Article

The Extended Contact Hypothesis: A Meta-Analysis on 20 Years of Research

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Abstract

According to the extended contact hypothesis, knowing that in-group members have cross-group friends improves attitudes toward this out-group. This meta-analysis covers the 20 years of research that currently exists on the extended contact hypothesis, and consists of 248 effect sizes from 115 studies. The aggregate relationship between extended contact and intergroup attitudes was r = .25, 95% confidence interval (CI) = [.22, .27], which reduced to r = .17, 95% CI = [.14, .19] after removing direct friendship's contribution; these results suggest that extended contact's hypothesized relationship to intergroup attitudes is small-to-medium and exists independently of direct friendship. This relationship was larger when extended contact was perceived versus actual, highlighting the importance of perception in extended contact. Current results on extended contact mostly resembled their direct friendship counterparts, suggesting similarity between these contact types. These unique insights about extended contact and its relationship with direct friendship should enrich and spur growth within this literature.

Keywords

extended contact, indirect contact, vicarious contact, contact hypothesis, intergroup contact, cross-group friendship

In studying prejudice reduction, attention has mostly focused on personal contact between members of different social groups and the conditions under which such contact improves intergroup attitudes (Allport, 1954). Since this pioneering work, cross-group contact has been deemed a major route to attitude change. Recent evidence suggests that a particular type of cross-group contact, close friendship, is especially important and facilitates not only tolerance but also positive regard toward the other group (Davies, Tropp, Aron, Pettigrew, & Wright, 2011; Pettigrew, 1997). An extension of this idea proposes that personally having cross-group friends is not necessary, and that merely knowing about friendships between in-group and out-group members also achieves this effect (Wright, Aron, McLaughlin-Volpe, & Ropp, 1997). The idea that this indirect cross-group experience, called *extended contact*, improves intergroup attitudes has become an integral part of the intergroup relations literature.

Since its inception 20 years ago, a great deal of research has demonstrated the intergroup benefits of extended contact. A recent comprehensive narrative review (Vezzali, Hewstone, Capozza, Giovannini, & Wölfer, 2014) summarized this body of research into a new theoretical model that outlines the antecedents and intergroup consequences of extended contact as well as the mediators and moderators of its relationship to such consequences. This meta-analysis complements and extends this earlier review by providing a systematic quantitative overview of the extended contact hypothesis while couching theoretical discussions thereof within this model.

Evidence for the Extended Contact Hypothesis

The extended contact hypothesis has received a good deal of empirical support. This support comes from dozens of studies from multiple countries that used various methods, participants, groups, as well as indices of extended contact and intergroup attitudes. It also comes from experimental studies, which support the hypothesized causal direction from extended contact to intergroup attitudes. The extended contact hypothesis is therefore demonstrably valid and reliable across numerous considerations.

Many survey studies have found that extended contact indeed predicts intergroup attitudes. In the seminal survey study of the extended contact hypothesis, European American undergraduate students who reported knowing more

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European Americans with at least one African, East Asian, or Latin American friend also reported less affective prejudice toward and better evaluations of the corresponding out-groups (Wright et al., 1997, Study 1). Using the same population, measures, and groups, Study 2 then replicated these results for the ethnic minority participants' extended contact with and attitudes toward the European American majority. From secondary school, undergraduate, or adult community samples, Northern Irish Catholics and Protestants who reported knowing more people from their own group with friends from the other group also rated the other group more favorably along several indices: trust (Paolini, Hewstone, & Cairns, 2007, Study 3; Tam, Hewstone, Kenworthy, & Cairns, 2009, Study 2; Tausch, Hewstone, Schmid, Hughes, & Cairns, 2011), warmth (Paolini et al., 2007, Study 3; Paolini, Hewstone, Cairns, & Voci, 2004, Study 1), behavioral intentions (Christ et al., 2010, Study 2; Paolini et al., 2007, Study 3), self-reported behavior (Tam et al., 2009, Study 2), evaluations (Paolini et al., 2004, Study 1), stereotypes (Tam et al., 2009, Study 2; Turner, Tam, Hewstone, Kenworthy, & Cairns, 2013), and prejudice (Paolini et al., 2004, Study 2). Survey studies have also found the same results when looking at heterosexual secondary students' extended contact with and behavioral intentions toward homosexuals in Italy (Vezzali, Brambilla, Giovannini, & Paolo Colucci, 2017), Belgian adults' extended contact with and racism toward immigrants in Belgium (Dhont, Roets, & Van Hiel, 2011, Study 2), as well as male and female undergraduate students' extended contact with and evaluations of the other sex in Australia (Paolini et al., 2007, Study 2).

