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Ingroup Favoritism in Cooperation: A Meta-Analysis

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Although theory suggests individuals are more willing to incur a personal cost to benefit ingroup members, compared to outgroup members, there is inconsistent evidence in support of this perspective. Applying meta-analytic techniques, we harness a relatively recent explosion of research on intergroup discrimination in cooperative decision making to address several fundamental unresolved issues. First, summarizing evidence across studies, we find a small to medium effect size indicating that people are more cooperative with ingroup, compared to outgroup, members ($d = 0.32$). Second, we forward and test predictions about the conditions that moderate ingroup favoritism from 2 influential perspectives: a social identity approach and a bounded generalized reciprocity perspective. Although we find evidence for a slight tendency for ingroup favoritism through categorization with no mutual interdependence between group members (e.g., dictator games; $d = 0.19$), situations that contain interdependence result in stronger ingroup favoritism (e.g., social dilemmas; $d = 0.42$). We also find that ingroup favoritism is stronger when there is common (vs. unilateral) knowledge of group membership, and stronger during simultaneous (vs. sequential) exchanges. Third, we find support for the hypothesis that intergroup discrimination in cooperation is the result of ingroup favoritism rather than outgroup derogation. Finally, we test for additional moderators of ingroup favoritism, such as the percentage of men in the sample, experimental versus natural groups, and the country of participants. We discuss the implications of these findings for theoretical perspectives on ingroup favoritism, address implications for the methodologies used to study this phenomenon, and suggest directions for future research.

Keywords: ingroup favoritism, intergroup bias, discrimination, cooperation, trust

Groups are a pervasive feature of our social lives. We interact with people who share common group identities (e.g., nations, religions, and political parties) and also find ourselves interacting with others who belong to different groups. A great challenge to organizations, communities, and societies is that people sometimes discriminate between ingroup and outgroup members (Hewstone, Rubin, & Willis, 2002). People evaluate ingroup members more positively than outgroup members (Brewer, 1979; LeVine & Campbell, 1972; Mullen, Brown, & Smith, 1992; Perdue, Dovidio, Gurtman, & Tyler, 1990), tend to reward ingroup members more than outgroup members (Tajfel, Billig, Bundy, & Flament, 1971), and work harder to accomplish ingroup goals (Ellemers, De Gilder, & Haslam, 2004; Worchel, Rothgerbuer, Day, Hart, & Butemeyer, 1998). Such positive bias toward one's ingroup may promote the functioning and performance of one's ingroup, which may provide the individual with long-term benefits and increased survival probability (Brewer, 1999; Caporael, 2007; Darwin, 1871). At the same time, however, such intergroup bias may also create feelings of deprivation and resentment in outgroups, who may respond with hostility toward the discriminating ingroup (De Dreu, 2012; Dovidio & Gaertner, 2010; Fiske, 2002; Wildschut, Pinter, Vevea, Insko, & Schopler, 2003). Thus, intergroup bias appears a mixed blessing—it creates strong ingroups but potentially fuels intergroup tension, hostility, and competition (e.g., Bornstein, 2003; Halevy, Chou, Cohen, & Bornstein, 2010; Yzerbyt & Demoulin, 2010).

This mixed blessing becomes particularly relevant when intergroup bias manifests itself in decisions to be generous, to extend trust, or to cooperate rather than compete with ingroup members more than with members of outgroups. Such intergroup discrimination in cooperative decision making will then have tractable and potentially critical consequences for all those social systems composed of multiple groups including work organizations, political coalitions, and multiethnic communities. However, whether intergroup discrimination in cooperation exists remains an open question: The existing evidence is inconclusive and inconsistent, with some studies finding greater cooperation toward ingroup compared to outgroup members, and others failing to observe such intergroup discrimination. Second, *when* people would cooperate more with ingroup members continues to be a topic of scientific debate. Different boundary conditions are contained in the social identity approach (SIT; Ellemers & Haslam, 2012; Tajfel & Turner, 1979) and the theory of bounded generalized reciprocity (BGR; Yamagishi, Jin, & Kiyonari, 1999), and specific predictions can be for-

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warded about moderators of intergroup discrimination in cooperation, such as mutual outcome interdependence, opportunities for (in)direct reciprocity, and the presence of reputational concerns. Finally, *why* intergroup discrimination would emerge remains unclear. Is it because of a fundamental desire to benefit one's ingroup (ingroup favoritism), to aggress against and harm rivaling outgroups (outgroup derogation), or because of a combination of ingroup favoritism and outgroup derogation?

Here we address these three issues by meta-analyzing four decades of research on intergroup discrimination in cooperation. Before presenting our results, we review the various research traditions on intergroup discrimination in cooperation, discussing the inconclusive and sometimes inconsistent evidence and outlining the key experimental paradigms used in the studies included in the meta-analysis. Subsequently, we describe how SIT and BGR forward hypotheses on core boundary conditions of intergroup discrimination in cooperation, and then examine whether intergroup discrimination in cooperation is rooted in ingroup favoritism, outgroup derogation, or some combination thereof.

Intergroup Discrimination in Cooperation

The fact that people may discriminate between their own group and other groups was revealed already in classic studies in social psychology (e.g., Allport, 1954; Sherif, Harvey, White, Hood, & Sherif, 1961; Sumner, 1906; Tajfel et al., 1971). A pioneering study by Tajfel et al. (1971), for example, showed that when participants had to choose from a series of matrices containing rewards to ingroup and outgroup members (e.g., 42/22 points vs. 34/34 points to ingroup member/outgroup member), they seek either to maximize ingroup outcomes or to maximize differences between ingroup and outgroup outcomes (for reviews, see Brewer, 1979; Bourhis, Sachdev, & Gagnon, 1994). Tajfel et al. further showed this form of intergroup discrimination to be very basic and suggested it to emerge already in a very minimal setting where participants (a) are randomly assigned to groups, (b) are not allowed to communicate (e.g., either face to face or via some other medium), (c) do not know others in their ingroup or the outgroup, and (d) have no vested interest in serving their group (for reviews and discussions, see Brewer, 1979, 1999, 2007; Dovidio & Gaertner, 2010; Tajfel & Turner, 1979; Turner, Hogg, Oakes, Reicher, & Wetherell, 1987).

This pioneering work spurred decades of research on intergroup bias and discrimination. In most of these studies group membership of a partner is experimentally manipulated, and people are assigned to interact with another person who is classified as either an ingroup member or an outgroup member. Group membership is determined either by experimental assignment using the minimal group paradigm (e.g., Y. Chen & Li, 2009; Güth, Ploner, & Regner, 2009) or via naturally existing groups (e.g., religious, organizational, ethnic; Goette, Huffman, & Meier, 2006; Whitt & Wilson, 2007). For example, under the minimal group paradigm, participants would first indicate how much they liked different types of abstract art, and then they are categorized into two groups based on their preferences. However, in reality, group membership is randomly assigned across participants and then their cooperation toward ingroup and outgroup members is observed. Because research participants typically are told nothing about their partner except for their group membership, any differences in cooperation toward ingroup and outgroup partners can be attributed to discrimination based on group membership.

Although the pioneering work by Tajfel et al. (1971) provided some first insight in possible intergroup discrimination in cooperation, and a number of other studies replicated and extended these early findings (e.g., Tajfel & Turner, 1979; Turner et al., 1987), some criticized the evidence and argued that the reward allocation matrices used in these initial demonstrations of discrimination never allowed participants to extend rewards to both the ingroup and outgroup (e.g., Bornstein et al., 1983; Gaertner & Schopler, 1998; Rabbie, Schot, & Visser, 1989; Yamagishi & Kiyonari, 2000). Therefore, it was argued the reward allocation matrices biased participants toward favoring their own group and, inadvertently, forced upon the individuals a representation of the ingroup being opposed to the outgroup. Indeed, when using reward allocation tasks that allowed for any possible motive to be expressed, participants overwhelmingly choose to maximize ingroup outcomes and to maximize the joint outcomes of both ingroup and outgroup (Bornstein et al., 1983; Yamagishi et al., 1999).

In addition to reward allocation matrices, psychologists, sociologists, and especially economists more recently invoked a number of alternative decision making tasks to examine costly acts of generosity and cooperation toward either ingroup members, outgroup members, or unclassified strangers. A description of five commonly used decision-making tasks are provided in Table 1. Some studies employed the dictator game (Forsythe, Horowitz, Savin, & Sefton, 1994). The dictator game involves two players, a dictator and a recipient. The dictator receives an endowment (e.g., 10 euros) and then decides how much (if any) to give to the recipient. The recipient does not have a say in the decision and cannot affect the dictator's outcomes. Thus, it is in the dictator's self-interest not to give any money, and any money sent to the recipient is a costly act to self that benefits the recipient. Studies using the dictator game assign participants to the dictator role, who then decide how much to give to either an ingroup member, an outgroup member, or, in some cases, an unclassified stranger. Some studies found that people give more to ingroup members compared to outgroup members (Ben-Ner, McCall, Stephane, & Wang, 2009; Liebe, & Tutic, 2010; Whitt & Wilson, 2007), but other studies found no evidence for such intergroup discrimination (e.g., Lei & Vesely, 2010; Stürmer, Snyder, Kropp, & Siem, 2006).

Intergroup discrimination has also been studied with the trust game (Berg, Dickhaut, & McCabe, 1995). Here participants are assigned to be an investor, and they are paired to a trustee from their ingroup or an outgroup (some studies include an unclassified stranger). In the trust game, the investor receives an endowment (e.g., 10 euros) and decides how much to transfer to the trustee. The transferred amount is tripled by the experimenter, upon which the trustee decides how much of the money to return to the investor. Thus, investors can maximize both their own and other's outcomes if they decide to transfer their entire amount to their trustee (and the trustee delivers half of their amount back to the investor). However, because trustees have a selfish temptation to keep the entire amount for themselves, such cooperation by investors is potentially costly and involves the risk of exploitation by their trustee. While some studies found that participants give more money to ingroup compared to outgroup trustees (e.g., Fershtman & Gneezy,

Table 1
Five Decision-Making Tasks in the Meta-Analysis, Person A's Experienced Outcome Interdependence, and Possibility of (In)direct Reciprocity

Task	Decision path	Outcome interdependence experienced by A	(In)direct reciprocity expected by A
Dictator game ($x \leq 10$)	A → B (10–x) (x) A transfers x MU to B	No	Indirect
Trust game ($x \leq 10$, $y \leq 3x$)	A ⇄ B (10–x+y) (3x–y) A transfers x MU to B first. B receives 3x MU and then back-transfers y MU to A	Yes	Direct and indirect
Prisoner's dilemma (x, y = 0 to 10)	A ↔ B (10–x+2y) (10–y+2x) A (B) transfers x (y) MU to B (A) and x and y are doubled after being transferred	Yes	Indirect
Faith game ($x \leq 30$)	A ↔ B A chooses between taking the sure 10 MU and receiving whatever amount B gives A from 30 MU	Yes	Indirect
Stag hunt game ($x > y \geq z$)	A ↔ B If both cooperate, both receive x MU; If only A (B) defects, A (B) receives y MU, and B (A) receives 0 MU; If both defect, both receive z MU	Yes	Indirect

Note. A and B refer to the two persons interacting in the decision-making task, with A being the focal party in our analysis. In the dictator game and trust game, A is given 10 monetary units (MU), whereas in the prisoner's dilemma, A and B both start with an endowment of 10 MU. The faith game is a variant of the trust game where A can choose either not to trust B and earn a sure amount or to trust B and receive whatever amount B decides to give to A. However, unlike the trust game, in the faith game B decides to split a specific amount of money with A, without knowledge that A has a decision to trust B or not. The stag hunt game is similar to the prisoner's dilemma, except that each person receives the highest payoff if both cooperate. The formulas under A and B refer to their final outcomes based on their decision. For the dictator game, we classify this as independent from A's perspective, that is, their outcome only depends on their own action. Here we assume a single trial for each decision-making task.

2001; Hargreaves Heap, Verschoor, & Zizzo, 2009; Ioannou, Qi, & Rustichini, 2012), other studies found no evidence of intergroup discrimination in the trust game (Güth, Levati, & Ploner, 2008; Johansson-Stenman, Mahumud, & Martinsson, 2005).

Many studies on intergroup discrimination in cooperation relied on social dilemma games like the prisoner's dilemma game (Goette, Huffman, Meier, & Sutter, 2012; Wit & Wilke, 1992; Yamagishi, Mifune, Liu, & Pauling, 2008), or (structurally equivalent) public goods dilemmas and resource dilemmas (Brewer & Kramer, 1986; De Cremer & Van Vugt, 1999; Eckel & Grossman, 2005; Kramer & Brewer, 1984). For example, in the prisoner's dilemma, two individuals simultaneously decide how much of their endowment (e.g., 10 euros) to give to their partner. Because any amount provided to their partner is doubled, both individuals would be better off if both decided to cooperate, rather than not. However, similar to the dictator game, there is the greedy temptation to keep one's endowment. As in the trust game, there is the fear that one's cooperation may be exploited by a greedy partner. While many studies using social dilemmas found that people cooperate more with ingroup than with outgroup members (Ando, 1999; Goette, Huffman, et al., 2012), some reported no discrimination (Goerg, Meise, Walkowitz, & Winter, 2013; Simpson, 2006).[1]

Boundary Conditions of Intergroup Discrimination

Two theoretical perspectives that have directed much research on intergroup discrimination and ingroup favoritism in cooperation are SIT (Tajfel & Turner, 1979) and BGR (Yamagishi et al., 1999). We briefly describe these perspectives and then highlight the predictions they suggest regarding three moderating conditions: (a)

[1] Research on intergroup conflict has developed so-called team games (Bornstein, 2003), which model intergroup conflict as a double social dilemma—one within each opposing group and one between the two opposing groups. This work reveals whether and how between-group conflict, and the way between-group competition is structured, influences within-group cooperation and how within-group cooperation can fuel intergroup competition and conflict (e.g., Abbink, Brandts, Herrmann, & Orzen, 2012; De Dreu, 2010; De Dreu et al., 2010; Halevy, Bornstein, & Sagiv, 2008; Halevy et al., 2010; Wildschut et al., 2003). This type of work is reviewed elsewhere (Bornstein, 2003; De Dreu, Balliet, & Halevy, in press) and not included here because its focus is on within-group cooperation and only indirectly speaks to intergroup discrimination in cooperation.

the absence versus presence of mutual outcome interdependence, (b) opportunities for (in)direct reciprocity, and (c) the presence of reputational concerns.

SIT (see Ellemers & Haslam, 2012) is based on developments in social identity theory (Tajfel, 1974) and self-categorization theory (Turner et al., 1987). It proposes that categorizing oneself and others into distinct social categories is sufficient to elicit ingroup favoritism and intergroup discrimination (Turner & Reynolds, 2012).[2] The theory distinguishes between *personal identity*, which is how people think and feel about their unique idiosyncratic traits, and *social identity*, which is "the part of an individual's self-concept which derives from his knowledge of his membership of a social group (or groups) together with the emotional significance attached to that membership" (Tajfel, 1974, p. 69). SIT assumes that people self-categorize as belonging to a specific group in the presence of an outgroup, and because people are motivated to maintain a positive social identity, mere categorization initiates thoughts, feelings, and behaviors that attempt to positively differentiate the ingroup from the outgroup (Tajfel & Turner, 1979).

