
D	5.11	0.64	4.57	1.21	4.86	1.16

^a In-group and out-group were each evaluated on an eight-item semantic differential scale. Scores could range from 1 to 7. Values reported are difference scores.

^b Intergroup bias was measured by the difference in the allocation of \$500 between the in-group and out-group.

^c Quality of intergroup relations was evaluated on an eight-item semantic differential scale. Scores on this scale can range from 1 to 7.

our emphasis on awareness of other people's cross-group friendships, all analyses were based only on the participants who were not themselves participants in the friendship-building procedure.) Figure 1 presents the overall pattern with the grand means (averaged across the four sessions) for each variable over the three test occasions.

The pattern of results is clear and consistent. In all four sessions, the postintervention scores show a more positive view of the intergroup relations compared to the period just before the intervention (Phase 3). In addition, in three of the four sessions, the participants also indicated a reduction of in-group favoritism in money allocation and lower in-group favoritism in the evaluation of the two groups following the intervention. In many cases, the postintervention scores indicated better attitudes toward the out-group and a more positive assessment of the intergroup relations than were reported in the first measurement occasion (Phase 2). Figure 1 shows more clearly that the results on all three measures are unambiguously in the hypothesized direction.²

² As noted earlier, given the small number of sessions, following the method of Sherif et al. (1961), we have simply provided descriptive data. Nevertheless, we did conduct two types of analyses to provide some statistical evaluation of our interpretation of the descriptive data. The first analysis used session as the unit of analysis ($n = 4$), and was, therefore, well protected from Type I error. However, it is probably overly conservative because the participants' scores within a session are certainly far from being perfectly nonindependent. This analysis was a 2

Discussion

The data from this constructed group conflict study provide further support for the extended contact hypothesis and specifically support a causal direction from extended contact to reduced negative attitudes. The introduction of a cross-group friendship was followed by a positive change in the rivalry-induced, in-group favoritism in evaluation, in negative intergroup behavior (bias in resource allocation), and in participants' perceptions of the relationship between the groups. Thus the present data involving constructed groups and a systematic intervention to produce a perceived cross-group friendship lend support to a causal interpretation of the correlational findings from Studies 1 and 2.

The present findings are nevertheless limited in at least two major respects. First, they involve a very small effective sample size for purposes of the statistical analysis. Second, the design was only quasi-experimental (a time-series design), because there was no randomly assigned control group that did not get the intervention. (As had Sherif and his colleagues, we considered it impractically costly to conduct control sessions in which there were no interventions.) Thus it is possible that the obtained effects could have been due to time-related factors (or factors associated with repeated testing) independent of the experimental intervention. Study 4 was designed to test the causal direction of the extended contact hypothesis by using a truly experimental design.

Study 4

Study 4 was a fully experimental test of the extended contact hypothesis that used laboratory created groups in a modified

(time) \times 3 (dependent measures) repeated measures analysis of variance (ANOVA). The two levels of the time variable were Phase 3 (immediately preintervention) and Phase 5 (postintervention); the three levels of the dependent-measures variable were in-group favoritism in evaluation, in-group bias in money allocation, and evaluation of intergroup relations (scores reversed). Thus the main effect for the time variable tested the change (over all three variables) from before to after the intervention. Because this F ratio has a single degree of freedom in the numerator and because we had a clear prediction about the direction of this difference, we have converted the F ratio to a t (i.e., taken the square root) and used a one-tailed test of significance. The resulting main effect of time was statistically significant, $t(3) = 2.74$, $p < .05$; effect size (partial r) = .83. In our second approach to analyzing these data, we first conducted the same type of 2×3 repeated measures analysis four times, once for each session, with the number of participants in that session as the unit of analysis. After excluding those who took part in the friendship-building procedure, there were 8 to 10 participants included in each such analysis. (Before conducting each of these four analyses, we checked for main or interaction effects involving whether participants were in the green or blue group; none were significant.) Then, treating each session as an independent study, we combined across the four results by using standard meta-analysis procedures (Rosenthal, 1984). The result was clearly significant, $Z = 2.69$, $p < .01$; effect size (adjusted r) = .54. It should be noted that both of these analyses may be conservative in the additional sense that they assessed change from Phase 3 to Phase 5, ignoring the increasing trend from Phase 2 to Phase 3. Results would be stronger still (including being clearly significant in both analyses) if one were to take as the baseline for comparing the Phase 5 results not the Phase 3 scores but the Phase 5 scores expected on the basis of the trend from Phase 2 to Phase 3.