Several longitudinal survey studies have found that extended contact predicts intergroup attitudes up to 1 year later. In one such study, German elementary students' extended contact with Turkish children positively predicted their stereotypes toward this out-group 7 months later, doing so albeit less strongly than it did concurrently (Feddes, Noack, & Rutland, 2009). For a large community sample of Northern Irish Catholic and Protestant adults, extended contact with the other group positively predicted intentions to help and support them 1 year later, and it did so as strongly as it did concurrently (Christ et al., 2010, Study 2). These results suggest that extended contact's relationship to intergroup attitudes is enduring.

Experimental studies have found that experiencing extended contact by hearing or reading about friendships between in-group and out-group members improves intergroup attitudes. In a seminal laboratory study of the extended contact hypothesis (Wright et al., 1997, Study 3), American undergraduate students in a minimal groups paradigm had two representatives from each of the two groups pair with a representative from the other group for a friendship-making activity and then report on this experience to their respective groups; after hearing about these cross-group encounters, participants from both groups reported better stereotypes of and allocated more money to the other group compared with beforehand. In the first of a series of field studies, British elementary students reported better stereotypes of and greater intentions to interact with disabled children after reading stories featuring friendships between other British children and members of this out-group compared with beforehand (Cameron & Rutland, 2006). Using the same population and outcomes, subsequent studies in this series replicated these results when control participants did not read any stories and the out-group was refugees (Cameron, Rutland, Brown, & Douch, 2006), as well as when control participants read stories without extended contact and the out-group was Indian British children (Cameron, Rutland, Hossain, & Petley, 2011).

Several studies have also found improvements in intergroup attitudes from observing interactions between in-group and out-group members, which is a special kind of extended contact called *vicarious contact*. In a seminal laboratory study of the extended contact hypothesis (Wright et al., 1997, Study 4), American undergraduate students in a minimal groups paradigm reported better stereotypes of the other group after observing a live interaction between an in-group and out-group member that implied friendship rather than unfamiliarity or hostility. In two other studies (Mazziotta, Mummendey, & Wright, 2011), German undergraduate students reported more warmth toward (Study 1), better evaluations of (Study 2), and more willingness to interact with Chinese people after watching a video interaction between another German and a Chinese person rather than one between two Germans. Again, these experimental results evidence the causal component of the extended contact hypothesis.

Although the extended contact hypothesis focuses exclusively on intergroup attitudes as the outcome, extended contact may offer additional intergroup benefits. Research has indeed found such benefits to include perceptions of out-group variability (Paolini et al., 2004); future direct friendships (Mallett & Wilson, 2010, Study 2; Schofield, Hausmann, Ye, & Woods, 2010; Vezzali, Stathi, Giovannini, Capozza, & Visintin, 2015); and competitive victimhood, perceptions of how much one's group has suffered at the hands of the other group compared with vice versa (Andrighetto, Mari, Volpato, & Behluli, 2012). The narrative review on extended contact (Vezzali et al., 2014) discusses the nonattitude outcomes in this literature in detail. As research and theory on extended contact has focused intently on intergroup attitudes as the outcome, so too does this meta-analysis. "The extended contact effect" therefore specifically refers to extended contact's effect on intergroup attitudes in this article.