An important assumption in SIT is the so-called metacontrast principle (Turner et al., 1987), which states that ingroup favoritism requires intergroup comparisons and the presence of an outgroup. Without the presence of an outgroup, ingroup categorization does not occur and subsequent discrimination should not emerge (Van Knippenberg & Wilke, 1988). Put differently, according to SIT, there should be no meaningful difference in cooperation between ingroup members and unclassified strangers. Only in the presence of a salient outgroup, positive social identity striving should manifest itself in tendencies to cooperate with ingroup members more than with outgroup members (Brewer, 2008; Ellemers, 2012).

An alternative perspective on intergroup discrimination in cooperation is provided by BGR (Yamagishi et al., 1999; Yamagishi & Mifune, 2009). BGR is grounded in an evolutionary approach to (human) cooperation (e.g., Axelrod & Hamilton, 1981; Bowles & Gintis, 2011; Darwin, 1871; N. S. Henrich & Henrich, 2007; Trivers, 1972; Wilson, 1978) and assumes that groups were important for the survival and reproductive success of individuals. Furthermore, BGR assumes that human groups provide a container for a generalized exchange network. That is, human groups contain a system of indirect reciprocity whereby individuals behave in ways to maintain a positive reputation, because people cooperate with others who have a cooperative reputation and exclude others who lack such a reputation. Accordingly, people must maintain the reputation of a reliable cooperator to avoid the cost of exclusion from the group and to gain the benefits of cooperative exchange in future interactions with other ingroup members. BGR specifies that humans evolved (a) to have depersonalized and generalized trust that other ingroup members will cooperate, (b) to be motivated to establish and maintain a cooperative reputation among ingroup members, and (c) to expect to receive benefits from other ingroup members, but not necessarily from the same ingroup members they cooperated with or helped (Kiyonari & Yamagishi, 2004; Yamagishi et al., 1999). Cues of group membership are hypothesized to increase trust, reputational concerns, and cooperation, even in the absence of an explicit outgroup and without the potential for direct reciprocity from one's current interaction partner. Thus, BGR argues that because of generalized trust in ingroup members and the need to build a positive reputation among ingroup members, people cooperate with ingroup members more than with outgroup members and unclassified strangers (Mifune, Hashimoto, & Yamagishi, 2010; Yamagishi et al., 1999).

Taken together, both SIT and BGR predict that people will be more willing to cooperate with ingroup members compared to outgroup members (Hypothesis 1). However, SIT suggests that people will not discriminate against ingroup members in the absence of a salient outgroup, and so there should be no differences in cooperation between studies that observe cooperation among ingroup members and cooperation among strangers (Hypothesis 2a). However, BGR predicts that only cues of ingroup membership are necessary to spark ingroup favoritism, and so people should be more cooperative toward ingroup members than unclassified strangers (Hypothesis 2b). As illustrated above, these predictions are based on different psychological mechanisms (positive social identity striving vs. expectations of indirect reciprocity and reputation concerns). From these follow specific boundary conditions of intergroup discrimination in cooperation.

Mere Categorization Versus Outcome Interdependence

A long-standing debate about the conditions that give rise to intergroup bias in general, and intergroup discrimination in cooperation more specifically, has centered on the role of categorization and interdependence (Rabbie & Horwitz, 1969; Rabbie et al., 1989; Tajfel et al., 1971; Yamagishi et al., 1999). The findings of intergroup discrimination in the minimal group paradigm and subsequent theorizing to explain the phenomenon emphasized the role of self-categorization with the ingroup in the presence of an outgroup, can result in identifying with the ingroup and eventually the desire to positively discriminate the ingroup from the outgroup (i.e., mere categorization; Tajfel & Turner, 1979; Turner et al., 1987). Yet, the paradigm used in this original work did not completely eliminate the outcome interdependence among participants. In the minimal group studies, ingroup members and outgroup members allocated points among each other, and each person's outcomes were determined by their own and others' point allocations. Researchers have claimed that this interdependence, compared to self-categorization, could be the factor that caused ingroup favoritism in the minimal group studies (Gaertner & Schopler, 1998; Rabbie et al., 1989; Yamagishi & Kiyonari, 2000). The debate about how interdependence and categorization spark ingroup favoritism has inspired much research but, unfortunately, there currently exists no strong conclusion on the topic (e.g., Baron, 2001; Gaertner & Insko, 2000; Gagnon & Bourhis, 1996; Locksley, Ortiz, & Hepburn, 1980; Rabbie et al., 1989; for reviews, see Scheepers, Spears, Doosje, & Manstead, 2006; Yamagishi et al., 1999).

In the meta-analysis we can address two issues related to this debate. First, we can test whether intergroup discrimination occurs

[2] Although there are important differences between social identity theory (Tajfel & Turner, 1979) and self-categorization theory (Turner et al., 1987), the present research does not test predictions unique to each of these perspectives. Because both theories share the same emphasis on the process of categorization in relation to ingroup favoritism, as in past research, we discuss both perspectives under the label of SIT (see Ellemers & Haslam, 2012). We also note that we provide a relatively restrictive review of this perspective, and that SIT has broader implications than understanding categorization and identification on intergroup discrimination in cooperative decision making (e.g., see Brown, 2000; Hewstone et al., 2002).

in the absence of any mutual interdependence among ingroup members (i.e., the dictator games). In the dictator game, participants acting as dictators are not dependent on their recipients for any material outcomes. SIT assumes that mere categorization and concomitant identification leads to intergroup discrimination in cooperation in the absence of interdependence. BGR also predicts that people will discriminate in favor of ingroup members in the absence of interdependence. According to BGR, however, this is because of reputation concerns and the expectation of indirect benefits from being cooperative and generous. Thus, both perspectives predict that people will discriminate in favor of ingroup members compared to outgroup members in situations that contain no interdependence (i.e., the dictator games) during experiments that employ the minimal group paradigm (Hypothesis 3).

Second, we can examine whether the addition of mutual interdependence among ingroup members augments intergroup discrimination by comparing the amount of discrimination in dictator games to trust games and social dilemmas. Unlike participants in a dictator game, investors in the trust game and participants in social dilemmas are outcome interdependent. Thus, the decision making tasks used in research on intergroup discrimination differ in the presence versus absence of outcome-interdependence between participants and their partners (see Table 1). SIT suggests that with increased outcome interdependence people think more positively of ingroup members, and see them as more trustworthy, subsequently matching these expectations with an increase in their own cooperation (Turner et al., 1987). BGR takes an even stronger position. For example, Yamagishi and Mifune (2008) stated that "identifying oneself with a social category should not play an important role in our lives unless the social category is a source of some tangible outcomes" (p. 25). BGR predicts that there should be relatively stronger amounts of intergroup discrimination under mutual outcome interdependence, because people have a generalized expectation that their ingroup members will cooperate (Brewer, 2008; Yamagishi et al., 1999; see also Balliet & Van Lange, 2013). Thus, both perspectives hypothesize that (a) people will expect greater cooperation from ingroup members, compared to outgroup members (Hypothesis 4), and (b) people will display larger amounts of intergroup discrimination in situations that contain outcome interdependence (e.g., trust games and social dilemmas), compared to situations without interdependence (e.g., the dictator games; Hypothesis 5).

(In)direct Reciprocity

BGR suggests that the decision heuristic that gives rise to ingroup favoritism is adapted to meet the demands of *indirect* reciprocity contained in groups—the idea that one's cooperation will be reciprocated at some point by others, although not necessarily the person one cooperated with (Yamagishi et al., 1999). Expectations of indirect reciprocity require a long-term perspective, where repeated interactions with others are implicitly or explicitly possible. Expectations of indirect reciprocity are thus stronger with others belonging to one's ingroup and should be weak or absent with unfamiliar strangers and members of outgroups.

In addition to indirect reciprocity, humans are also strongly disposed toward *direct* reciprocity (Axelrod & Hamilton, 1981; Gouldner, 1960; Komorita, Parks, & Hulbert, 1992; Trivers, 1972). They may expect others, regardless of their group membership, to directly reciprocate their own initial cooperative efforts within the same interaction. Thus, when people have an opportunity to cooperate with another person, they may choose to cooperate with hopes that the other person will respond in kind. Such expectations of the direct benefits of reciprocity may encourage initiating a cooperative relationship with outgroup members but only to the extent that such returns are immediate and by the same person as one cooperated with. This is typically the case for investors in the trust game (see Table 1), whose trustee can "return the favor" before the interaction ends. Thus, investor's cooperation in the trust game is motivated, in part, by the expectation of direct reciprocity (Cox, 2004; Rabin, 1993; Trivers, 1972).

This expectation of direct reciprocity is absent among dictators in the dictator game, and among participants in social dilemma games who decide simultaneously to cooperate or not. Therefore, BGR argues that group membership may be relatively less important in determining own cooperation as the first mover in sequential cooperative decision-making tasks such as the trust game and should more strongly determine cooperation when people decide simultaneously, as in social dilemmas (Kiyonari & Yamagishi, 2004). Put differently, BGR predicts that intergroup discrimination will be stronger in simultaneous cooperative decisions (e.g., social dilemmas), compared to sequential cooperative decisions (e.g., trust games; Hypothesis 6). We note that SIT does not forward predictions about the role of (in)direct reciprocity and according to this perspective both trust games and social dilemmas should elicit similar amounts of intergroup discrimination in cooperation.

Reputational Concerns

The assumption that expectations of (in)direct reciprocity drive intergroup discrimination in cooperation rests on the idea that people are concerned about being seen by other (ingroup) members as a reliable and trustworthy cooperator. This means, in terms of BGR, that people will cooperate more with ingroup members than with outgroup members, when this can or will affect their reputation (Halevy, Weisel, & Bornstein, 2012; J. Henrich & Gil-White, 2001; Yamagishi et al., 1999; Yamagishi & Mifune, 2009). Such concerns about reputation can be removed by eliminating other's knowledge of one's own group membership. When a person knows his or her partner's group membership and also knows that the partner is unaware of his or her own group membership (*unilateral knowledge* of group membership), this removes the potential that a person's behavior may affect his or her reputation (Kiyonari & Yamagishi, 2004). In a unilateral knowledge situation, people cannot expect greater cooperation from their partner, and their own (non)cooperative behavior will not affect their reputation and chance of being excluded from the group (Yamagishi & Mifune, 2008). Thus, according to BGR, intergroup discrimination in cooperation will only occur when there is *common knowledge* of each person's group membership during an interaction, and not under unilateral knowledge (Hypothesis 7). In contrast, SIT suggests that a partner's knowledge of one's own group membership should not affect discrimination. Instead, discrimination allows for positive social identity striving regardless of a partner's knowledge of one's own group membership.

Ingroup Favoritism and Outgroup Derogation

Besides addressing when intergroup discrimination is more likely to occur, we can use the meta-analysis to address a long-standing issue in the study of intergroup discrimination: Do people discriminate out of a positive concern for one's ingroup or a motivation to harm the outgroup? In his pioneering book, Allport (1954) conjectured that ingroups are psychologically primary—people live in them and, sometimes, for them—and that ingroup favoritism has strong adaptive value because it facilitates within-group coordination and the survival of individuals and groups (see also Brewer, 1979, 2007; Darwin, 1871; Fu et al., 2012; Hammond & Axelrod, 2006; Masuda, 2012). This suggests that intergroup discrimination follows from the motivation to promote the ingroup (ingroup favoritism) more than from the motivation to harm and derogate outgroups (outgroup derogation). Indeed, decades of research consistently revealed that variations in bias in intergroup perceptions, attitudes, and evaluations emerge because of variation in ingroup favoritism more than because of variations in outgroup derogation (Brewer, 1999; Cikara, Farnsworth, Harris, & Fiske, 2010; De Dreu, Greer, Van Kleef, Shalvi, & Handgraaf, 2011; Dovidio & Gaertner, 2010; Mummendey & Otten, 1998). For example, subtle forms of racism are the result of the absence of positive sentiments toward outgroups, compared to the presence of negative sentiments (Dovidio & Gaertner, 2010; Greenwald & Pettigrew, 2014).

Whether intergroup discrimination in cooperation can also be attributed to ingroup favoritism rather than to outgroup derogation remains an open question. Studies using reward allocation matrices did not tease apart ingroup favoritism and outgroup derogation. Some of the more recent work using dictator, trust, or social dilemma games attempted to examine these two motives by observing how people treat ingroup members and outgroup members compared to an (unclassified) stranger. The idea is that ingroup favoritism motivates more cooperation toward ingroup members than toward both outgroup members and unclassified strangers (Hypothesis 8a), whereas outgroup derogation motivates less cooperation toward outgroup members than toward unclassified strangers and ingroup members (Hypothesis 8b). Unfortunately, however, primary studies using these contrasts provide mixed evidence, finding evidence for either ingroup favoritism (e.g., Yamagishi & Mifune, 2009; see also Gaertner, Iuzzini, Witt, & Oriña, 2006; Yamagishi & Kiyonari, 2000) or outgroup derogation (e.g., Hargreaves Heap & Zizzo, 2009). Here we apply a meta-analytic test of these two hypotheses about the motives underlying intergroup discrimination in cooperation.

Overview of the Meta-Analysis

We meta-analyzed studies on intergroup discrimination in cooperation to achieve three goals. Our first goal was to document the existence and strength of intergroup discrimination in cooperation. Our second goal was to examine a parsimonious set of theory-specific moderators derived from SIT and BGR. Our third and final goal was to identify why intergroup discrimination occurs, and we predicted that it emerges because of ingroup favoritism and/or because of outgroup derogation. The specific Hypotheses 1–8 are summarized in Table 2.

To achieve these three goals, and to test our hypotheses, we analyzed effect sizes from studies that compare cooperation with ingroup and outgroup members and, in some cases, with unclassified strangers. In addition to study characteristics relevant to our hypotheses, we coded study characteristics that potentially confound hypothesis testing and may reveal (method-specific) boundary conditions of intergroup discrimination. Thus, because people may identify more with existing groups than with experimentally created groups (Jackson, 2008; see also Goette, Huffman, & Meier, 2012), we coded whether group membership was created experimentally, with members being randomly assigned to in- and outgroup, or whether the study relied on existing groups (e.g., nation, ethnicity, or religion). Second, we coded the country where the study was conducted to examine possible societal differences in intergroup discrimination. Third, we coded the percentage of male participants to test for possible sex differences in intergroup discrimination. These are not unlikely, as some have argued that males may have a specialized "coalitional psychology" that functions to promote male–male cooperation within groups, especially in the context of intergroup competition (see McDonald, Navarrete, & Van Vugt, 2012). Finally, we coded for several additional moderators (described below) to control for confounds during the analyses.