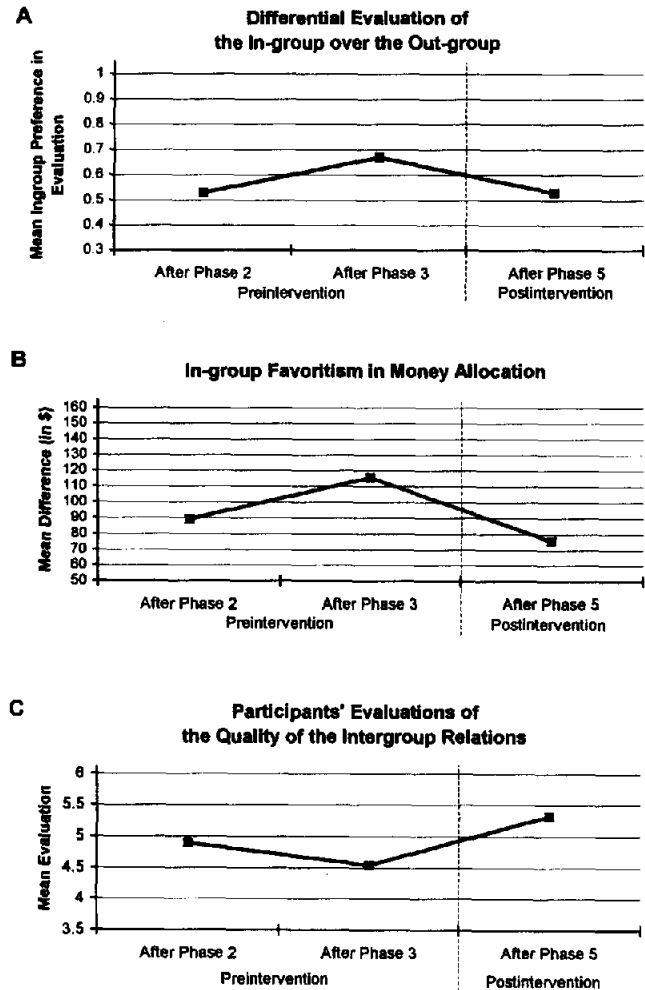


Figure 1. Mean scores on each of three dependent measures at each of two preintervention and one postintervention testing, averaged across four experimental sessions in Study 3.

minimal group paradigm (Tajfel et al., 1971). Numerous experiments that have used this paradigm have shown that mere categorization of individuals can lead to discrimination in favor of the in-group on attitude measures, evaluations, and the allocation of resources (for reviews, see Brewer, 1979; Mullen, Brown, & Smith, 1992). Consistent with typical procedures in this kind of research, participants were led to believe that they were divided into groups on the basis of their performance on an estimation task. We then arranged for participants to observe an in-group and an out-group member (actually confederates) interacting as they solved a puzzle task. Their relationship appeared to observers as one of close friends, strangers, or disliked acquaintances (e.g., in the close friends condition, when the in-group and out-group members met, each expressed delight at seeing an old friend, and they hugged). Participants then completed various measures of in-group-out-group bias. Our prediction, based on the extended contact hypothesis, was that there would be less in-group-out-group bias following exposure to

the close friends pair than following the exposure to the stranger or disliked acquaintance pair.