Mediators of the Extended Contact Effect

Research has supported several theoretically predicted mechanisms of the extended contact effect. The model of extended contact (Vezzali et al., 2014) categorizes these

mediators as affective or cognitive, both of which may work with and influence the other. Studies that test mediation in the extended contact effect are unfortunately too few to analyze meta-analytically, so such questions are beyond the scope of this article. The hypothesized indirect routes from extended contact to positive intergroup attitudes are nonetheless important, especially given recent theoretical work on these processes (Vezzali et al., 2014). Due to space limitations, this section focuses on the four such mediation effects that have received the most research attention (see Vezzali et al., 2014, for a comprehensive review of mediation in the extended contact effect).

The four most common mediators of the extended contact effect are in-group norms, out-group norms, intergroup anxiety, and transitive inclusion-of-the-out-group-in-the-self (TIOGS). First, extended contact provides information about normative attitudes and behaviors between the two participating groups: By conveying that "we like them" (in-group norms) and that "they like us" (out-group norms), extended contact improves attitudes toward "them" through in-group conformity and reciprocal liking, respectively. Extended contact also improves attitudes toward the target out-group by reducing intergroup anxiety, the discomfort and stress that one experiences while anticipating unpleasant intergroup interactions. Finally, TIOGS is a process by which one identifies with the other group: By identifying with the in-group member and then through the in-group member's link to the cross-group friend, one adopts elements of the out-group to which this friend belongs and therefore attains an "honorary membership" therein that allows one to perceive and treat it to some extent as his or her own (see Aron et al., 2004, for more information about TIOGS). The model of extended contact (Vezzali et al., 2014) classifies intergroup anxiety as affective and the other three mediators as cognitive.

Research has indeed supported the mediations of in-group norms, out-group norms, intergroup anxiety, and TIOGS in the extended contact effect. Each of these variables uniquely mediated extended contact's relationship to intergroup warmth in a survey study of relations between Spanish people and immigrants in Spain (Gómez, Tropp, & Fernández, 2011). Two survey studies (Turner, Hewstone, Voci, & Vonofakou, 2008) using European British participants and South Asian targets found that the best fitting structural equation model is one in which extended contact predicted the four mediators, which subsequently predicted intergroup warmth (Study 1) and stereotypes (Study 2); also considering direct friendship's contribution, these models accounted for 38% and 66% of the variance in each respective index of intergroup attitudes.

Defining Extended Contact

The original definition of extended contact was "knowledge that an in-group member has a close relationship with an out-group member" (Wright et al., 1997, p. 74), which the seminal survey studies thereof (Studies 1 and 2) operationalized as the number of in-group members respondents knew who have at least one friend from the target out-group. Considering the implications of operationalizing extended contact in this manner requires noting that it comprises of several components: a relationship between the self and in-group member (i.e., direct relationship) plus one between the in-group and out-group members (i.e., extended relationship), each of which has a quantity and closeness component. Extended contact's original operational definition asks directly about the number of in-group members (who have cross-group friends), which implies that direct relationship quantity is primary in extended contact. It also specifies that the in-group and out-group members are friends, which implies that extended relationship closeness is important; in fact, such closeness is part of extended contact's conceptual definition and therefore a necessary component therein. Conversely, this definition downplays extended relationship quantity by requiring only one cross-group friendship and direct relationship closeness because such closeness is unnecessary: Through observation and hearsay, one can learn about the cross-group friendships of in-group members who one does not even know.

An overwhelming majority of the research on extended contact has operationalized it in the aforementioned manner. Although the in-group member under inquiry has varied greatly from acquaintances to family members, measures of extended contact have reliably asked about the *number* of these *in-group members* who have any *cross-group friends*. Manipulations thereof have also reliably varied the number of extended contact relationships while featuring friendships between in-group and out-group members: For example, they may show or tell about friendship behaviors (e.g., joint recreational activities) between an in-group and out-group member or between two in-group members. Operational definitions of extended contact have therefore reliably emphasized direct relationship quantity and extended relationship closeness.