Method

Search for Studies

We searched PsycINFO and Google Scholar for articles that contained either of the following terms: *ingroup favoritism*, *outgroup favoritism*, *outgroup derogation*, *intergroup bias*, *parochialism*, *cooperation*, *social dilemma*, *trust game*, *dictator game*, and *prosocial behavior*.[3] Next, we examined abstracts to identify relevant articles and searched more generally for studies using several databases in the social sciences (e.g., ABI/INFORM, PsycARTICLES, Social Sciences Citation Index, Sociological Abstracts, Web of Science, Dissertations Online, and Econlit). We also searched the references of all relevant articles and posted a call for papers on several listservs in psychology and economics. Last, we contacted over 150 experts who attended the 14th International Conference on Social Dilemmas for unpublished manuscripts or data.

The search closed in September 2013. We uncovered 77 articles that contained 212 eligible studies. Year of publication ranged from 1965 to 2013. The median year of publication (2008) suggests a recent explosion of interest in intergroup discrimination in cooperation.

[3] Although some search terms (e.g., *ingroup favoritism*, *parochialism*) could have resulted in a biased sample of studies that found greater amounts of cooperation with ingroup members, compared to outgroup members, we also included more neutral terms (e.g., *intergroup*, *intergroup bias*) and terms susceptible to a bias in favor of the outgroup (e.g., *outgroup favoritism*, *outgroup derogation*). Furthermore, in our call for unpublished data, we neutrally enquired for studies that manipulated group membership and measured cooperation or behavior in economic games. Indeed, when analyzing the overall ingroup–outgroup effect size distribution, we find that there is no publication bias. If outgroup favoritism (derogation) studies were underrepresented in our sample, this would likely result in asymmetry in the funnel plot. Instead, we find symmetry in the funnel plot.

Table 2
Key Predictions on Intergroup Discrimination in Cooperation

	Partner effects			Interdependence		Reciprocity		Knowledge	
Variable	IN/OUT	IN/STR	OUT/STR	DG	SD/TG	TG	SD	Common	Unilateral
Social identity approach	+	0	0	+	++	+	+	+	+
Bounded generalized reciprocity	+	+	0	+	++	+	++	+	0
Ingroup favoritism	+	+	0						
Outgroup derogation	+	+	−						

Note. IN/OUT = difference in cooperation when facing an ingroup versus outgroup member; IN/STR = difference in cooperation when facing an ingroup member versus unclassified stranger; OUT/STR = difference in cooperation when facing an outgroup member versus unclassified stranger; DG = dictator game; SD = social dilemmas (e.g., prisoner's dilemmas and public goods dilemmas); TG = trust game; common versus unilateral = common knowledge versus unilateral knowledge about group membership; −/0/+ = expected direction of the effect size; ++ = a relatively stronger positive effect across the row.

Inclusion Criteria

To be included in the meta-analysis, studies had to have (a) adult participants (age 18 and above); (b) applied commonly used decision-making tasks to measure and compare (personally costly) cooperation with ingroup members, outgroup members, and/or unclassified strangers; and (c) provide sufficient statistical detail to calculate effect sizes of interest. Every study involved relatively anonymous social interactions, and participants have only the minimal information about the group membership of their partner. It is common in this research tradition that people do not know the name, age, or gender of the person with whom they are interacting. Note that such minimal information designs eliminate potential confounds (e.g., previous social interaction history) that could otherwise explain how people respond to ingroup versus outgroup members.

We excluded studies that did not manipulate a partner's group membership, but only correlated individual differences in social identity with prosociality toward those group members (e.g., De Cremer & Van Knippenberg, 2005; Smith, Jackson, & Sparks, 2003). These purely correlational studies are uninformative regarding possible intergroup discrimination. Also not included were studies examining helping behaviors (e.g., Dovidio et al., 1997; Kunstman & Plant, 2008; Stürmer, Snyder, & Omoto, 2005), because they showed a dramatic variation in methods across studies, making it difficult to examine focused hypotheses about intergroup discrimination in cooperation. Finally, we excluded from the meta-analysis studies that involved dividing a reward between ingroup and outgroup members (Yamagishi et al., 1999) and used reward allocation matrices (Tajfel et al., 1971). The two primary reasons for excluding these later studies is that (a) they are not directly comparable to the decision making tasks used in the meta-analysis and (b) the matrix allocation studies do not contain variability in interdependence, (in)direct reciprocity, and reputational concerns, and so do not allow us to test our hypotheses.[4]

Coding of Effect Sizes

We used the *d* value as the measure of standardized effect size. The *d* value is the difference between two means divided by the pooled standard deviation and is corrected for sample size bias (Hedges & Olkin, 1985). When these descriptive statistics were unavailable, we derived *d* from a *t* score, *F* score, chi-square value, or rates of cooperative behavior.

For each of the 212 eligible studies, we coded for four effect sizes relevant to the predictions summarized in Table 2. First, we coded for ingroup cooperation versus outgroup cooperation (k = 168). A positive *d* value here indicates that cooperation (e.g., donations in a dictator game, investments in a trust game, amount given in a social dilemma) was higher when the partner (e.g., the recipient in a dictator game, the trustee in the trust game, partner in a social dilemma) was from one's ingroup than from an outgroup. However, in comparing cooperation with ingroup versus outgroup members, we are unable to distinguish between the possible motives underlying intergroup discrimination: ingroup favoritism or outgroup derogation. Therefore, we coded additional effect sizes when possible comparing how people cooperate with ingroup and outgroup members to their cooperation with unclassified strangers. Thus, our second effect size coded for ingroup cooperation versus cooperation toward unclassified strangers (k = 79). Here a positive *d* value indicates cooperation was higher when the partner was from one's ingroup rather than someone with unknown group membership. Third, we coded for cooperation toward outgroup members versus unclassified strangers (k = 40), so that a positive *d* value indicates more cooperation when the partner belonged to an outgroup rather than being unknown. Fourth, we coded for expectations of cooperation from ingroup versus outgroup partners (k = 51). Expectations were usually elicited with short questionnaires before or after decisions to cooperate. A positive *d* value indicates that people expect more cooperation from an ingroup member than from an outgroup member.

[4] In the present meta-analysis, we do not include studies that examine ingroup favoritism with the use of matrices for several reasons. First, these methodologies are distinct from the paradigms employed in the meta-analyses, and the addition of matrices would introduce unnecessary heterogeneity in the sample of effect sizes. For example, the matrices always involve participants distributing points, money, or some resource between an ingroup and outgroup member, and the studies included in the meta-analysis involve participants exchanging resources between themselves and ingroup members or outgroup members. Second, matrix allocation decisions are only indirectly related to costly cooperation, which is the focus of the current analysis. Third, there is considerable variation in how the matrices are employed (e.g., the type of matrix) and how decisions in different matrices are analyzed. This makes it difficult, if not impossible, to include results in a thorough meta-analysis, especially since a majority of these studies were conducted between the 1970s and 1990s and the data sets are unavailable.

Some studies employed a within-subject design and only reported the means and standard deviations for each condition. When we were unable to obtain the requested correlation for the dependent variable between conditions in a within-subject design, we estimated the correlation at .30 when calculating the effect size. We note that this is a very conservative (lower) estimate of the correlation compared to the obtained correlations across studies and is based on a prior meta-analysis on personality and cooperation in social dilemma paradigms (Balliet, Parks, & Joireman, 2009).

For some studies we coded several effect sizes, and these effect sizes may be nonindependent because they involve (a subset of) the same participants and/or contained very similar methodology. In these cases we applied Cooper's (1998) shifting-units-of-analysis approach to handle nonindependent effect sizes when conducting moderator analyses. Specifically, we averaged over all the nonindependent effects from a single study that shared the same study characteristics. This method creates a single effect size for a study with multiple effect sizes that share the same coding on a specific moderator. For example, some studies reported the ingroup versus outgroup comparisons in cooperation for both men and women (Kiyonari, Foddy, & Yamagishi, 2007; Rand et al., 2009). These effect sizes are nonindependent because men and women participated in the same experiment. Therefore, prior to conducting our analyses, we create a single average effect size across both samples of men and women. We then use this single effect size when conducting our analyses. However, we do consider estimated effect sizes for men and women separately when analyzing the moderating effect of gender on the effect size. In Table 3, we indicate which studies and effect sizes have nonindependent effect sizes that have been combined during analyses.

Coding of Study Characteristics

Study characteristics were coded to test our hypotheses and to explore additional moderators of intergroup discrimination in cooperation. Table 3 provides an overview of all studies, effect sizes, and coded study characteristics. For each study characteristic reported below, we report the number of ingroup versus outgroup contrasts coded in each category.

Interdependence and (in)direct reciprocity. We classified effect sizes as derived from dictator games ($k = 38$), trust games ($k = 34$), and social dilemmas ($k = 76$; including both prisoner's dilemmas, $k = 55$, and public-goods/resource dilemmas, $k = 21$). To test Hypothesis 5 we contrast dictator games (without outcome interdependence) against both trust games and social dilemmas (that contain outcome interdependence). To test Hypothesis 6 we contrast trust games (that involve sequential decisions) with social dilemmas (that involve simultaneous decisions).

Some studies employed paradigms that could not be neatly classified as one of the paradigms mentioned above, and were coded as "other" ($k = 20$). Importantly, each of these "other" paradigms includes personally costly decisions that provided some benefit to another person. For example, we coded studies that employed the faith game ($k = 5$; see Kiyonari et al., 2007), which was developed as a variant of the trust game whereby the investor decides either not to trust and receive 10 euros from the experimenter or to trust and have the trustee decide how much of 30 euros to split with the investor. However, unlike the trust game, the trustee in the faith game does not know the investor has made this decision and decides regardless how to split the 30 euros. We also coded studies that employed the stag hunt game where, unlike with the social dilemmas, mutual cooperation results in the best outcome for everyone, compared to unilateral defection, but that unilateral defection results in a better outcome than both unilateral cooperation and mutual defection ($k = 4$; see also Table 1).

Reputation: Common versus unilateral knowledge of group membership. We coded whether both the participant and the interaction partner were knowledgeable about group membership (common knowledge; $k = 153$) or only the participant but not the partner held such knowledge (unilateral knowledge; $k = 15$). Hypothesis 7 states that there will be greater intergroup discrimination in the common knowledge, compared to unilateral knowledge, studies.

Additional study characteristics. As noted, we explored a number of potentially relevant study characteristics. First, we coded whether the study used experimentally created ($k = 91$) or existing ($k = 77$) group membership. Examples of natural group categories are national identity (Bogach & Leibbrandt, 2012; Whitt & Wilson, 2007), race (Wrightsman et al., 1972), political party (Rand et al., 2009), school identity (Van Vugt & Hart, 2004), and sport fan (Platow et al., 1999). Second, we coded the country of participants. Most studies were conducted in the United States ($k = 37$), followed by Japan ($k = 27$), Italy ($k = 14$), United Kingdom ($k = 8$), Germany ($k = 10$), Canada ($k = 6$), Switzerland ($k = 5$), Netherlands ($k = 5$), and Belgium ($k = 4$). Other countries represented in the sample include Bangladesh, China, Croatia, India, Israel, New Zealand, Palestine, Papua New Guinea, Sweden, and Uganda. Third, we coded the percentage of men in the sample (0.00 = all women, 1.00 = all men; $k = 92$; $M = 0.55$, $SD = 0.26$). Several studies and effect sizes only included women ($k = 12$) or men ($k = 20$). Some studies only reported the overall percentage of men in the entire sample, but we coded effect sizes in separate experimental conditions. When this occurred, we generalized the overall percentage of men in the sample to each experimental condition.

Fourth, we coded whether group membership was manipulated between subjects or within subject. Although in all studies participants interacted with ingroup and outgroup members, partner group membership was manipulated either between subjects ($k = 88$) or within subject ($k = 80$). In a between-subjects design, participants were randomly assigned to interact with either an ingroup member or an outgroup member. However, in a within-subject design, participants were assigned to interact with an ingroup (or outgroup) member and then with a different person who was an outgroup (or ingroup) member. These conditions were often counterbalanced to avoid potential order effects.

Fifth, we coded whether the personal cost to cooperate was hypothetical ($k = 9$) or involved some real monetary cost to the individual ($k = 159$). Sixth, we assessed whether participants interacted in a dyad with only one other person from their ingroup or the outgroup ($k = 148$) or in a group of three or more persons who belonged to the ingroup or outgroup ($k = 11$). Group size was also coded as a continuous variable (dyads to nine-person groups). Seventh, we coded whether participants either interacted once ($k = 157$) or more than once ($k = 10$). In several studies participants interacted many times with either an ingroup or outgroup member, but each time they were randomly assigned to a new partner. We