Method

Participants. A total of 178 undergraduates at the University of California, Santa Cruz participated in partial fulfillment of a psychology course requirement. Eight participants were dropped from the sample; 2 knew one of the confederates, and 6 indicated suspicions about the purpose of the study on an open-ended prebriefing question. The final sample included 59 men and 111 women.

Procedure. Participants signed up for an experiment entitled "Personality Assessment." On arrival, each participant was immediately escorted to an individual observation cubicle. Participants were tested in groups of 3 to 6. However, because all work was done in separate cubicles and the experimenters' behavior was identical in all sessions, participants did not know how many other participants were present. (When their own blind was raised, each participant could see three one-way mirrors with the blinds up across the room.) The laboratory consisted of six cubicles surrounding a larger room. Each cubicle was equipped with a one-way mirror that allowed participants to see into the larger room and an intercom system that allowed them to hear the activities in that larger room. When all participants had arrived, the experimenter raised the blinds on the one-way mirrors, turned on the intercom, and proceeded to describe the details of the study. Participants were told they would be given several personality tests that, although very simple to administer, had been shown in past research to distinguish two unique personality types. They were led to believe that the purpose of the study was to replicate these previous findings and to determine if untrained laypeople could correctly identify the differences between the two personality groups. The experimenter then read vague profiles describing the traits of the two personality groups and indicated that for the purposes of this study the two groups would be arbitrarily designated as *blue* and *green*.

The participants then engaged in three quantity estimation tasks. While their answers were ostensibly being scored, participants completed a personality scale (to fill time and to further substantiate the study's interest in assessing personality differences). Each participant was then designated as a member of the blue or green group, supposedly on the basis of their quantity estimation scores (actually randomly), and given a correspondingly colored T-shirt to wear.

After distributing the T-shirts, the experimenter returned to the larger room and explained that 2 participants, 1 blue and 1 green, would be randomly selected to participate in the first problem-solving task. These 2 would be called into the large room. Those not selected would serve as observers and would rate the two individuals and their associated groups on a variety of measures. The experimenter then randomly selected a participant number from each group, and shortly thereafter the two confederates (supposedly the participants whose numbers had been selected) entered the central room. Their interaction created the experimental manipulation.

Confederates. Two pairs of female confederates were used. To avoid confounding our experimentally created groups with real-world groups, one pair consisted of confederates who were both members of an ethnic minority (Asian American), and the other pair consisted of ethnic majority group members (White/Anglo). All four confederates received extensive training and practice in the scripts for each of three experimental conditions. Included in this training were numerous observations of the other pair to ensure standardization across the two pairs. The confederate pairs and the color of T-shirt worn by each confederate were both counterbalanced across conditions.

Experimental manipulations. In all three conditions, the confederates entered the room separately and were told to sit at the table in the middle of the room (all participants could see this table clearly through

their one-way mirror). The task was a three-dimensional wooden puzzle and was described to the confederates. Before the confederates began working, the observing participants were given final instructions on the use of their rating scales. The confederates were given 5 min to complete the puzzle, which was always left uncompleted. After 5 min, the confederates were thanked and told to return to their own cubicles.

The confederates' scripts were standardized across the three conditions. The manipulation included modifications in the initial verbal greeting but was primarily contained in the nonverbal messages provided by the confederates. In the friend (close friend) condition, on entering the room, the two confederates showed that they recognized each other and reacted positively. One said, "I didn't know you were going to be here!" They smiled and greeted with an affectionate hug. During the puzzle-solving task, the pair used nonverbal demonstrations of friendship: They talked in a friendly, upbeat tone, leaned toward each other (reducing interpersonal space), smiled and nodded, and touched each other on the arm. When leaving the room, confederates walked close together, smiled at each other, and appeared to be talking about the task. In the neutral (neutral stranger) condition, on entering the room, the two confederates appeared not to recognize each other and greeted each other with a polite "Hello." During the puzzle-solving task, their nonverbal behavior was consistent with that of a new acquaintance. They were polite but not openly friendly. They gave each other reasonable interpersonal space and nodded pleasantly at the other's suggestions. When leaving the room, they walked out at the same time but not side-by-side. In the hostile (disliked acquaintance) condition, on entering the room, the two confederates indicated that they recognized each other and reacted negatively. One muttered, "I didn't know you were going to be here!" They frowned and turned away from each other. The pair used nonverbal demonstrations of hostility during the puzzle-solving task: They muttered and spoke in lowered tones, leaned back, turned away, crossed their arms or legs (increasing interpersonal space), and showed a mocking or disdainful expression at the other's efforts or suggestions. When leaving the room, one confederate waited until the other had left before getting up to leave.