Beyond this de facto method of defining extended contact, alternative methods of doing so also exist. The only such alternative definition in the literature to date looks at the number of cross-group friends that other in-group members have, which targets extended relationship quantity. The in-group members under inquiry have included participants' best friend or friends (Eller, Abrams, & Zimmermann, 2011; Feddes et al., 2009; Vezzali, Giovannini, & Capozza, 2012; Vezzali, Hewstone, Capozza, Trifiletti, & Di Bernardo, 2017), the friends who accompanied participants to the study (Hewstone, Judd, & Sharp, 2011), classmates (Aboud, Friedmann, & Smith, 2015, Study 2; Munniksma, Stark, Verkuyten, Flache, & Veenstra, 2013; Wölfer, Schmid, Hewstone, & van Zalk, 2016), family members (Meeusen, 2014), and media personalities (Lienemann & Stopp, 2013). Asking overwhelmingly about in-group members who were close to participants, these alternative measures of extended contact emphasized direct relationship closeness in addition

to extended relationship quantity. Other possible ways to define extended contact include the number of cross-group friends held by *all* the in-group members one knows, which emphasizes extended relationship quantity, and closeness to the in-group members who have cross-group friends, which emphasizes direct relationship closeness.

Note that extended contact's de facto operational definition was originally selected for purely practical instead of theoretical or empirical reasons (S. Wright, personal communication, February 14, 2014). Without any relevant research to date, whether this take on extended contact improves intergroup attitudes more effectively than any of its alternatives is debatable. Two studies touched upon this question by examining the importance of closeness in the two extended contact relationships. Contradicting this definition's de-emphasis on direct relationship closeness, a survey study (Tausch et al., 2011) found that only when the in-group members under inquiry were close to participants did extended contact between Northern Irish Catholics and Protestants predict their trust toward each other. Supporting its emphasis on extended relationship closeness, the original study of vicarious contact found better stereotypes from watching an interaction between an in-group and out-group member that implied friendship rather than unfamiliarity or hostility (Wright et al., 1997, Study 4).

Perceived or Actual Extended Contact

Another noteworthy dimension of extended contact is whether it is perceived or actual. Extended contact may be in-group members' true levels of cross-group friendship, which we call *actual extended contact*, or one's perception thereof, which we call *perceived extended contact*. Perceived extended contact can differ from actual extended contact (i.e., be inaccurate) in two ways: In-group members may have cross-group friends about which one does not know (i.e., underestimate extended contact) and one may infer that an in-group and out-group member are friends when they are not (i.e., overestimate extended contact). Perception should matter more than reality in extended contact: In-group members' real cross-group friendships are unlikely to do anything if one does not know about them and perceiving these friendships when they are not real should improve intergroup attitudes nonetheless. In fact, this knowledge is part of extended contact's core definition.

Very little research to date has compared perceived and actual extended contact in terms of their relative levels or effects on intergroup attitudes. The one exception to this lacuna is a set of two studies investigating the accuracy of perceived extended contact (Hewstone et al., 2011): Using a round robin design and European British participants, they found that reports of other group members' friendships with South Asians (Studies 1 and 2) and homosexual people (Study 2) generally matched the respective group member's own report thereof. This meta-analysis will add to this line of research by looking at the relative efficacy of perceived and actual extended contact.

Extended Contact in Context

Extended contact is merely one kind of cross-group experience. As previously mentioned, it grew from and complements theory on direct contact and friendship. Extended contact has also inspired work on other forms of *indirect contact*, which is an umbrella term describing contact with out-group members that does not involve personal interactions or relationships. To clarify the focus of this meta-analysis, we distinguish extended contact from direct contact and friendship as well as from other forms of indirect contact.