Table 3
Studies Included in the Meta-Analysis

Study	Sample			IN/OUT		Study characteristics						IN/STR	OUT/STR	EXP
	N	Sex	CO	*d*	95% CI	IV	DV	Kn	Cst	IT(#)	GS	*d*	*d*	*d*
Ahmed (2007)	96	.52	SE	0.74	[0.26, 1.22]	M	PD	C	$	OS	2	0.63	−0.13	
Study 2	100	.52	SE	0.73	[0.28, 1.19]	M	PD	C	$	OS	2	0.42	−0.32	
Ando (1999)	36	.50	JP	0.58	[0.23, 0.93]	M	PGD	C	$	IT (3)	9			
Balliet et al. (2014)[a]	432	.54	US	0.83	[0.72, 0.94]	N	PD	C	H	OS	2			
Study 2[a]	149	.61	US	0.47	[0.30, 0.64]	N	PD	C	H	OS	2			
Banuri et al. (2013)	36	.77	US			N	TG	C	$	OS	2	0.53		
Sample b	34	.72	US			N	TG	C	$	OS	2	0.25		
Sample c	39	.72	US			N	TG	C	$	OS	2	0.17		
Sample d	39	.72	US			N	TG	C	$	OS	2	0.43		
Sample e	36	.77	US			N	DG	C	$	OS	2	0.53		
Sample f	39	.72	US			N	DG	C	$	OS	2	0.10		
Sample g	34	.72	US			N	DG	C	$	OS	2	0.08		
Sample h	39	.72	US			N	DG	C	$	OS	2	0.21		
Baxter (1973)	90	.00	US	0.36	[−0.06, 0.77]	N	PD	C	$	IT (30)	2			
Bogach & Leibbrandt (2012)	54 (90)	.32	##	0.55	[−0.06, 0.77]	N	SH	C	$	OS	2	0.28	−0.05	
Sample b	54 (72)	.32	##	0.03	[−0.35, 0.42]	N	SH	C	$	OS	2	0.03	0.00	
Sample c	54 (90)	.32	##	0.10	[−0.13, 0.33]	N	DG	C	$	OS	2	0.00	−0.10	
Bohm et al. (2013)	60	.38	DE			M	PD	C	$	OS	3		−0.76	
Sample b	110	.38	DE			M	PD	C	$	OS	1		0.22	
Study 2	48	.23	DE			M	O	C	$	OS	3		−0.70	
Bouas & Komorita (1996)	80	.00	US			M	PGD	C	$	OS	4	0.00		
Bouckaert & Dhaene (2004)	47	1.00	BE	0.23	[−0.35, 0.80]	N	TG	C	$	OS	2			
Sample b	42	1.00	BE	0.12	[−0.48, 0.73]	N	TG	C	$	OS	2			
Buchan et al. (2006)	188		##	0.19	[−0.10, 0.48]	M	TG	C	$	OS	2			
Castro (2008)[a]	48		IT	1.17	[0.53, 1.82]	N	PGD	C	$	IT (10)	4			
Sample b[a]	48		IT	−0.02	[−0.62, 0.57]	N	PGD	C	$	IT (10)	4			
Sample c[a]	48		UK	1.34	[0.68, 1.99]	N	PGD	C	$	IT (10)	4			
Sample d[a]	48		UK	3.08	[2.21, 3.96]	N	PGD	C	$	IT (10)	4			
Charness et al. (2007)[a]	78		US	0.06	[−0.09, 0.22]	M	PD	C	$	OS	2			
Sample b[a]	78		US	0.77	[0.61, 0.92]	M	PD	C	$	OS	2			
De Cremer & Van Vugt (1999)	95	.65	UK			N	PGD	C	$	OS	6	0.44		
Study 2	93	.46	UK			N	PGD	C	$	OS	6	0.67		
Study 3	94		UK			N	PGD	C	$	OS	6	0.74		
De Cremer et al. (2008)	108	.37	NL			N	PGD	C	$	IT ()	5	0.11		
De Cremer & Van Vugt (1998)	93		UK			N	PGD	C	$	OS	6	0.58		
De Cremer & Van Vugt (2002)	94	.40	UK			N	PGD	C	$	OS	6	0.55		
Study 2	72		UK			N	PGD	C	$	OS	6	−0.05		
Degli Antoni & Grimalda (2012)	16		IT			N	TG	C	$	OS	2	−0.33		
Sample b	11		IT			N	TG	C	$	OS	2	−1.39		
Sample c	18		IT			N	TG	C	$	OS	2	−0.32		
Sample d	39		IT			N	TG	C	$	OS	2	0.32		
Sample e	23		IT			N	TG	C	$	OS	2	−0.15		
Sample f	31		IT			N	TG	C	$	OS	2	0.24		
Sample g	26		IT			N	TG	C	$	OS	2	−0.36		
Sample h	11		IT			N	TG	C	$	OS	2	2.08		
Sample I	58		IT			N	TG	C	$	OS	2	0.34		
Sample j	19		IT			N	TG	C	$	OS	2	0.15		
Dion (1973)[a]	22	1.00	CA	0.46	[0.02, 0.89]	M	PD	C	$	IT (20)	2			
Sample 2[a]	20	1.00	CA	0.00	[−0.44, 0.44]	M	PD	C	$	IT (20)	2			
Dorrough & Glockner (2013)	72		DE	0.21	[−0.07, 0.48]	N	PD	C	$	OS	2			0.24
Study 2	96		DE	0.28	[0.04, 0.52]	N	PD	C	$	OS	2			0.36
Dugar & Shahriar (2013)	60	.66	IN	0.20	[−0.32, 0.72]	M	TG	C	$	OS	2			
Sample b	60	.66	IN	0.75	[0.19, 1.30]	N	TG	C	$	OS	2			
Sample c	60	.66	IN	0.56	[0.02, 1.10]	N	TG	C	$	OS	2			
Fershtman et al. (2005)	51		BE	0.93	[0.35, 1.51]	N	TG	C	$	OS	2	0.07		
Sample b	51		BE	0.68	[0.11, 1.25]	N	TG	C	$	OS	2	−0.13		
Study 2	50		IL	0.59	[0.02, 1.16]	N	TG	C	$	OS	2		−0.01	
Foddy et al. (2009)	15	.13	AU			N	DG	C	$	OS	2			0.08
Sample 1b	15	.13	AU			N	DG	C	$	OS	2			1.06
Study 2	33	.13	AU			N	DG	C	$	OS	2			0.43
Sample 2b	34	.13	AU			N	DG	C	$	OS	2			−0.40
Sample 2c	38	.13	AU			N	DG	C	$	OS	2			0.92

(*table continues*)

Table 3 (*continued*)

	Sample			IN/OUT		Study characteristics						IN/STR	OUT/STR	EXP
Study	*N*	Sex	CO	*d*	95% CI	IV	DV	Kn	Cst	IT(#)	GS	*d*	*d*	*d*
Sample 2d	35	.13	AU			N	DG	C	$	OS	2			0.75
Fowler & Kam (2007)	173	.44	US	0.20	[0.05, 0.35]	N	DG	C	$	OS	2	0.10	−0.16	
Study 2	54	.44	US	0.27	[0.00, 0.54]	N	DG	C	$	OS	2			
Study 3	78	.44	US	0.19	[−0.03, 0.41]	N	DG	C	$	OS	2	0.19	0.11	
Gaertner et al. (2006)	76	.50	US			M	PD	C	$	OS	2	0.53		
Giner-Sorolla et al. (2013)	70	.19	UK	0.12	[−0.35, 0.59]	N	DG	C	$	OS	2			
Study 2	77	.20	UK	−0.25	[−0.70, 0.20]	M	DG	C	$	OS	2			
Goerg et al. (2013)	40		IL	0.39	[−0.23, 1.02]	N	PD	C	$	OS	2			0.27
Sample 2	40		PAL	0.71	[0.07, 1.35]	N	PD	C	$	OS	2			0.38
Goette et al. (2006)	116	1.00	CH	0.44	[0.02, 0.86]	N	PD	C	$	OS	2			0.36
Goette, Huffman, & Meier (2012)	111	1.00	CH	0.22	[−0.19, 0.64]	M	PD	C	$	OS	2			0.42
Sample 2	111	1.00	CH	0.47	[0.04, 0.90]	N	PD	C	$	OS	2			0.34
Goette, Huffman, et al. (2012)	140	1.00	CH	0.85	[0.42, 1.28]	N	PD	C	$	OS	2			0.38
Sample b	140	1.00	CH	0.44	[0.06, 0.82]	N	PD	C	$	OS	2			0.73
Güth et al. (2008)	32		DE	0.11	[−0.58, 0.80]	M	TG	C	$	OS	2	−0.30	−0.30	
Sample b	32		DE	−0.10	[−0.79, 0.60]	M	TG	C	$	OS	2	−0.04	0.07	
Sample c	32		DE	−0.02	[−0.71, 0.68]	M	TG	C	$	OS	2	0.18	0.19	
Sample d	16		DE	−0.08	[−1.06, 0.90]	M	TG	C	$	OS	2	−0.09	0.02	
Güth et al. (2009)	32		DE	0.40	[−0.30, 1.10]	M	DG	C	$	OS	2			0.33
Sample b	32		DE	0.47	[−0.23, 1.17]	M	DG	C	$	OS	2			0.83
Sample c	32		DE	0.16	[−0.54, 0.85]	M	DG	C	$	OS	2			0.42
Sample d	32		DE	0.18	[−0.51, 0.88]	M	DG	C	$	OS	2			0.19
Guala et al. (2013)[a]	62		IT	0.46	[−0.04, 0.96]	M	PD	C	$	OS	2			
Sample b[a]	40		IT	0.19	[−0.43, 0.82]	M	PD	C	$	OS	2			
Sample c[a]	36		IT	0.72	[0.04, 1.40]	M	PD	C	$	OS	2			
Sample d[a]	63		IT	0.25	[−0.25, 0.74]	M	PD	U	$	OS	2			
Sample e[a]	33		IT	0.21	[−0.46, 0.89]	M	PD	U	$	OS	2			
Sample f[a]	38		IT	0.00	[−0.64, 0.69]	M	PD	U	$	OS	2			
Sample g[a]	63		IT	0.76	[0.25, 1.27]	M	PD	U	$	OS	2			
Sample h[a]	33		IT	−0.03	[−0.70, 0.65]	M	PD	U	$	OS	2			
Sample I[a]	38		IT	−1.24	[−1.93, −0.54	M	PD	U	$	OS	2			
Hargreaves Heap et al. (2009)	120		UG	−0.41	[−0.77, −0.05]	N	TG	C	$	OS	2			
Hargreaves Heap & Zizzo (2009)	56		UK	0.58	[0.30, 0.87]	M	TG	C	$	OS	2	0.18	−0.35	
Sample b	48		UK	0.88	[0.54, 1.22]	M	TG	C	$	OS	2	0.29	−0.67	
Sample c	48		UK	0.63	[0.32, 0.94]	M	TG	C	$	OS	2	0.23	−0.41	
Sample d	48		UK	0.68	[0.36, 0.99]	M	TG	C	$	OS	2	0.20	−0.43	
Sample e	48		UK	0.60	[0.29, 0.91]	M	TG	C	$	OS	2	0.28	−0.33	
Holm (2001)[a]	108	1.00	SE	−0.04	[−0.46, 0.38]	N	O	C	$	OS	2			
Sample b[a]	118	.00	SE	−0.04	[−0.44, 0.36]	N	O	C	$	OS	2			
Ioannou et al. (2012)	144		US	0.58	[0.40, 0.76]	M	TG	C	$	OS	2			
Sample b	96		US	0.27	[0.06, 0.47]	M	TG	C	$	OS	2			
Sample c	96		US	0.07	[−0.03, 0.17]	M	DG	C	$	OS	2			
Sample d	96		US	0.20	[0.10, 0.30]	M	DG	C	$	OS	2			
Jackson (2008)	48	.48	US			N	PGD	C	$	OS	6	1.43		
Study 2	66	.48	US	0.51	[0.01, 1.02]	N	PGD	C	$	OS	6			
Jin & Shinotsuka (1996)	88	.50	JP	0.27	[−0.15, 0.69]	M	PD	C	$	OS	2	0.11	−0.17	0.79
Jin & Yamagishi (1997)	70	.54	JP	0.24	[−0.23, 0.71]	M	PD	U	$	OS	2	0.45	−0.01	0.80
Study 2	70	.54	JP	0.45	[−0.02, 0.93]	M	PD	C	$	OS	2	0.14	−0.09	
Johansson-Stenman et al. (2005)	128		BD	0.04	[−0.31, 0.39]	N	TG	C	$	OS	2			
Sample b	128		BD	0.18	[−0.17, 0.53]	N	TG	C	$	OS	2			
Kiyonari (2013)[a]	100	.63	JP	0.89	[0.57, 1.22]	M	SH	C	$	OS	2	0.63	−0.26	
Sample 2[a]	100	.63	JP	0.49	[0.15, 0.84]	M	SH	C	$	OS	2			
Kiyonari et al. (2007)[a]	19	1.00	AU	0.43	[−0.11, 0.97]	M	FG	C	$	OS	2			
Sample b[a]	23	.00	AU	0.30	[−0.17, 0.74]	M	FG	C	$	OS	2			
Sample c[a]	17	1.00	AU	0.00	[−0.17, 0.17]	M	TG	C	$	OS	2			
Sample d[a]	24	.00	AU	−0.15	[−0.34, 0.04]	M	TG	C	$	OS	2			
Sample e[a]	20	1.00	JP	0.36	[−0.10, 0.82]	M	FG	C	$	OS	2			
Sample f[a]	18	.00	JP	0.13	[−0.35, 0.61]	M	FG	C	$	OS	2			
Sample g[a]	25	1.00	JP	−0.27	[−0.74, 0.20]	M	TG	C	$	OS	2			
Sample h[a]	16	.00	JP	0.47	[−0.33, 1.26]	M	TG	C	$	OS	2			
Kiyonari & Yamagishi (2004)	73	.66	JP			M	RD	C	$	IT (4)	2	0.48		
Koopmans & Rebers (2009)	132	.34	NL	0.25	[0.04, 0.45]	N	PGD	C	$	OS	6	0.38	0.13	0.12
Sample b	129	.34	NL	0.27	[0.06, 0.48]	N	PGD	C	$	OS	6	0.35	0.08	0.23

(*table continues*)

Table 3 (*continued*)