Dependent measures. During and following the interaction between the confederates, participants completed several tasks that contained the primary dependent measures. One set of tasks were evaluation ratings. Using four identical 16-item scales, participants rated four targets: (a) the specific in-group member (the confederate wearing the same color T-shirt as the participant), (b) the specific out-group member (the other confederate), (c) the in-group in general, and (d) the out-group in general. Participants always evaluated the group member first and then the group as a whole; whether the in-group or out-group was rated first was counterbalanced within condition. Each scale included 10 character traits (e.g., intelligent, confident, inflexible, indifferent) and six performance qualities (e.g., communicates effectively, effective problem solver), rated on a 9-point scale that ranged from 1 (*not at all*) to 9 (*very much*). These items were pretested, using a sample similar to the actual participants, and included seven positive evaluations, six negative evaluations and three neutral evaluations.³

³ We also included a second set of tasks, focusing on resource allocation, that involved dividing lottery tickets between 2 other persons identified only by their group membership. Participants were told that following the completion of the study a \$200 lottery would be held. This allocation task used six of the Tajfel matrices commonly used in minimal group research. These matrices, their use, and their scoring are described in detail elsewhere (Bourhis, Sachdev, & Gagnon, 1994). By computing pull scores, the matrices have been used to monitor a number of in-group favoritism strategies, fairness strategies, and out-group favoritism strategies. However, the interpretation of the Tajfel matrices remains controversial (see Bornstein & Crum, 1988; Bornstein, Crum, Wittenbraker, Harring, Insko, & Thibaut, 1983; Turner, 1983). We therefore present these results as a footnote, because they provide support for the

Participants also completed: (a) a scale designed to measure identification with the experimentally constructed blue or green in-group. This scale consisted of four items, each answered on a 9-point scale ($\alpha = .69$; items were taken from a list in Ellemers, 1993); (b) a one-item manipulation check of the confederates' relationship, rated on a 10-point scale that ranged from 1 (*friendly*) to 10 (*hostile*); and (c) two questions designed to identify suspicion.

Results

Preliminary analyses. The potential differential impact of the confederate pairs was assessed by using a MANOVA and subsequent univariate analysis for each dependent variable. No significant main or interaction effects emerged; thus all data were collapsed across this variable. Also, because participants observed the interaction of the confederate pairs in groups, it was necessary to test the possibility of nonindependence of participants within experimental sessions. Three separate MANOVAs (one for each of the three experimental conditions) that used all dependent variables (three evaluation scales for each of four targets) yielded no significant effects of experimental session. None of the subsequent 36 univariate ANOVAs were significant. Thus we used participant as the unit of analysis throughout.⁴

Manipulation checks. A one-way ANOVA on the manipulation check item indicated a highly significant effect of condition, $F(2, 165) = 119.62, p < .001$. Post hoc comparisons (with Newman-Keuls procedure, $\alpha < .05$) indicated that participants in the friend condition ($M = 6.50$) perceived the relationship between the confederates to be significantly more friendly than did participants in the neutral condition ($M = 3.83$), who saw the relationship as significantly more friendly than did participants in the hostile condition ($M = 1.61$).