Forms of Indirect Contact

Several forms of indirect contact exist in the literature, all of which have reliably demonstrated positive effects on intergroup attitudes. The most common of such forms is *parasocial contact*, which describes contact with out-group members through the media (Schiappa, Gregg, & Hewes, 2005). *Imagined contact* is the "mental simulation of a social interaction with a member or members of an outgroup category" (Crisp & Turner, 2009, p. 234), and is akin to parasocial contact except it uses imagination as the medium. Contrasting the first person perspective inherent in these other forms of indirect contact, extended and vicarious contact occur through other in-group members and therefore take a third person perspective. In addition, vicarious contact can and often does occur through the media in which case it is classifiable along with parasocial contact as *media contact*.

Extended Contact and Direct Friendship

In comparing their effects, theory has focused on the advantages of direct friendship over extended contact. The largest such advantage is that direct friendship may improve intergroup attitudes more strongly. Evidence for this differential effect comes from two survey studies of relations between Northern Irish Catholics and Protestants in which effects decomposition analyses found that direct friendship had a larger total effect on evaluations (Study 1) and prejudice (Study 2) than did extended contact (Paolini et al., 2004). In another study (Paolini et al., 2007, Study 1), participants who reported some direct friendship and no extended contact with elderly people, mature-age students, vegetarians, or engineering students also reported less prejudice toward the corresponding out-groups than did those who reported the reverse pattern of intergroup contact. Direct friendship also predicts implicit intergroup attitudes, with meta-analytic results (Davies et al., 2011) showing that it does so as well as it does explicit intergroup attitudes; extended contact does not predict implicit intergroup attitudes, however, according

to studies examining the implicit evaluations of European British adolescents toward South Asians (Turner, Hewstone, & Voci, 2007, Studies 2 and 3) and of Italian children toward immigrants (Vezzali, Giovannini, et al., 2012).

On the contrary, extended contact also has advantages over direct friendship. The largest such advantage is practical in that it disseminates easily and is far-reaching: News about in-group members' cross-group friendships can travel to many people, on a small scale, via personal conversation or observation and, on a large scale, via media. These practical benefits would be especially important when the opportunity for direct contact is low and extended contact is the only cross-group experience available. Extended contact may also have some theoretical advantages over its direct counterpart. First, extended contact should provoke less intergroup anxiety because of its personal detachment, thereby bypassing this barrier between direct friendship and intergroup attitudes (Wright et al., 1997). Second, the division between in-group and out-group should be more salient during extended contact because of its third person perspective, which minimizes subtyping and helps generalize positivity for the cross-group friend to the entire out-group (Wright et al., 1997). Unfortunately, whereas the practical advantages of extended contact are clear, its potential theoretical advantages remain untested to the best of our knowledge.

Extended contact and direct friendship clearly have advantages and disadvantages relative to each other. Intergroup contact researchers therefore agree that intergroup attitudes benefit the most when they work together. Indeed, extended contact and direct friendship tend to co-occur because (a) one could interact with and befriend an in-group member's cross-group friend, (b) extended contact could remove barriers to and encourage friendly interactions with out-group members who one does not know to be friends of in-group members, and (c) reciprocally, direct friendship provides extended contact to the other members of the friends' groups if they learn about this friendship. Extended contact and direct friendship may also influence each other in terms of their respective effects on intergroup attitudes: Existing extended contact may improve the quality of direct friendships and therefore potentiate their effect, but existing direct friendships would obviate the need for extended contact and therefore weaken its effect. Of the ways in which extended contact and direct friendship can influence each other, theory has focused on a model whereby extended contact encourages direct friendship (Mallett & Wilson, 2010, Study 2; Schofield et al., 2010; Vezzali et al., 2015) while ameliorating it and potentiating its effect on intergroup attitudes. Supporting the value of combining their effects, a survey study of relations between men and women found better evaluations among people who were high in both extended contact and direct friendship compared with those who were high in one contact type but low in the other (Paolini et al., 2007, Study 2). Given this combined effect, this meta-analysis considers extended contact's effect with and without the contribution of direct friendship.