Study	Sample			IN/OUT		Study characteristics						IN/STR	OUT/STR	EXP
	N	Sex	CO	*d*	95% CI	IV	DV	Kn	Cst	IT(#)	GS	*d*	*d*	*d*
Sample c	122	.34	NL	−0.01	[−0.22, 0.20]	N	PGD	C	$	OS	6	0.17	0.18	0.07
Kramer & Goldman (1995)[a]	80		US	0.43	[−0.02, 0.87]	M	RD	C	$	IT ()	5			
Lei & Vesely (2010)[a]	34		##	−0.22	[−0.56, 0.12]	M	DG	C	$	OS	2			
Sample b[a]	32		##	0.09	[−0.26, 0.43]	M	DG	C	$	OS	2			
Sample c[a]	34		##	−0.03	[−0.37, 0.30]	M	DG	C	$	OS	2			
Sample d[a]	32		##	0.33	[−0.03, 0.69]	M	DG	C	$	OS	2			
Sample e[a]	36		##	−0.03	[−0.35, 0.30]	M	TG	C	$	OS	2			
Sample f[a]	36		##	0.01	[−0.34, 0.35]	M	TG	C	$	OS	2			
Sample g[a]	32		##	0.00	[−0.34, 0.35]	M	TG	C	$	OS	2			
Sample h[a]	32		##	−0.07	[−0.42, 0.27]	M	TG	C	$	OS	2			
McLeish & Oxoby (2007)	91		CA	0.19	[−0.01, 0.40]	M	O	C	$	OS	2			
Sample b	37		CA	0.35	[0.02, 0.68]	M	O	C	$	OS	2			
Sample c	38		CA	0.20	[−0.12, 0.52]	M	DG	C	$	OS	2			
Mifune & Yamagishi (2012)	93		JP	0.34	[0.21, 0.47]	M	PD	C	$	OS	2			
Sample b	93		JP	0.05	[−0.04, 0.15]	M	PD	U	$	OS	2			
Mifune et al. (2010)	45	.63	JP	0.61	[−0.02, 1.24]	M	DG	C	$	OS	2			
Study 2[a]	142	.54	JP	0.41	[0.08, 0.74]	M	DG	C	$	OS	2			
Sample 2b[a]	142	.54	JP	−0.10	[−0.43, 0.23]	M	DG	C	$	OS	2			
Morrison (1999)	44		PG	0.50	[−0.10, 1.10]	N	PGD	C	H	OS				1.06
Sample b	44		PG	0.31	[−0.28, 0.91]	N	PGD	C	H	OS				0.89
Sample c	44		PG	0.28	[−0.31, 0.88]	N	PGD	C	H	OS				0.67
Morita & Servatka (2013)	129		NZ	0.44	[0.02, 0.86]	M	O	C	$	OS	2	0.29	−0.15	
Sample b	118		NZ	0.29	[−0.15, 0.73]	M	O	C	$	OS	2	0.14	−0.15	
Parks (2001)	144		US	−0.05	[−0.42, 0.33]	N	PD	C	$	IT (20)	2			
Study 2	216		US	0.36	[0.10, 0.63]	N	PGD	C	$	IT (20)	2			
Platow et al. (1999)	196		AU	0.64	[0.33, 0.97]	N	PGD	C	$	OS		0.66	0.02	
Sample 2	390		AU	0.24	[0.02, 0.46]	N	PGD	C	$	OS		−0.21	−0.46	
Rand et al. (2009)[a]	58	.00	US	0.30	[−0.21, 0.82]	N	DG	C	$	OS	2			
Sample b[a]	68	1.00	US	0.65	[0.13, 1.17	N	DG	C	$	OS	2			
Study 2[a]	82	1.00	US	0.14	[−0.29, 0.58]	N	DG	C	$	OS	2			
Sample 2b[a]	58	1.00	US	0.48	[−0.04, 1.00]	N	DG	C	$	OS	2			
Study 3[a]	60	.00	US	−0.09	[−0.59, 0.42]	N	DG	C	$	OS	2			
Sample 3b[a]	82	.00	US	0.05	[−0.38, 0.48]	N	DG	C	$	OS	2			
Ruffle & Sois (2006)	171	.50	IL	0.35	[0.03, 0.66]	N	RD	C	$	OS	2			0.06
Simpson (2006)	51	.50	US	0.03	[−0.58, 0.64]	M	TG	C	$	OS	2			
Sample b	56	.50	US	0.65	[0.05, 1.26]	M	PD	C	$	OS	2			
Sample c	47	.50	US	−0.10	[−0.77, 0.56]	M	PD	C	$	OS	2			
Study 2	62	.53	US	0.18	[−0.07, 0.43]	M	PD	C	$	OS	2			
Sample 2b	52	.53	US	0.39	[0.08, 0.70]	M	PD	C	$	OS	2			
Spiegelman (2013)	116	.50	CA	0.32	[−0.16, 0.81]	M	PD	C	$	OS	2	0.29	−0.03	
Stürmer et al. (2006)	40	.43	US	−0.35	[−0.98, 0.27]	M	DG	C	H	OS	2			
Takahashi et al. (2008)	337	.63	##	0.19	[0.08, 0.30]	N	TG	C	$	OS	2			
Tanis & Postmes (2005)	62	.32	NL	0.57	[0.01, 1.14]	N	TG	C	$	OS	2			0.57
Sample b	62	.32	NL	−0.24	[−0.82, 0.34]	N	TG	C	$	OS	2			−0.14
Thompson et al. (1995)	28		US	0.31	[−0.43, 1.06]	N	DG	C	H	OS	2			
Sample b	30		US	−0.46	[−1.18, 0.27]	N	DG	C	H	OS	2			
Sample c	30		US	0.69	[−0.04, 1.43]	N	DG	C	H	OS	2			
Trifiletti & Capozza (2011)	40		IT	−0.78	[−1.42, 1.14]	N	TG	C	$	OS	2			
Sample b	40		IT	0.26	[−0.36, 0.89]	N	TG	C	$	OS	2			
Van Vugt & De Cremer (1999)	96	.34	UK			N	PGD	C	$	OS	6	0.44		
Study 2	93	.37	UK			N	PGD	C	$	IT (8)	6	0.61		
Van Vugt & Hart (2004)	60	.33	UK			N	PGD	C	$	OS	6	−0.11		
Study 3	50	.40	UK			N	PGD	C	$	IT (6)	6	0.63		
Van Vugt et al. (2007)	120	.33	UK			N	PGD	C	$	OS	6	0.07		
Study 2	43	.46	UK			N	PGD	C	$	OS	6	0.68		
Study 3	90	.53	UK			N	PGD	C	$	IT (6)	6	0.51		
Wallace & Rothaus (1969)	48	1.00	US	1.51	[0.87, 2.15]	N	PD	C	$	IT (10)	2			
Wang & Yamagishi (2005)	46	.45	CN	0.22	[−0.09, 0.53]	M	TG	C	$	OS	2			0.22
Study 2	42	.45	CN	0.05	[−0.27, 0.37]	M	FG	C	$	OS	2			0.04
Whitt & Wilson (2007)[a]	252	.50	BA	0.37	[0.22, 0.52]	N	DG	C	$	OS	2			
Study 2[a]	203	.50	HR	0.24	[0.07, 0.40]	N	DG	C	$	OS	2			
Study 3[a]	216	.50	HR	0.27	[0.11, 0.43]	N	DG	C	$	OS	2			
Wilson & Kayatani (1968)	112	.50	##	2.13	[1.67, 2.59]	M	PD	C	$	IT (20)	2			
Wilson et al. (1965)	40		US	0.79	[0.34, 1.36]	M	PD	C	$	IT (20)	2			

(*table continues*)

Table 3 (*continued*)

Study	Sample			IN/OUT		Study characteristics						IN/STR	OUT/STR	EXP
	N	Sex	CO	*d*	95% CI	IV	DV	Kn	Cst	IT(#)	GS	*d*	*d*	*d*
Winterich et al. (2009)	143	.52	US	0.35	[0.01, 0.68]	N	PGD	C	$	OS				
Study 2	258	.39	US	0.26	[0.01, 0.51]	N	PGD	C	$	OS				
Study 3	233	.45	US	0.18	[−0.08, 0.44]	N	PGD	C	$	OS				
Wit & Wilke (1992)	189	.66	NL			M	PD	C	H	OS	10	0.20		0.02
Sample b	190	.66	NL			M	O	C	H	OS	10	0.49		0.14
Sample c	180	.66	NL			M	O	C	H	OS	10	0.06		0.13
Wrightsman et al. (1972)	80	.00	US	0.03	[−0.41, 0.47]	N	PD	C	$	IT (30)	2			
Yamagishi & Kiyonari (2000)	47	.58	JP	−0.17	[−0.45, 0.12]	M	PD	C	$	OS	2			−0.21
Sample 2	44	.58	JP	0.48	[0.19, 0.76]	M	PD	C	$	OS	2			0.38
Yamagishi & Mifune (2008)	80	.54	JP	0.52	[0.06, 0.99]	M	DG	C	$	OS	2			0.69
Study 2	75	.54	JP	−0.13	[−0.60, 0.33]	M	DG	U	$	OS	2			
Yamagishi & Mifune (2009)	69	1.00	JP	0.50	[0.25, 0.75]	M	PD	C	$	OS	2	0.37	0.11	0.66
Sample b	64	.00	JP	0.50	[0.25, 0.77]	M	PD	C	$	OS	2	0.19	0.02	0.66
Sample c	69	1.00	JP	0.39	[0.15, 0.64]	M	PD	U	$	OS	2	0.61	−0.10	0.02
Sample d	64	.00	JP	0.00	[−0.25, 0.25]	M	PD	U	$	OS	2	0.51	0.15	0.00
Yamagishi et al. (2005)	56		JP	−0.22	[−0.53, 0.08]	N	PD	U	$	OS	2	−0.05	−0.05	0.20
Sample b	56		JP	−0.18	[−0.43, 0.07]	N	PD	C	$	OS	2	−0.11	0.11	0.32
Sample c	49		AU	0.08	[−0.13, 0.28]	N	PD	C	$	OS	2	0.02	0.25	0.17
Sample d	49		AU	−0.09	[−0.22, 0.05]	N	PD	U	$	OS	2	−0.05	0.02	0.10
Yamagishi et al. (2008)	48	.63	JP	0.33	[0.12, 0.54]	M	PD	C	$	OS	2			0.81
Sample b	48	.63	JP	0.06	[−0.10, 0.22]	M	PD	U	$	OS	2			0.09
Sample c	49	.42	NZ	0.40	[0.21, 0.59]	M	PD	C	$	OS	2			
Sample d	49	.42	NZ	−0.05	[−0.25, 0.15]	M	PD	U	$	OS	2			

Note. *N* = sample size; sex = the percentage of men in the sample; IN/OUT = standardized mean difference (*d* value) in cooperation when interacting with an ingroup and an outgroup member; IN/STR = standardized mean difference (*d* value) in cooperation when interacting with an ingroup member and an unclassified stranger; OUT/STR = standardized mean difference (*d* value) in cooperation when interacting with an outgroup member and an unclassified stranger; EXP = standardized mean difference (*d* value) in expected cooperation from ingroup versus outgroup members; CO = country of participants in the sample; ## = study included participants from multiple countries; SE = Sweden; JP = Japan; US = United States; DE = Germany; BE = Belgium; IT = Italy; UK = United Kingdom; NL = the Netherlands; CA = Canada; IN = India; IL = Israel; AU = Australia; UG = Uganda; CH = Switzerland; BD = Bangladesh; PG = Papua New Guinea; NZ = New Zealand; BA = Bosnia; HR = Croatia; *d* = standardized mean difference; CI = confidence interval; Kn = knowledge of group membership; C = common knowledge of group membership; U = unilateral knowledge of group membership; Cst = cost to participants; H = hypothetical costs; $ = costly behavior; IV = the type of group manipulation; N = natural groups; M = experimentally manipulated groups (e.g., a minimal group paradigm); DV = the dependent measure of cooperation; PD = prisoner's dilemma; PGD = public goods dilemma; RD = resource dilemma; TG = trust game; FG = faith game; DG = dictator game; SH = stag hunt game; O = Other; IT(#) = one-shot versus iterated interactions (number of iterations); OS = one-shot; IT = iterated (number of iterations); GS = group size.
[a] Effect size is nonindependent from another effect size and is combined during analyses.

treat these studies similar to the studies that only involve a single interaction between partners. We also coded the number of times participants interacted as a continuous variable (range: 1–30).

Results

Analytic Strategy

In our analysis, we first estimated the overall effect size using a random-effects model, along with both the 95% confidence interval (CI) and the 90% prediction interval (PI). We then considered the variation in the effect size distribution by using several indicators of heterogeneity of variance (T, T^2, and I^2). Next, we examined the possibility that the effect size distribution contains a publication bias. In so doing, we formally examine the distribution of studies in a funnel plot (plotted according to their sample size and standard error) using Egger's regression intercept and Duval and Tweedie's (2000) trim and fill approach.

Following these analyses, we use a mixed-effects model to conduct several univariate moderator analyses, and a random-effects multiple regression analysis to examine our hypotheses while controlling for several study characteristics. Analyses were conducted with Hedges and Olkin's (1985) approach with the Comprehensive Meta-Analysis software.

Overall Estimate of Intergroup Discrimination

Ingroup versus outgroup cooperation. Table 4 displays the distribution of the ingroup versus outgroup cooperation effect sizes in a stem-and-leaf plot. As can be seen, the sample contains a normal distribution of effect sizes with a considerable amount of variation. Notably, a substantial majority of studies report a positive effect size, reflecting intergroup discrimination with individuals favoring ingroup members, and very few studies actually report any negative effect sizes, reflecting some form of outgroup favoritism.

To estimate the overall ingroup versus outgroup effect size distribution, we begin by focusing on only the studies that involve common knowledge of group membership; that is, all individuals are aware of each other's group membership during an interaction. In fact, across all of the overall and univariate moderator analyses that follow, we first report analyses only including studies that involve common knowledge about group membership. We do this because (a) most studies include common knowledge, (b) common

Table 4

Stem-and-Leaf Diagram of the Overall Distribution of Effect Sizes on the Standardized Mean Difference in Cooperation When Interacting With an Ingroup Member Versus an Outgroup Member

d	.1 units of the *d* value
0.9	3
0.8	3 5 8 9
0.7	1 2 3 4 5 6 7 9
0.6	0 1 3 4 5 5 8 8 9
0.5	0 0 0 1 2 5 6 7 8 8 8 9
0.4	0 0 1 3 3 4 4 4 5 6 6 7 7 7 7 8 8 9
0.3	0 0 1 1 2 3 3 4 5 5 5 6 6 6 7 9 9 9
0.2	0 0 0 0 1 1 2 2 3 4 4 4 5 5 6 6 7 7 7 7 7 8 8 9
0.1	0 1 2 2 3 4 6 8 8 8 8 9 9 9 9 9
0.0	0 0 0 1 3 3 3 4 5 5 5 6 6 7 8 9
−0.0	0 0 1 2 2 3 3 3 4 4 5 5 7 8 9 9
−0.1	0 0 0 3 5 7 8
−0.2	2 2 4 5 7
−0.3	5
−0.4	1 6

Note. This plot excludes seven outliers: 3.08, 2.13, 1.51, 1.34, 1.17, −0.78, and −1.24.

versus unilateral knowledge has been found in primary studies to strongly influence intergroup discrimination in cooperation, and (c) the studies that involve unilateral knowledge are primarily social dilemma studies, which can suppress the estimated effect size for these studies while conducting univariate results. However, for the purposes of transparency, we report the results of the overall and univariate moderator analyses both with and without the unilateral knowledge studies.

Table 5 summarizes the overall analysis of studies of the distribution of ingroup versus outgroup effect sizes, both with and without unilateral knowledge studies. After collapsing all nonindependent effect sizes into a single effect size, the total sample of effect sizes is reduced from $k = 153$ to $k = 125$. A random-effects model results in a small to medium positive effect size, indicating that people cooperate more with ingroup members compared to outgroup members ($d = 0.32$, 95% CI [0.27, 0.38], 90% PI [−0.07, 0.73]). Moreover, there is heterogeneity in the effect size distribution ($T = 0.25$, $T^2 = 0.06$), and much of this variation may be explained by between-study differences ($I^2 = 77.75$). Including the studies that involve unilateral knowledge of group membership slightly reduces the effect size ($d = 0.29$). Overall, these results reveal the existence of intergroup discrimination in cooperation: Individuals cooperate more with ingroup members compared to outgroup members. This finding supports Hypothesis 1.

Using the trim-and-fill approach with random effects, 16 studies were added above the average effect size, which resulted in a reestimation of an average effect size slightly larger than the original average effect size ($d = 0.39$, 95% CI [0.33, 0.44]; see also Table 5). Whereas these results suggest that there is a slight bias to underestimate the effect size, Egger's regression intercept is nonsignificant, intercept = 0.27, $t(123) = 0.72$, $p = .47$. We conclude that there is no publication bias in the data.

Ingroup, outgroup, and unclassified strangers. Comparing how cooperative people are with ingroup members and outgroup members to unclassified strangers allows us to test several hypotheses. First, recall that SIT and BGR differ in their predictions that ingroup cooperation will be equal to or greater than cooperation toward unclassified strangers (Hypothesis 2a and Hypothesis 2b, respectively). Second, we can compare how people treat ingroup members and outgroup members compared to unclassified strangers to test whether intergroup discrimination is driven by ingroup favoritism (Hypothesis 8a) and/or outgroup derogation (Hypothesis 8b). Table 6 shows the overall estimated average effect sizes for each analysis. As can be seen, there was a small to medium positive effect size for cooperation with ingroup members versus unclassified strangers ($d = 0.30$, 95% CI [0.24, 0.36], 90% PI [0.02, 0.60]). Furthermore, the mean difference between cooperation with ingroup members compared to unclassified strangers is equivalent to the mean difference between cooperation with ingroup members and cooperation with outgroup members ($d = 0.30$ and $d = 0.32$, respectively). However, there is no difference between the amounts of cooperation with outgroup members, compared to unclassified strangers ($d = -0.09$, 95% CI [−0.17, 0.00], 90% PI [−0.37, 0.21]). Put differently, people cooperate more with ingroup members compared to unclassified strangers or members of outgroups, and people do not treat outgroup members differently from unclassified strangers. This finding supports Hypothesis 2b, that a salient outgroup is not necessary for the emergence of intergroup discrimination.[5] Moreover, intergroup discrimination in cooperation is due primarily to ingroup favoritism, as predicted in Hypothesis 8a, and not to outgroup derogation. Accordingly, Hypothesis 8b is rejected.