General evaluation ratings. The 16 trait and performance ratings were subjected to a series of factor analyses. After eliminating the neutral items, a consistent three-factor solution emerged over the four different targets. The factors were used to create three scales: a five-item Admiration Scale (e.g., intelligent, confident; alphas ranged from .75 to .86 across the four targets); a three-item Positive Affection Scale (e.g., warm, car-

ing; alphas ranged from .63 to .70); and a three-item Negative Affection Scale (e.g., inflexible, discourages other; alphas ranged from .71 to .77). Scores on the Negative Affection Scale were reversed (higher scores indicated more positive evaluations). Consistent with work on person perception and attitude ambivalence (e.g., MacDonald & Zanna, 1996), the correlations between these three scales for each target were generally mild to modest.

The effect of experimental condition on evaluation of the specific in-group and out-group member (i.e., the confederates) was investigated by using a 3×2 mixed MANOVA, with the three scales as multiple dependent variables, condition as a three-level between-subject independent variable, and in-group-out-group target as a two-level, within-subject independent variable. This analysis yielded only a significant main effect of condition, $F(6, 330) = 16.82, p < .001$. Univariate analyses indicated that condition was significant for both Positive Affection Scale, $F(2, 166) = 47.82, p < .001$, and Negative Affection Scale, $F(2, 166) = 9.73, p < .001$. Although the means were in the predicted direction, the effect for the Admiration Scale was not significant. Post hoc comparisons (Newman-Keuls, $\alpha < .05$) indicated that this effect was the result of both the in-group and out-group confederates in the friend condition (Positive Affection Scale $M = 6.12$; Negative Affection Scale $M = 6.81$) being perceived more positively than were those in the neutral condition (M s = 5.48 and 6.20, respectively). Similarly, the confederates in the neutral condition were perceived more positively than those in the hostile condition (Positive Affection Scale $M = 4.03$; Negative Affection Scale $M = 5.55$). However, no differences were found between the evaluation of the in-group versus the out-group confederate in any of the three conditions on any of the three scales.

Our first analysis testing the extended contact effect focused on the effect of experimental condition on participants' evaluation of the in-group and out-group as a whole. This analysis used the analysis strategy that was described in the previous section and yielded a significant main effect of condition, $F(6, 314) = 2.35, p < .05$, a significant main effect of in-group-out-group target, $F(3, 156) = 12.19, p < .001$, and a significant Condition \times Target interaction, $F(6, 314) = 2.40, p < .05$ (which is the effect of interest). Univariate analyses indicated that this interaction was significant for both Positive Affection Scale, $F(2, 159) = 4.84, p < .01$, and Negative Affection Scale, $F(2, 159) = 3.58, p < .05$ (see Figure 2). Subsequent simple effects tests yielded a significant main effect of condition on evaluations of the out-group for both the Positive Affection Scale, $F(2, 159) = 4.33, p < .05$, and the Negative Affection Scale, $F(2, 159) = 5.66, p < .01$, but indicated no main effect of condition on evaluations of the in-group for either of these scales. In addition, post hoc pairwise comparisons (Newman-

findings of the other dependent variables. The six matrices we used allowed for the computation of six pull scores that were considered in two sets: (a) discrimination strategies (three scores that represent the pull of in-group favoritism over other strategies) and (b) fairness strategies (three scores that represent the pull of parity and maximizing the joint profits received by both persons). Two separate multivariate analyses of variance (MANOVAs) were performed to determine the impact of experimental condition on participants' use of discrimination strategies and fairness strategies. No overall effect of condition was found for the use of discrimination strategies. However, the analysis of the fairness strategies yielded a significant overall effect of experimental condition, $F(6, 296) = 2.42, p < .05$. Comparisons of the univariate means indicated that, compared to participants in both the neutral and hostile conditions, participants in the friend condition showed significantly stronger pull toward parity and maximizing joint profits. Thus, on at least some of the allocation matrices, participants exposed to a cross-group friendship showed a stronger tendency to demonstrate fairness in their allocation of lottery tickets than did participants who observed a hostile or neutral cross-group interaction.