Moderators of the Extended Contact Effect

Research has supported several theoretically predicted moderators of the extended contact effect. The model of extended contact (Vezzali et al., 2014) categorizes these moderators as (a) contextual conditions, the social and intergroup contexts surrounding the extended contact experience; (b) situational perceptions, perceptions about the extended contact experience itself; or (c) individual differences. Note that the moderators in this model are based on the within-study tests of moderation in the extended contact literature, many of which were insufficient in terms of quantity or variation across studies to test meta-analytically. We could therefore test the effects of only two moderators in this model, the contextual condition of direct friendship and the individual difference of closeness in the extended contact relationships. We also tested moderation by intergroup attitude type, which is not in the model of extended contact but is theoretically tied to the situational perception of out-group attitude structure in that they both speak to whether extended contact is cognitive or affective in nature (see below). Due to space limitations, this section focuses exclusively on moderations by direct contact or friendship and by out-group attitude structure (the "Defining Extended Contact" section discusses moderation by closeness of the extended contact relationships; see Vezzali et al., 2014, for a comprehensive review of moderation in the extended contact effect).

As previously mentioned, extended contact may encourage people to interact with and befriend out-group members themselves. High levels of existing direct contact or friendship would obviate this preparatory role of extended contact, however, meaning that it should work better among people who have little compared with more personal experience with members of the other group. This moderation has received a good deal of empirical support. One survey study (Christ et al., 2010, Study 1) found that Germans' extended contact with foreigners predicted prejudice toward this out-group better in East Germany, where there are fewer foreigners, compared with West Germany, where there are more foreigners, and that contact with foreigners fully mediated this differential effect. Study 2 then replicated these findings for the positive behavioral intentions of Catholics and Protestants toward each other in mixed and segregated neighborhoods in Northern Ireland. Several studies using various methods, group domains, and indices of intergroup attitudes have also found that extended contact's relationship to intergroup attitudes holds *only* when direct contact or friendship is low (Cameron et al., 2011; Dhont & Van Hiel, 2011; Eller, Abrams, & Gomez, 2012; Vezzali, Giovannini, et al., 2012; Vezzali, Hewstone, et al., 2017).