Publication bias in these two effect size distributions can be excluded. For the ingroup-versus-stranger comparison, using Duval and Tweedie's (2000) trim-and-fill approach with random effects, we find that there were only two studies added above the estimated effect size, which resulted in a slightly larger estimated effect size ($d = 0.32$). Moreover, Egger's regression intercept is nonsignificant, intercept = 0.67, $t(53) = 1.34$, $p = .19$, which indicates a lack of bias in the data. Applying the same approach to the outgroup-versus-stranger effect size, we find that two studies are added above the estimated effect size, which results in a slightly smaller estimated effect size ($d = -0.07$, 95% CI [−0.15, 0.00]). Egger's regression intercept is also nonsignificant, intercept = −1.34, $t(33) = 2.01$, $p = .051$. These findings suggest that,

[5] Experiments that report a comparison between cooperation toward ingroup members and unclassified strangers differ in the extent that an outgroup was salient during the experiment. For example, some studies contain a within-subject design, and participants interact with both ingroup and outgroup members and unclassified strangers (e.g., Yamagishi et al., 2005). Moreover, some designs induce group membership in the context of a salient outgroup, but then only observe cooperation with the ingroup and compare this to an experimental condition that observes cooperation among strangers (e.g., De Cremer & Van Vugt, 1999). Therefore, experiments that employ a between-subjects experimental manipulation of partner group membership and use experimental procedures that exclude mention of outgroups provide the strongest test of Hypotheses 2a and 2b about the necessity of a salient outgroup for intergroup discrimination in cooperation. When we analyze the comparison between cooperation toward ingroup members versus strangers in this later type of studies ($k = 21$), we continue to find that people cooperate more with ingroup members, compared to unclassified strangers ($d = 0.27$, 95% CI [0.15, 0.37]).

Table 5
Overall Average Effect Sizes, Heterogeneity, and Publication Bias

	Overall effect size				Heterogeneity			Publication bias		
Type of effect size	*k*(w/uk)	*d*(w/uk)	95% CI	90% PI	*T*	T^2	I^2	*d*(left)	*d*(right)	ER*p*
Ingroup versus outgroup cooperation	125 (136)	0.32 (0.29)	[0.27, 0.38]	[−0.07, 0.73]	0.25	0.06	77.75	0.32	0.39	0.47
Ingroup versus stranger cooperation	55 (60)	0.30 (0.28)	[0.24, 0.36]	[0.02, 0.60]	0.16	0.03	51.43	0.29	0.32	0.19
Outgroup versus stranger cooperation	35 (40)	−0.09(−0.06)	[−0.17, 0.00]	[−0.37, 0.21]	0.17	0.03	56.61	−0.09	−0.07	0.05
Ingroup versus outgroup expectations	45 (51)	0.41 (0.37)	[0.29, 0.53]	[−0.22, 1.04]	0.37	0.14	84.15	0.41	0.44	0.06

Note. *k* = the number of studies; *d* = standardized mean difference; *d*(w/uk) = the estimated effect size including studies that have unilateral knowledge; CI = confidence interval; PI = prediction interval; *d*(left/right) = reestimation of effect sizes using the trim-and-fill approach with random effects with either studies added to the left or the right; ER*p* = the *p* value for Egger's regression intercept.

if anything, the outgroup-versus-stranger comparison is a slightly smaller nonsignificant effect.

Ingroup versus outgroup expectations of partner cooperation. From both SIT and BGR, we derived Hypothesis 4, that individuals expect more cooperation from ingroup members compared to outgroup members. Table 5 reports a medium positive effect size (based on $k = 45$), indicating that participants expect more cooperation from ingroup members, compared to outgroup members ($d = 0.41$, 95% CI [0.29, 0.53], 90% PI [−0.22, 1.04]).

Indicators of publication bias suggest that this may be an underestimation of the effect size. Applying the trim-and-fill approach, two studies were added above the estimated effect size and in turn estimated a larger effect size ($d = 0.44$, 95% CI [0.31, 0.56]). Egger's regression also supports the possibility of this potential bias in the data, intercept = −1.68, $t(43) = 1.90$, $p = .06$. Thus, if anything, people expect more cooperation from fellow ingroup members, compared to outgroup members, and the estimated effect size here may be slightly underestimated. Hypothesis 4 is supported.

Moderators of Intergroup Discrimination

We first report a series of univariate moderator analyses that test our hypotheses. This is followed by a multiple regression analysis that examines our hypotheses while statistically controlling for several study characteristics. All of these analyses only include the ingroup-versus-outgroup cooperation effect sizes, because this contrast directly provides an estimate of intergroup discrimination.

Mere categorization versus outcome interdependence. Both SIT and BGR predict (a) some amount of discrimination in the dictator games (Hypothesis 3) and (b) stronger intergroup discrimination in cooperation in social dilemmas and trust games (with outcome interdependence) compared to dictator games (without outcome interdependence; Hypothesis 5). We examine these hypotheses using only the studies that employ experimentally created group membership and compare cooperation with ingroup and outgroup members. This is the strongest approach for examining intergroup discrimination in the absence of any interdependence, because in these experimentally created groups people have no real interdependence outside the laboratory. In support of Hypothesis 3, there is a small positive bias in the dictator game to give more to ingroup members than outgroup members ($d = 0.19$, 95% CI [0.07, 0.37]). In support of Hypothesis 5, there is less discrimination in the dictator games ($d = 0.19$), compared to the social dilemmas and trust games ($d = 0.39$, 95% CI [0.30, 0.47]), $Q(1) = 4.06$, $p = .04$. As displayed in Table 7, these results do not change when we also included natural groups in the analyses.

(In)direct reciprocity. BGR predicts that when direct reciprocity is possible, people should be less willing to condition their own cooperation on their partner's group membership. In support of Hypothesis 6, intergroup discrimination in the trust game was less pronounced ($d = 0.26$, 95% CI [0.14, 0.38]) than in social dilemmas ($d = 0.42$, 95% CI [0.33, 0.51]), $Q(1) = 4.62$, $p = .03$.[6]

Reputational concerns. BGR finally implied that cooperation with ingroup members emerges especially when both the actor and the partner are aware of the shared group membership (Hypothesis 7). Table 6 shows, indeed, a significant difference between common knowledge studies, where reputational concerns are present, and unilateral knowledge studies, where reputational concerns are eliminated, $Q(1) = 10.08$, $p = .001$. In fact, intergroup discrimination in cooperation emerges in common knowledge studies ($d = 0.32$, 95% CI [0.27, 0.37] but not in unilateral knowledge studies ($d = 0.04$, 95% CI [−0.12, 0.21]). Hypothesis 7 is supported.

Auxiliary Analyses

Natural versus manipulated (minimal) groups. There is no significant difference between the results of studies using natural or experimentally manipulated groups, $Q(1) = 0.84$, $p = .36$ (natural groups, $d = 0.30$, 95% CI [0.21, 0.37], versus experimentally created group memberships, $d = 0.35$, 95% CI [0.27, 0.42]).

Country of participants. When analyzing variation across countries, we only include studies that are represented by four or more effect sizes. As displayed in Table 7, taking this approach resulted in nine countries included in the analysis, with no systematic variation across countries, $Q(8) = 12.57$, $p = .13$.

Sex differences. We were able to code gender for 90 studies. Intergroup discrimination in cooperation is somewhat stronger in studies that include more (if not all) men, compared to women ($b = 0.23$, $p = .049$). Similarly, intergroup discrimination is slightly stronger in samples of men ($k = 19$, $d = 0.34$, 95% CI [0.21, 0.49]), compared to samples of only women ($k = 11$, $d = 0.15$, 95% CI [−0.04, 0.35]), $Q(1) = 2.29$, $p = .13$, but these differences do not meet traditional levels of statistical significance. However, considering just the percentage of males in the mixed

[6] In the categorization of social dilemmas, we include both prisoner's dilemmas and public goods dilemmas. Although these are conceptually similar experimental paradigms, there could be some difference in methodologies between these paradigms, and so we did consider whether the effect size varied between them. We did not find that the ingroup versus outgroup effect size differed between prisoner's dilemma studies ($d = 0.43$) and public goods dilemma studies ($d = 0.43$).

Table 6
Testing the Key Predictions on Intergroup Discrimination in Cooperation

Variable	Partner effects			Interdependence		Reciprocity		Knowledge	
	IN/OUT	IN/STR	OUT/STR	DG	SD/TG	TG	SD	Common	Unilateral
Social identity approach	+ (.32)*	0 (.30)*	0 (−.09)	+ (.15)*	++ (.39)*	+ (.25)*	+ (.43)*	+ (.32)*	+ (.04)
Bounded generalized reciprocity	+ (.32)*	+ (.30)*	0 (−.09)	+ (.15)*	++ (.39)*	+ (.25)*	++ (.43)*	+ (.32)*	0 (.04)
Ingroup favoritism	+ (.32)*	+ (.30)*	0 (−.09)						
Outgroup derogation	+ (.32)*	+ (.30)*	− (−.09)						

Note. Estimated average effect sizes for each analysis shown in parentheses. IN/OUT = difference in cooperation when facing an ingroup versus outgroup member; IN/STR = difference in cooperation when facing an ingroup member versus unclassified stranger; OUT/STR = difference in cooperation when facing an outgroup member versus unclassified stranger; DG = dictator game; SD = social dilemmas (e.g., prisoner's dilemmas and public goods dilemmas); TG = trust game; common versus unilateral = common knowledge versus unilateral knowledge about group membership; −/0/+ = expected direction of the effect size; ++ = a relatively stronger positive effect across the row.
* $p < .05$.

sex studies (assessed in $k = 59$), we find that the percentage of males is significantly and positively related to the effect size for intergroup discrimination ($b = 1.12$, $p = .003$). Thus, the general pattern emerging from these analyses is that there is greater amount of intergroup discrimination in cooperation in studies that contain more (if not all) men, compared to women.[7]

Multiple Regression Analyses

Table 8 shows a few of the study characteristics are moderately correlated, and only the number of iterations and year of publication are strongly related ($r = -.72$, $p < .001$; more recent studies tend to use a fewer number of iterations). To examine whether these correlations affect the interpretation of our univariate moderator analyses, we performed several random-effects multivariate metaregressions with method of moments estimations in which we examined the difference between the dictator games, trust games, and the social dilemmas (prisoner's dilemmas, public goods dilemmas, and resource dilemmas). We created dummy variables for the dictator game, by coding the dictator games as 1 and the trust games and social dilemmas as 0, and for the trust game by coding the trust games as 1 and the dictator games and social dilemmas as 0. We first enter these dummy variables in Model 1, reported in Table 9. This model explains a significant amount of variation in the effect size. The dictator game dummy has a significant negative relation with the effect size ($\beta = -.20$, $p = .027$), indicating more intergroup discrimination in social dilemmas, compared to dictator games. The trust game dummy is not significant.

In Models 2–8 (see Table 9) we sequentially control for several other study characteristics (note that in Model 8, where we control for year of publication, we remove from the model the highly correlated variable number of iterations). While the trust game dummy initially had a nonsignificant relation with the effect size in Model 1, this became significant after controlling for common knowledge in Model 2 and throughout the remaining models (except in Model 8, where the trust game dummy became marginally significant; $\beta = -.20$, $p = .053$). In Model 2 we find that studies with common knowledge of group membership obtained stronger intergroup discrimination, compared to unilateral knowledge of group membership ($\beta = -.32$, $p < .001$). When statistically controlling for common knowledge, we continue to find that the dictator game dummy has a significant negative relation with the effect size ($\beta = -.27$, $p = .003$). In fact, this relation is slightly strengthened due to the fact that most of the unilateral knowledge studies were social dilemma studies (and including them thus suppressed the estimated effect size in social dilemmas).

Across the remaining Models 4–8, we find that none of the other control variables holds any predictive value for the effect size. Importantly, across all the models both the dictator game dummy and common knowledge dummy have a significant relation with the effect size. In summary, confirming the univariate analyses, we find that ingroup favoritism is stronger in situations that contain interdependence (vs. no interdependence), in common (vs. unilateral) knowledge studies, and during simultaneous decisions (vs. sequential decisions).

Discussion

We meta-analyzed the history of research on intergroup discrimination in cooperative decision making to address three fundamental, yet unresolved, issues regarding intergroup discrimination in cooperation: (a) how robust is intergroup discrimination in cooperation, (b) what are the key boundary conditions of intergroup discrimination in cooperation, and (c) is discriminating between ingroup and outgroup members due to ingroup favoritism and/or outgroup derogation? With regard to the first question, we observed indeed that people are more inclined to incur a personal cost to provide benefits to another ingroup, compared to outgroup, member ($d = 0.32$). This moderately sized effect is robust, and it generalizes across different group size, the presence or absence of an explicit outgroup, real versus hypothetical cost of cooperation, experimental groups versus natural groups, and several different

[7] We consider additional univariate moderators, such as cost to participants, group size, iterations, and between-subjects versus within-subject manipulations of partner group membership. Comparing studies that involved cost (either direct payment or a lottery) demonstrated similar amounts of intergroup discrimination ($d = 0.32$), compared to studies with hypothetical costs ($d = 0.40$), $Q(1) = 0.49$, $p = .48$. We do not find that group size moderates the effect size ($b = 0.02$, $p = .44$). However, we note that most of these studies involve dyadic interactions with ingroup and outgroup members and there is not much range or variation in group size. Intergroup discrimination is stronger during interactions that unfold over time ($d = 0.58$), compared to one-shot interactions ($d = 0.30$), $Q(1) = 6.43$, $p = .01$. We found no difference in discrimination between ingroup and outgroup members when comparing between-subjects manipulations of partner group membership ($d = 0.34$) to within-subject manipulations ($d = 0.30$), $Q(1) = 0.56$, $p = .45$.