⁴ In addition, we also ran the analyses for each dependent variable using session as the unit of analysis. The resulting effect sizes were considerably larger, as would be expected from the gain in reliability by combining scores over the several participants in each session. Indeed, the effect sizes were so large that, even with the substantially reduced sample size when using group as the unit of analysis, all the significant findings found by using individuals as the unit of analysis remained significant when we used session as the unit of analysis.

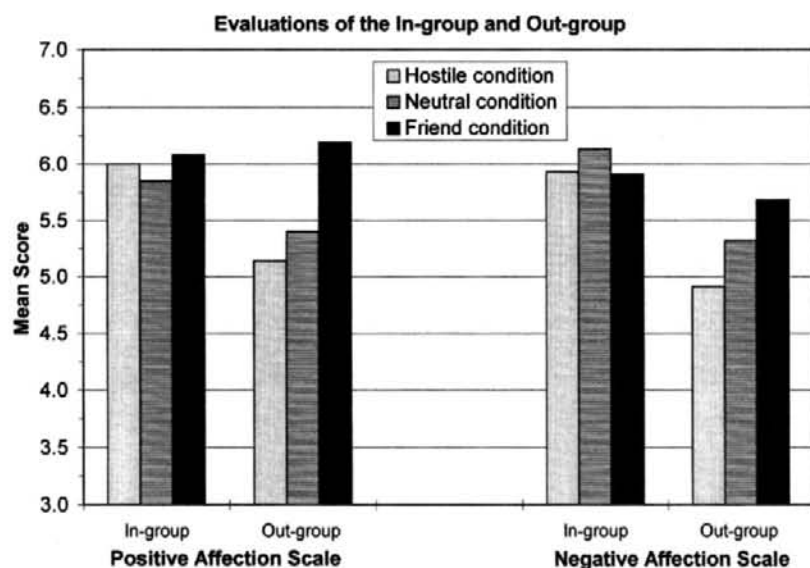


Figure 2. Mean evaluation of the in-group and the out-group on two scales by participants in each of the three experimental conditions in Study 4.

Keuls, $\alpha < .05$) indicated that participants who viewed a hostile or neutral interaction showed a clear bias against the out-group. Participants in these two conditions rated the out-group significantly lower than their in-group on the Positive Affect Scale and the Negative Affect Scale. On the other hand, participants in the friend condition showed no significant bias on either scale; that is, the out-group was not rated significantly more negatively than the in-group. Thus it appears that the observation of a cross-group friendship successfully removes the bias against the out-group found in the neutral and hostile conditions and that this is accomplished not by reducing the evaluations of the in-group but by improving the evaluation of the out-group.

In-group identification. A one-way ANOVA for condition was not significant.

Discussion

The findings of Study 4 provide direct causal evidence for the extended contact hypothesis. Observation of an interaction between cross-group friends led to more positive evaluations of the out-group than did observation of an interaction between an in-group and an out-group member who were either strangers or disliked acquaintances. In fact, the observation of the cross-group friendship successfully eliminated the in-group bias that is commonly found in this minimal group paradigm and that was clearly replicated here in our neutral and hostile conditions.

In terms of the potential mechanisms that might account for this effect, these results are clearly consistent with the in-group exemplar explanation and, more specifically, with an explanation based on referent informational influence (the influence exerted by members of the in-group when social identity is salient). The procedures we used were intended to raise the salience of the participants' membership in the relevant group. When that social identity is salient, the individual should be receptive to information from an in-group member that provides

insight into the in-group's shared consensus about the out-group and the type of intergroup behaviors that are normative (Haslam et al., 1996). In addition, the strength of this referent informational influence is affected by the present situation of the group. Group members will be more susceptible to influence from an in-group member and will actively search for it when there is ambiguity about the in-group norms and in-group stereotypes (i.e., in a new group, when the group situation is changing, when the target stimulus is novel—a new out-group or a new situation). Given that this study created a new intergroup context and that the norms for intergroup behavior are largely unknown, participants may have been particularly responsive to the influence of an in-group member.