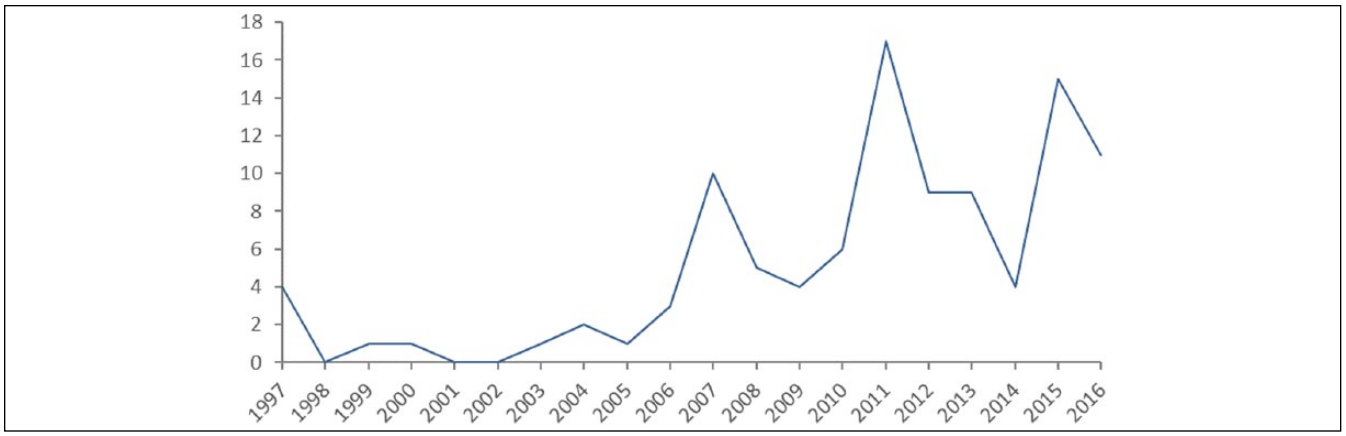

Figure 1. Number of extended contact studies published by year from 1997 to 2016.

Extended contact is purportedly cognitive in nature because knowledge of friendships is cognitive and because its relationship to intergroup attitudes has several cognitive mediators (i.e., in-group norms, out-group norms, and TIOGS). Three survey studies (Paolini et al., 2007) have examined extended contact's cognitive nature by testing moderation by out-group attitude structure, whether the out-group in question elicits more affect (e.g., liking or fear) or cognition (e.g., biases or stereotypes). In Study 1, extended contact predicted prejudice more strongly toward an out-group that pilot testing found to primarily elicit beliefs and thoughts (engineering students) versus ones it found to elicit more emotions (e.g., elderly people, mature-age students, and vegetarians). In Study 2, extended contact with the opposite sex predicted evaluations of this out-group more strongly among participants who reported primarily thoughts and beliefs rather than emotions about the other sex compared with participants who reported vice versa. Using the same methods, Study 3 then replicated these results for the trust and negative behavioral intentions of Northern Irish Catholics and Protestants toward each other. These results all suggest that extended contact works better when people hold more cognitions than affect for the out-group in question. In addition to this moderation by out-group attitude structure, extended contact's cognitive nature would receive support from findings that it works better on cognitive versus affective intergroup attitudes (Turner, Hewstone, Voci, Paolini, & Christ, 2007).

The Current Meta-Analysis

The extended contact hypothesis is therefore an important and empirically robust addition to the intergroup relations literature. Research on this idea has burgeoned despite a slow start, with dozens of such studies being published in the past few years: Figure 1 illustrates these publications by year. These waxes in the extended contact literature make it ripe for meta-analysis, which is especially fitting now, 20 years since its inception in 1997. The present meta-analysis marks this important anniversary by statistically summarizing the relevant research to date.

The place of this meta-analysis in the intergroup contact literature is noteworthy. As previously mentioned, extended contact complements direct contact and friendship as well as other forms of indirect contact so looking at the current meta-analysis within this context is important. Meta-analyses currently exist on direct contact (Pettigrew & Tropp, 2006), direct friendship (Davies et al., 2011), and imagined contact (Miles & Crisp, 2014), but they do not exist on extended or media contact. The present meta-analysis on extended contact, along with the one currently under way on media contact (Zhou, Sharples, & Page-Gould, 2017), therefore completes the series of meta-analyses on the various types of intergroup contact. Complementing this line of meta-analyses, which focuses on theory, two other meta-analyses looked at real-world applications of contact theory by studying interventions based on direct and indirect forms of contact (Beelmann & Heinemann, 2014; Lemmer & Wagner, 2015).[1]

The current meta-analysis on the extended contact hypothesis primarily aimed to determine the aggregate relationship between extended contact and intergroup attitudes across all pertinent studies. As many such studies also reported intercorrelations with direct friendship, we also examined the secondary effect sizes of extended contact's relationship to intergroup attitudes without the contribution of direct friendship plus that of direct friendship with and without the contribution of extended contact. According to the extended contact hypothesis, we predicted that extended contact will relate positively to intergroup attitudes overall. According to the partial mediation of direct friendship in the extended contact effect, we predicted that extended contact's overall relationship to intergroup attitudes will weaken but still remain after removing the contribution of direct friendship. According to the idea that extended contact is less effective than direct friendship, we also predicted that the two relationships between extended contact and intergroup attitudes

will be smaller than their respective direct friendship counterparts. Although we did not have an explicit hypothesis about the overall size of direct friendship's partial effect, either on its own or relative to its full counterpart, these results will nonetheless be important because they consider the role of extended contact in the direct friendship effect.