Table 7
Results of the Univariate Categorical Moderator Analyses

Class	*Q*	*k*	*d*(w/uk)	95% CI	*T*
Knowledge of partner's group	10.08*				
Common knowledge		125	0.32	[.027, 0.37]	0.25
Unilateral knowledge		11	0.04	[−0.12, 0.21]	0.09
Interdependence (minimal groups)	4.06*				
Dictator games		12	0.19 (0.17)	[0.01, 0.36]	0.05
Social dilemmas and trust games		42	0.39 (0.33)	[0.30, 0.47]	0.25
Interdependence (minimal + natural groups)	5.76*				
Dictator games		27	0.19 (0.19)	[0.08, 0.32]	0.07
Social dilemmas and trust games		87	0.36 (0.32)	[0.30, 0.43]	0.29
(In)direct reciprocity	4.62*				
Social dilemmas		54	0.42 (0.36)	[0.33, 0.51]	0.29
Trust games		33	0.26 (0.26)	[0.14, 0.38]	0.27
Sex differences	2.29				
All men		19	0.34 (0.34)	[0.19, 0.49]	0.27
All women		11	0.15 (0.13)	[−0.04, 0.35]	0.20
Type of group	0.84				
Natural		63	0.30 (0.28)	[0.21, 0.37]	0.29
Manipulated		62	0.35 (0.31)	[0.27, 0.42]	0.23
Country of participants	12.57				
Belgium		4	0.49 (0.49)	[0.11, 0.88]	0.23
Canada		5	0.26 (0.26)	[0.00, 0.52]	0.00
Germany		10	0.19 (0.19)	[−0.06, 0.43]	0.00
Italy		4	0.20 (0.17)	[−0.15, 0.56]	0.44
Japan		18	0.30 (0.23)	[0.16, 0.43]	0.24
Netherlands		5	0.17 (0.17)	[−0.10, 0.44]	0.14
Switzerland		5	0.48 (0.48)	[0.19, 0.77]	0.07
United Kingdom		8	0.63 (0.63)	[0.41, 0.86]	0.43
United States		32	0.31 (0.31)	[0.20, 0.42]	0.28
Cost to participant	0.49				
Hypothetical costs		8	0.40 (0.40)	[0.15, 0.65]	0.50
Real costs		117	0.32 (0.29)	[0.27, 0.37]	0.21
Iterations	6.43*				
One-shot		115	0.30 (0.27)	[0.24, 0.36]	0.23
Iterated		9	0.58 (0.58)	[0.37, 0.80]	0.57
Group membership manipulation	0.56				
Between subjects		70	0.34 (0.33)	[0.26, 0.43]	0.33
Within subject		55	0.30 (0.27)	[0.23, 0.38]	0.23

Note. *d*(w/uk) = standardized mean difference in cooperation with ingroup members compared to outgroup members excluding studies with unilateral knowledge (estimated *d* value including unilateral knowledge studies); CI = confidence interval.
* $p < .05$.

countries. Below, we address the other two key issues in light of our results, discuss implications for SIT and BGR, and highlight avenues for new research on intergroup discrimination in cooperation.

Social Identity Approach and Bounded Generalized Reciprocity

Two psychological theories have been proposed to explain why humans would cooperate more with ingroup members compared to outgroup members. While SIT focuses on how people discriminate to maintain a positive social identity with their ingroup, BGR states that humans possess an adaptation to cooperate with ingroup members in order to maintain a positive reputation and avoid the cost of exclusion from the group. Both theories find some support in the meta-analysis, because people cooperate more with ingroup members, compared to outgroup members. Yet, each theory discusses different psychological processes that explain intergroup discrimination in cooperation and forward predictions about the boundary conditions of intergroup discrimination. Below we address several of these predictions.

Is an explicit outgroup necessary for ingroup favoritism? SIT advances the metacontrast principle that the presence of an outgroup is necessary for ingroup categorization and subsequent discrimination (Turner et al., 1987). According to this line of reasoning, there should be no meaningful difference in cooperation between ingroup members and unclassified strangers. Additionally, some traditional evolutionary perspectives suggest that outgroups are necessary for the evolution of ingroup favoritism (e.g., Alexander, 1979; Choi & Bowles, 2007; Kurzban & Leary, 2001; Van Vugt, De Cremer, & Janssen, 2007), which may imply that an outgroup is necessary for the display of ingroup favoritism.

In contrast to these traditional views, however, we find that people do cooperate more with ingroup members compared to unclassified strangers, which suggests that the presence of an outgroup is not necessary for ingroup favoritism in cooperation to

Table 8
Correlations Between Study Characteristics Included in the Multiple Regression Model

Variable	1	2	3	4	5	6	7	8	9
1. Dictator game dummy	—		−.10	−.14*	−.17	.00	.05	−.15	.12
2. Trust game dummy		—	−.19*	−.25	.15	.08	.05	−.16	.16
3. Common versus unilateral knowledge			—	−.07	.07	−.20*	−.16	−.07	.07
4. Number of iterations				—	.06	.05	.10	.02	−.76*
5. Hypothetical versus real costs					—	−.17*	−.16	.05	.16
6. Manipulated versus natural groups						—	.23*	.09	−.01
7. Within subject versus between subjects							—	−.05	.06
8. Group size								—	−.03
9. Year of publication									—

* $p < .05$.

emerge. This conclusion fits more recent evolutionary models that conjecture that ingroup favoritism can evolve in the absence of intergroup competition (e.g., Fu et al., 2012; García & van den Bergh, 2011; Masuda, 2012). Moreover, this conclusion is bolstered by previous research showing that ingroup favoritism in cooperation can occur in the absence of an explicit outgroup comparison and that group membership may form merely by making participants interdependent, such as sharing a common fate (e.g., Brewer & Kramer, 1986; Gaertner et al., 2006; Kiyonari & Yamagishi, 2004; Kramer & Goldman, 1995; Tan & Zizzo, 2008; Wit & Kerr, 2002; Wit & Wilke, 1992). This finding also supports a BGR perspective that only cues of ingroup membership are necessary to elicit enhanced cooperation toward ingroup members (Yamagishi et al., 1999). Thus, we conclude that ingroup favoritism in cooperation emerges in the absence of intergroup competition, or (symbolic) intergroup comparison. A salient outgroup is not necessary to give rise to ingroup favoritism in cooperation.

Is mere categorization sufficient to spark ingroup favoritism? At the outset, we noted the long-standing debate regarding the role of mere categorization and interdependence as sufficient conditions for ingroup favoritism. SIT emphasizes the importance of categorizing groups and then forming a social identity that people are motivated to maintain as positive (Tajfel et al., 1971; Turner et al., 1987). However, research supporting this perspective using the minimal group paradigm has been criticized for not completely ruling out interdependence, and some studies even showed that removing the interdependence between ingroup members effectively eliminated the display of ingroup favoritism (Gaertner & Schopler, 1998; Ng, 1981; Rabbie et al., 1989; Yamagishi et al., 1999; Yamagishi & Kiyonari, 2000).[8] This notwithstanding, when we considered intergroup discrimination in dictator games that lack mutual interdependence, we did find a small amount of ingroup favoritism in the dictator games ($d = 0.19$), implying that interdependence is not needed for ingroup favoritism to emerge.

SIT explains this small effect in terms of mere categorization and concomitant identification. BGR predicts that such an effect in dictator games emerges because people are concerned about their reputation even when there is no interdependence with that ingroup member (Yamagishi & Mifune, 2008). Although people do not expect to receive direct benefits from their partner (who cannot return the favor, given the structure of the dictator game), they do expect to gain the indirect benefits of a positive reputation. Fitting this possibility, primary studies showed that behavior in dictator games can affect people's reputation in groups (Milinski, Semmann, & Krambeck, 2002; Wedekind & Milinski, 2000) and that people give more to ingroup members in dictator games in the presence of cues of others watching (Mifune et al., 2010). Accordingly, new research is needed to unravel whether ingroup favoritism in situations lacking interdependence emerges because of social identity concerns, reputational concerns, or perhaps some combination of these two.

Does interdependence augment ingroup favoritism? Although the above analyses suggest that ingroup favoritism may occur in the absence of outcome interdependence, both SIT and BGR predict that interdependence augments ingroup favoritism. SIT assumes that outcome interdependence strengthens the tendency to see other ingroup members in a positive light (e.g., trustworthy), and BGR suggests that when the participants' own outcomes are jointly determined by their own and their partner's behavior, then they will base their decisions on a generalized expectation that ingroup members will be kind and cooperative to each other (Brewer, 2008; Yamagishi et al., 1999). In line with these ideas, we indeed found that people expected more cooperation from ingroup members, compared to outgroup members ($d = 0.41$).

We also found that there was a greater amount of cooperation with ingroup compared to outgroup members in social dilemmas with substantial outcome interdependence ($d = 0.47$, in the minimal group studies), compared to dictator games that lack outcome interdependence ($d = 0.19$). In social dilemmas, partner group

[8] More specifically, minimal group studies that employed the matrices to study ingroup favoritism (e.g., Tajfel et al., 1971) still contained some interdependence that has been demonstrated to affect ingroup favoritism. In the matrix games, participants simultaneously allocate some points between ingroup and outgroup members, and at the same time other ingroup and outgroup participants are making decisions that affect the participant's own outcomes. Rabbie et al. (1989) found that when a participant's own rewards were solely determined by outgroup members, this resulted in the participant's actually favoring the outgroup members, compared to ingroup members. Additionally, Karp, Jin, Yamagishi, & Schinostsuka (1993; in Japanese and reported in Yamagishi et al., 1999) provided a fixed reward to each participant and found that this eliminated ingroup favoritism. Gaertner and Schopler (1998) also found that positive intragroup interdependence was enough to form perceptions of a group and ingroup favoritism, in the absence of an explicit outgroup comparison (see also Gaertner & Insko, 2000; Gaertner et al., 2006). This research strongly indicates that interdependence is an important cause of ingroup favoritism and that it may even be a necessary and sufficient condition for ingroup favoritism.

Table 9
Multiple Regression Model Testing How the Study Characteristics Moderate Ingroup Favoritism Across All Studies Included in the Meta-Analysis

Variable	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7		Model 8	
	β	*p*	β	*p*	β	*p*	β	*p*	β	*p*	β	*p*	β	*p*	β	*p*
Dictator game dummy	−.20	.027	−.27	.003	−.29	.003	−.29	.003	−.30	.002	−.30	.002	−.29	.005	−.26	.010
Trust game dummy	−.13	.161	−.21	.022	−.23	.018	−.23	.021	−.23	.019	−.24	.017	−.23	.030	−.20	.053
Common versus unilateral knowledge			−.32	.001**	−.33	.001**	−.33	.001**	−.32	.001**	−.31	.001	−.31	.001	−.29	.002
Group size					−.01	.842	−.02	.860	−.02	.824	−.01	.871	−.01	.921	−.00	.942
Hypothetical versus real costs							−.04	.694	−.03	.732	−.03	.765	−.03	.756	−.02	.825
Manipulated versus natural groups									.03	.714	.02	.837	.01	.870	.03	.774
Within subject versus between subjects											.09	.286	.09	.328	.08	.353
Number of iterations													.04	.702		
Year of publication															−.16	.067
R^2	.04*		.13*		.14*		.14*		.14*		.15*		.15*		.18*	

Note. $k = 125$ in Models 1 and 2; $k = 118$ in Models 3–8. In Model 8, we remove the variable number of iterations when adding year of publication because these variables are strongly correlated (see Table 8). Codings of the dichotomous predictors are the following: dictator game dummy, 1= dictator game, 0 = trust game and social dilemmas; trust game dummy, 1 = trust game, 0 = dictator games and social dilemmas; 1 = common knowledge of group membership, 2 = unilateral knowledge of group membership; 1 = hypothetical behaviors, 2 = costly behaviors; 1 = manipulated groups, 2 = natural groups; 1 = within-subject experimental manipulation of partner group membership, 2 = between-subjects experimental manipulation of partner group membership.
* $p < .05$. ** $p < .001$.

membership explains about 5% of the variation in cooperation, which is a pronounced difference from the 1% in the dictator games. This particularly steep increase in ingroup favoritism may be explained by a generalized trust in ingroup members. Yamagishi and Mifune (2008) found that expectations of either ingroup or outgroup members' behavior fully mediated ingroup favoritism in cooperation, and that this proved to be a better explanation than a measure of social identity. We also found that the overall mean difference in expectations of cooperation from ingroup and outgroup members has a strong positive correlation with the overall mean difference in cooperation toward ingroup and outgroup members.[9] This finding further supports the position that ingroup favoritism during interdependent decision making may be driven by perceptions of ingroup members' trustworthiness. Yet, additional studies can help further uncover the psychological mechanism underlying the effect of interdependence on ingroup favoritism in cooperation.

Does the possibility for direct reciprocity weaken ingroup favoritism? According to BGR, ingroup favoritism emerges because of the generalized expectation that cooperation with ingroup members provides indirect benefits—from others than one's current interaction partner. BGR also assumes that when possibilities for direct reciprocity exist, cues of a partner's group membership become less important and people rely, in their cooperative decision making, more on the expectation that interaction partners will respond in kind to one's cooperation (i.e., the norm of direct reciprocity; Axelrod & Hamilton, 1981; Gouldner, 1960; Komorita et al., 1992). We reasoned that the possibility for indirect reciprocity is contained in one-shot social dilemmas as well as in trust games, but that direct reciprocity is also possible for investors in trust games (who move first and then the trustee responds). Indeed, the potential for direct reciprocity is the key difference between the sequential decisions in the trust game and simultaneous decisions in social dilemmas (Hayashi, Ostrom, Walker, & Yamagishi, 1999; Nowak & Sigmund, 2005), and previous research has manipulated the presence of direct reciprocity by observing sequential versus simultaneous exchanges in social dilemmas (Kiyonari & Yamagishi, 2004). In support of BGR, we find that the possibility of direct reciprocity via sequential exchange weakens ingroup favoritism. When controlling for common knowledge of group membership, we found a significant difference between social dilemmas (simultaneous decision making; indirect reciprocity only; $d = 0.42$) and trust games (sequential decision making; both direct and indirect reciprocity; $d = 0.26$). Thus, we conclude that whereas the possibility of direct reciprocity does not eliminate ingroup favoritism, it substantially weakens the effect of group membership—ingroup favoritism in cooperative decision making rests on expectations of indirect reciprocity within one's ingroup and is weakened by expectations of direct reciprocity.

Does reputational concern affect ingroup favoritism? According to BGR, ingroup favoritism in cooperation is the result of an evolved decision heuristic that leads individuals to strive for a reputation as a cooperator, thus securing future indirect benefits and reducing the probability of being excluded from the group. Accordingly, people should express ingroup favoritism more in situations when their partner is knowledgeable of their group membership (and reputation benefits can accumulate), compared to when their partner does not know the participant's group membership. This is exactly what we find: greater intergroup discrimination under common ($d = 0.32$) compared to unilateral knowledge of partner group membership ($d = 0.04$).

[9] We conducted a random-effects regression analysis with method of moments estimation using the ingroup-versus-outgroup standardized mean difference in expected cooperation to predict the ingroup-versus-outgroup standardized mean difference in cooperation ($k = 41$). We found a strong positive correlation between these effect sizes ($b = .55$, $p < .001$). This indicates that in studies where people expected greater amounts of cooperation from ingroup, compared to outgroup, members, they also displayed a stronger tendency to cooperate more with ingroup members, compared to outgroup members.