In addition, it is possible that the initial introduction to the study provided by the experimenter may have provided participants with additional encouragement to consider referent informational influence. The introduction provided to participants at the beginning of Study 4 indicated that the study was designed to determine if untrained laypeople could correctly identify the differences between the two groups. This may have further heightened participants' search for referent information.

In sum, to the extent that referent informational influence explains the extended contact effect, the effect will be greatest when the individual is motivated to determine in-group norms and stereotypes. Future research should investigate the relative effectiveness of procedures designed to enhance the impact of referent informational influence and observe the subsequent strength of the extended contact effect.

There is also some evidence that the out-group exemplar may explain these findings. Consider the association of the evaluations of the in-group and the out-group as a whole and the evaluations of the specific in-group and out-group members. The specific out-group member's interaction with the in-group member (friendly, neutral, or hostile) had a clear effect on parti-

cipants' evaluation of her as an individual. The out-group member who was in a friendly relationship was evaluated most positively, and the out-group member in the hostile relationship was evaluated most negatively. In addition, as shown in Figure 2, one can see a similar pattern in the evaluations of the out-group as a whole. The evaluations of the specific out-group member appear to be reflected in the evaluations of the out-group as a whole, with more positive attitudes toward the out-group expressed in the friend condition and relatively negative attitudes expressed in the neutral, and especially in the hostile, condition. This finding provides some indirect support for the possible role of the out-group exemplar explanation. It appears that the actions of the specific out-group member in the cross-group relationship influences the affection felt for the out-group as a whole.

However, a very different relationship appears to exist between participants' evaluations of the specific in-group member and their evaluations of the in-group in general. The level of affection expressed toward the specific in-group member was clearly affected by her interactions with the out-group member. However, as shown in Figure 2, evaluations of the in-group as a whole were only minimally affected by the type of relationship that was observed between a specific in-group and out-group member. Perhaps an in-group member's actions are dismissed as the individual tendencies of a single member of the heterogeneous in-group, whereas the actions of an out-group member, be they friendly or hostile, are seen as more representative of the general nature of the out-group as a whole (Mullen & Hu, 1989; Park & Judd, 1990).

Finally, the Study 4 data also bear on the inclusion-of-other-in-the-self mechanism. This mechanism implies a difference in effects for the friend versus neutral or hostile conditions but not much difference between the neutral and hostile conditions. This is because each member of the cross-group pair should have been perceived as including the other in the self only in the friend condition; both the neutral and hostile conditions equally represented noninclusion. (Similarly, in a resource allocation task, Aron et al., 1991, found that a friend was treated quite differently from either a stranger or disliked other but that these two nonfriends were treated only slightly differently.) The present findings of strong effects on Positive Affection Scale for the friend condition but not for the other two conditions are thus consistent with this mechanism. (On the manipulation check there was a smaller difference in perceived friendliness for neutral versus hostile than for friend versus neutral. However, this difference on the manipulation check was nevertheless significant, whereas the effect on the Positive Affection Scale was clearly not significant and of smaller magnitude.)

In considering the influence of the manipulation on evaluations of the out-group, one might be tempted to advance an explanation that is based on a general elevation of the participants' mood. Perhaps witnessing a friendly interaction induces a generally positive mood state, which leads to more favorable evaluation of the out-group. Although a reasonable explanation for the changes in evaluation of the out-group, a mood induction explanation cannot account for the differential effect of the friendly manipulation on the out-group and the in-group. Dovidio, Gaertner, Isen, and Lowrance (1995) found that positive mood led to more positive ratings of both the in-group and the

out-group. In fact, Dovidio et al. (1995) suggested that "positive affect may enhance ingroup evaluations to a greater extent than outgroup evaluations" (p. 861). As shown in Figure 2, ratings of the in-group were not much affected by the manipulation. Also, Forgas and Fiedler (1996) found that when the personal importance of the relevant groups is low (as is usually the case in the minimal group paradigm), positive mood induction leads to more intergroup discrimination.

In sum, Study 4 provided relatively unambiguous experimental evidence for the causal direction of the extended contact effect. It also provided preliminary support for all three of the hypothesized mechanisms: in-group exemplar, out-group exemplar, and including other in the self.

General Discussion

Although each of these studies has particular limitations, when taken together they provide substantial overall support for the extended contact hypothesis. Survey data (Studies 1 and 2) collected from both majority and minority ethnic groups show that knowledge of a cross-ethnic friendship, the number of such friendships known, and the perceived closeness of these friendships all were associated with lower levels of overall prejudice toward that ethnic out-group. In Study 3, the creation of cross-group friendships reduced the escalating negative intergroup attitude among noncontact group members in a laboratory constructed intergroup conflict. Finally, Study 4 used a modified minimal group experiment to show that observation of a cross-group friendship prevents the in-group bias that is demonstrated following observation of a cross-group interaction between strangers or disliked acquaintances. Studies 1 and 2 supported the potential impact of extended contact on real-world interethnic attitudes. Studies 3 and 4 provided initial experimental support for the causal direction proposed by the extended contact hypothesis: from knowledge and observation of a cross-group friendship to more positive intergroup attitudes. Together, these four studies provide the basis for the substantiality of this theoretically interesting effect.

We proposed three potential mechanisms for this effect: (a) that the in-group friendship partner provides a positive exemplar for more tolerant and caring norms for interaction with the out-group; (b) that the out-group member provides a positive exemplar that disconfirms negative expectations and beliefs about the out-group's attitudes toward, and relationship with, the in-group; and (c) that knowledge of the closeness of the cross-group friends leads to a partial inclusion of the out-group in the self. The results of all four studies are consistent with all of these mechanisms. However, these results regarding mechanisms must be taken as very preliminary. The present research program was designed primarily to demonstrate the effect itself. Unambiguous delineation of the precise mechanisms must await further research.

In addition to the theoretical implications, the extended contact effect is potentially a very exciting practical addition to social science's repertoire of strategies for improving intergroup relations. The many barriers and pitfalls associated with efforts to create direct interpersonal contact make it unlikely that a large number of group members will have optimal contact experiences. Producing interpersonal closeness on a large scale is a

daunting task. However, most of these obstacles do not apply to the extended contact case. Observing the interactions of others is unlikely to produce the anxiety and fears that can disrupt direct contact experiences (Stephan & Stephan, 1985; Wilder, 1993). In addition, interactions between friends contain many of the conditions described as necessary for optimal contact. Thus, if there can be significant improvements in intergroup attitudes among noncontact group members as a result of observing the cross-group friendships of other in-group members, making salient even a few cases of cross-group closeness may be an effective way to initiate improved intergroup attitudes.

There are also a number of qualifications that should be considered. First, as noted earlier, there is the issue of subtyping or referencing (Allport, 1954). Also, the number of salient cross-group friendships should play a role in the ease with which the partners are subtyped. (This factor may be reflected in the quantitative effects in Studies 1 and 2: the greater attitude difference associated with knowing more in-group members who have friends in a particular out-group.) Clearly, as these friendships become more numerous, subtyping becomes increasingly constrained. However, we propose that when the other criteria necessary to reduce subtyping are met, relatively few friendships, or even a single highly prototypical pair (as in our Study 4), might serve to reduce negative out-group attitudes.

In conclusion, the specific psychological mechanisms that are involved in the extended contact effect require further investigation. In addition, future research must attempt to determine the conditions under which it will and will not be an effective means of attitude change. However, the present set of studies provide an exciting possibility for intergroup relations: the possibility that under some conditions simply being aware of a cross-group friendship may be sufficient to produce positive intergroup attitude change.

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