Our meta-analytic results confirm previous studies that have reached the same conclusion (e.g., Guala, Mittone, & Ploner, 2013; Yamagishi et al., 1999; Yamagishi, Makimura, Foddy, Kiyonari, & Platow, 2005; Yamagishi & Mifune, 2009). Thus, ingroup favoritism may secure one's positive reputation in a specific group, which results in subsequent benefits of indirect reciprocity and reduced cost of potential exclusion. Indeed, a growing body of research supports the assumption of the benefits associated with a positive reputation, such as people conditioning their own cooperation on others' reputation (Fehrler & Przepiorka, 2013; Nowak & Sigmund, 1998, 2005) and people selecting/excluding partners from exchange based on their reputation (Barclay & Willer, 2007; Feinberg, Willer, & Schultz, 2014; Macfarlan, Quinlan, & Remiker, 2013; Sylwester & Roberts, 2010).

SIT suggests that both common knowledge and unilateral knowledge conditions should result in people positively discriminating between ingroup and outgroup members to maintain a positive social identity. Yet, across experiments people consistently show a stronger tendency for discrimination in the common knowledge condition. It may be that participants in the common knowledge condition have a stronger social identity, compared to the unilateral knowledge conditions, and this may explain the higher amount of intergroup discrimination under common knowledge. However, previous studies that measured ingroup identification observed no differences in social identity between common knowledge and unilateral knowledge conditions (see Yamagishi & Mifune, 2008). Nonetheless, future work may consider alternative methods to manipulate reputational concerns that do not affect the extent that participants identify with their group members.

Overview of Support for Theories

To explain and predict intergroup discrimination in cooperation, we advanced two theoretical perspectives: SIT and BGR. An overview of these hypotheses along with the effect size estimates obtained from our meta-analysis was provided in Table 5. As can be seen, results support certain predictions from each theoretical account.

As displayed in Table 5, BGR finds support in the partner effects on ingroup favoritism, whereas we do not find support for the SIT prediction that there should be no ingroup favoritism in the absence of an outgroup (i.e., no difference in the amount of cooperation toward an ingroup member compared with an unclassified stranger). Both perspectives predict (a) some amount of discrimination in the absence of outcome interdependence (i.e., dictator games), (b) that people expect greater cooperation from ingroup members than outgroup members, and (c) these differences in expectations would translate into stronger ingroup favoritism in situations that contain outcome interdependence. Each of these three predictions received full support. We also found support for BGR and the prediction that there would be a stronger display of ingroup favoritism in situations involving simultaneous decisions (e.g., social dilemmas), compared to first-mover decisions in sequential cooperative decision task (e.g., trust games). SIT did not predict any differences in effects across sequential and simultaneous cooperative decisions. Additionally, SIT would suggest that both common and unilateral knowledge of group membership provide an opportunity for people to positively discriminate the ingroup from the outgroup. Yet, we do not find support for the prediction of similar amount of ingroup favoritism across both common and unilateral knowledge contexts. Instead, BGR is supported in predictions of greater ingroup favoritism in the common versus unilateral knowledge experiments. Although we find some support for each theoretical perspective, only BGR receives support for three unique predictions (i.e., ingroup members vs. stranger, common vs. unilateral knowledge, and simultaneous vs. sequential decisions), and each SIT prediction that finds support in the meta-analysis can also be explained by a BGR perspective.

Besides testing specific hypotheses, we explored the relevance of several additional moderators. We observed no meaningful effects of group size, the cost of cooperation, or the country where the study was conducted. The one exception to this general conclusion is that intergroup discrimination in cooperation appeared somewhat stronger when the research population studied contained more rather than fewer males. These findings on sex differences fit the argument that throughout evolution, men faced specific selection pressures (e.g., warfare, intergroup competition, and patrilocality) that selected for a male-specific coalitional psychology that promotes male–male cooperation in groups, especially in the presence of outgroups (Balliet, Li, Macfarlan, & Van Vugt, 2011; Bowles, 2009; Makova & Li, 2002; McDonald et al., 2012; see also Navarrete, McDonald, Molina, & Sidanius, 2010; Van Vugt et al., 2007). Although the overall effect was statistically significant across 90 studies, the comparison on the effect size for the 30 studies that only contained men or women were not statistically different. Perhaps the aggregate difference between men and women is not so pronounced. In fact, previous research identified several boundary conditions for sex differences in intergroup discrimination. For example, Gaertner and Insko (2000) found that women showed ingroup favoritism regardless of their degree of interdependence with others, whereas men only displayed ingroup favoritism when they were interdependent with others. In short, whereas there is some reason to conclude that ingroup favoritism in cooperation is stronger among males than females, there may be critical exceptions that warrant new research.

Ingroup Favoritism Versus Outgroup Derogation

Consistent with the claim that ingroup favoritism should be more central than outgroup derogation (e.g., Allport, 1954; Brewer, 1979; Dovidio & Gaertner, 2010; Hinkle & Brown, 1990; Mummendey & Otten, 1998), our meta-analysis showed that people cooperate more with ingroup members compared to strangers with unknown group membership (i.e., unclassified strangers). In fact, the amount of discrimination between ingroup members and unclassified strangers is very similar to the amount of discrimination between ingroup members and outgroup members ($d = 0.30$ and $d = 0.32$, respectively). Furthermore, we found no statistical difference in cooperation with outgroup members and unclassified strangers. This pattern of results supports the conclusion that intergroup discrimination in cooperation is driven by ingroup favoritism.

If intergroup discrimination in cooperation is the result of a motivation to derogate the outgroup, we should have seen that people cooperate more with an unclassified stranger, compared to the outgroup. Although there is a slight trend in that direction ($d = -0.09$), this is not statistically significant, and the effect size is exceptionally small, explaining less than 0.2% of the variance in cooperation. Moreover, these results are aligned with recent research that finds that people rarely behave in ways that harm the outgroup but instead favor behavioral options that promote the ingroup (De Dreu, 2010; De Dreu et al., 2010; Halevy, Bornstein, & Sagiv, 2008; Halevy et al., 2010; Halevy et al., 2012; Weisel & Bohm, 2014), and that ingroup love develops earlier in childhood compared with outgroup hate (Buttelmann & Böhm, 2014; Fehr, Glätzle-Rützler, & Sutter, 2013). Therefore, we conclude that intergroup discrimination in cooperation is the result of ingroup favoritism and not the result of outgroup derogation. This finding bolsters previous conclusions about ingroup favoritism based on intergroup perceptions, attitudes, and evaluations, such as subtle racism is the result of the absence of positive emotions toward an outgroup (Dovidio & Gaertner, 2010), patriotism is distinct from nationalism (Feschbach, 1994), and people only tend to favor their ingroup over outgroup during positive, but not negative, point allocations (Mummendey & Otten, 1998).

Ingroup Favoritism as a Solution and a Barrier to Human Cooperation

Humans cooperate with other nongenetically related others on a grand scale known only to a few other species (e.g., Bowles & Gintis, 2011; Wilson, 1978). How best can we explain such heightened amounts of cooperation, especially when others can take advantage of one's cooperation? One evolutionary solution to this puzzle is through adaptations by selectively providing benefits to ingroup members and excluding noncooperative members from groups (Brewer, 1979, 1999; Efferson, Lalive, & Fehr, 2008; Fu et al., 2012; Konrad & Morath, 2012; Masuda, 2012). Seen as such, discrimination in terms of cooperation may be functional, or at least provided functional benefits to individuals and/or groups in our evolutionary past. Indeed, research has found that ingroup favoritism can be an efficient solution to social dilemmas within groups (e.g., Bernhard, Fehr, & Fischbacher, 2006; Bornstein & Ben-Yossef, 1994; Bornstein, Gneezy, & Nagel, 2002; R. Chen & Chen, 2011) but can also be the cause of inefficiencies that occur between groups (Bornstein, 2003; Schwartz-Shea & Simmons, 1991).

Ingroup favoritism not only solves the problem of human cooperation. At a higher level of analysis, ingroup favoritism may provide critical impetus to intergroup hostility, conflict, and violence. The reason for this is twofold. First, through ingroup favoritism, individuals benefit their own group and deprive, directly or indirectly, outgroups of these benefits. Such deprivation easily translates into resentment among outgroup members, protest and, perhaps, aggressive action geared at the discriminating ingroup (e.g., De Dreu et al., 2010; De Dreu, Aaldering, & Saygi, 2014; Dovidio & Gaertner, 2010; Fiske, 2002; Hewstone et al., 2002). Second, ingroup favoritism strengthens the ingroup, making it function well and relatively effective. Well-functioning, strong ingroups are a potential threat to outgroups, who may fear being aggressed and subordinated by the relatively powerful ingroup (Jervis, 1976). To protect against such aggression, or to neutralize the threat, outgroups may aggress against the ingroup (as in a preemptive strike; Deutsch, 1949; Jervis, 1976), and indeed, studies using intergroup competition games provide evidence that high- compared to low-threat outgroups promote within-group cooperation and between-group competition especially among individuals with strong tendencies toward ingroup favoritism (e.g., De Dreu et al., 2010; De Dreu, Shalvi, Greer, Van Kleef, & Handgraaf, 2012).

Methodological Implications

With ingroup favoritism operating as a two-edged sword, both solving the problem of human cooperation and at a higher level of analysis creating conflict and competition, it becomes critical to understand how to improve research on this important problem. Our meta-analysis offers several implications about the methodologies of future research. First, the overall effect size is generally quite small ($d = 0.32$), and the average number of participants to obtain adequate statistical power (0.80) then is about 300. However, adding participants may not be the only strategy for increasing statistical power. The experiment may be designed in ways to increase the magnitude of discrimination, such as choosing a paradigm with mutual interdependence (e.g., a social dilemma) rather than independence (e.g., a dictator game).

Second, some researchers have suggested that the type of group (experimental vs. natural) may affect the outcomes of experiments on cooperation (Goette, Huffman, & Meier, 2012; Jackson, 2008; Tajfel & Turner, 1979). We find that experimental groups display the same amount of ingroup favoritism as natural groups. This finding fails to support previous claims derived from SIT that people possess stronger social identities with natural groups and that this may result in stronger ingroup favoritism (Jackson, 2008). One reason may be that research using natural groups may vary on characteristics that could affect discrimination (e.g., status, wealth, and majority/minority). Future experimental work on intergroup discrimination in cooperation may benefit by varying characteristics of the outgroup. A second reason may be that natural groups used in the studies included here often are social categories as opposed to a collection of people who are interdependent in some meaningful way. For example, members of certain ethnic groups may feel less interdependent than members of political parties. In fact, a limitation of this research tradition, to date, is that little work examines differences in discrimination among various groups that differ in how they are perceived as groups. Future research could, therefore, measure perceived characteristics of the groups, such as perceived entitativity (Gaertner & Schopler, 1998), to understand whether participants actually think of others as ingroup members.

Third, researchers have questioned whether people actually engage in costly acts of intergroup discrimination. According to one perspective, ingroup favoritism may simply be the result of ingroup boasting, especially when people are dividing resources, points, or things of value that do not actually have any financial consequences for the self (Yamagishi et al., 1999). Such a criticism is important to address if findings from experiments should generalize beyond the laboratory to real costly behaviors. Importantly, a strength of this research tradition is that the studies examined here involve predominantly costly forms of cooperation, but a few studies were about hypothetical costs. We found that people do engage in costly acts of ingroup favoritism, which should provide confidence that these results contain meaningful implications for real costly behaviors outside the laboratory.

Certainly, the decision-making tasks included in the meta-analysis have their unique strengths and limitations when testing theory of intergroup discrimination. As strengths, they champion internal validity by examining interactions between anonymous strangers in situations with finely crafted incentive structures that conceptualize costly acts of generosity and cooperation. These paradigms complement other approaches in the study of prosocial behavior that may contain greater external and mundane validity (e.g., helping behaviors), but that also open themselves to many confounds. Although behavior in economic games has been shown to be predictive of behavior outside the laboratory (for a review, see Van Lange, Balliet, Parks, & Van Vugt, 2014), the studies and methods meta-analyzed here should be complemented by other methods and with other subject populations.

We also note that experimental studies comparing intergroup discrimination across different decision-making tasks can provide insights on discrimination. To illustrate, here we compared the dictator game with social dilemmas and trust games to test hypotheses about the role of interdependence on discrimination and compared the latter two paradigms to test hypotheses about the role of direct reciprocity on discrimination. However, such comparisons in the meta-analysis could be affected by potential confounds that are not identified by theory. For example, while we hypothesize that differences in discrimination during the trust game and social dilemma may be due to the presence of direct reciprocity, there could be other potential differences in these paradigms that account for any observed differences (e.g., asymmetric power). However, we are unaware of any theory that would predict how these more subtle differences across the paradigms would moderate intergroup discrimination. Moreover, our conclusions here are further strengthened by the results of experiments that have directly compared discrimination in a simultaneous social dilemma, with first-mover decisions in an equivalent sequential social dilemma or trust game (Kiyonari & Yamagishi, 2004; Simpson, 2006). Nonetheless, future research may benefit by directly manipulating key features of the social context in these cooperative decision-making tasks that are predicted to influence discrimination. For example, cardinal payoffs can be changed in such a way that specific motivation for (non)cooperation (e.g., fear of exploitation, or greed) can be made stronger or weaker without the structure of the game being affected (e.g., Ahn, Ostrom, Schmidt, Shupp, & Walker, 2001; De Dreu et al., 2012).

Last, many studies employ a within-subject design when studying ingroup favoritism in cooperation. In these studies, participants are placed into groups and then are assigned to interact with ingroup members and outgroup members on different trials (often counterbalanced). Such methodologies give rise to concerns about contrast effects that may enlarge the effect of ingroup favoritism and overestimate the phenomenon. However, we find no statistical difference when comparing the effect size in these studies to the effect size in between-subjects designs. Therefore, we can conclude that the findings and conclusions from within-subject designs for the study of ingroup favoritism may not be explained by a contrast effect.

Concluding Remarks

Summarizing four decades of experiments on intergroup discrimination in cooperation, we find that people demonstrate a consistently small positive preference to incur a personal cost to provide benefits to ingroup members, compared to outgroup members. We showed that people display more ingroup favoritism when their own outcomes depend on their partner's behavior, in the absence of any potential for direct reciprocity, and when their reputation is at stake. We also found that intergroup discrimination is due to ingroup favoritism, not outgroup derogation. While our findings do fit core propositions advanced in SIT, the results fit better a BGR perspective on ingroup favoritism in cooperation.

The robust observation that people cooperate, at a cost to themselves, with members of their ingroup shows that people care not only about their own outcomes but also about those of others with whom they interact and share group membership. Whereas this is a positive take-away from the current meta-analysis, we must be mindful of the potential effects such ingroup favoritism can have on intergroup competition and conflict. Future research should not only delve deeper into the cognitive, affective, and motivational processes underlying this phenomenon but also, eventually, provide the tools and instruments that enable people to strike a healthy balance between promoting within-group cooperation and reducing intergroup conflict.

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