This meta-analysis also aimed to test several moderations of the extended contact effect, several of which have important theoretical implications. These theoretical potential moderators included perceived or actual extended contact, which is novel to the extended contact literature and for which we predicted larger effects from the former. They also included intergroup attitude type, for which extended contact's cognitive nature predicts larger effects on cognitive versus affective intergroup attitudes (Turner, Hewstone, Voci, et al., 2007). They finally included extended contact definition and closeness of the extended contact relationships, for which extended contact's de facto definition respectively predicts larger effects from traditional versus alternative definitions and from closer extended relationships; this definition of extended contact is not itself theoretically or empirically grounded, however, so we treated these tests of moderation as exploratory. We also tested the atheoretical moderations of time, location, and group domain, which are completely novel to the extended contact literature, as well as participant demographics and in-group status, which were tested in previous research but which we nonetheless considered exploratory because the evidence for their moderations or lack thereof has been unreliable.

Extended contact complements direct friendship so considering their effects in this context is important. To this end, we compared the aggregate results on extended contact and direct friendship within the current meta-analysis and we compared our findings on extended contact with the corresponding results of the meta-analysis on direct friendship (Davies et al., 2011) wherever possible. Note that approximately 18 studies were in both meta-analyses because they tested extended contact and direct friendship effects; this overlap will undoubtedly create some similarity in the results of these meta-analyses, making the pattern of similarities and differences between them especially important. This discussion will nonetheless highlight the ways in which extended contact and direct friendship are similar to and different from each other.

Method

Inclusion Criteria

Studies in this meta-analysis must have tested the relationship between extended contact with and attitudes toward a particular out-group. From each of these studies, we needed either a correlation between extended contact and intergroup attitudes or a comparison of intergroup attitudes between an extended contact and a no-contact control condition. As very few studies of the extended contact hypothesis looked at delayed posttest effects, this meta-analysis included only concurrent correlations and immediate posttest experimental effects.[2]

Locating Studies

We located suitable studies for this meta-analysis using several strategies. We first searched Google Scholar plus 11 electronic databases[3] using the search terms "extended," "vicarious," and "indirect" combined with "friendship" or "contact." In the same databases, we also filtered for and manually searched the articles that cite the seminal article on the extended contact hypothesis (Wright et al., 1997). We manually searched (a) the reference lists of all the articles that we already had, (b) the reference list of the 2011 Special Issue on extended contact from *Group Processes and Intergroup Relations*, and (c) the archived conference proceedings of the Society for Personality and Social Psychology. For any study that reportedly looked at our variables of interest but for which the necessary effect size was not available, we requested this information from its article's corresponding author. We finally solicited unpublished studies by emailing more than a dozen researchers who have published extensively on extended contact and by posting on the listservs of several social psychological organizations (i.e., Society for Personality and Social Psychology, Society for the Psychological Study of Social Issues, and Society of Experimental Social Psychology). These search strategies yielded 117 studies that met the aforementioned inclusion criteria, two of which we excluded post hoc because their conceptual definitions of extended contact were inconsistent with those of the other studies.[4] The 115 studies that we retained contained 248 effect sizes, came from 75 articles, and contained data from 47,133 participants. This literature search covered research up to the beginning of 2017.

Effect Sizes

The final effect size statistic in this meta-analysis was Pearson's product–moment correlation coefficient[5] (r). From correlational studies, the primary effect size was the bivariate correlation between extended contact and intergroup attitudes. From the correlational studies for which they were available or for which we could derive them, we also extracted three secondary effect sizes: (a) the partial correlation between extended contact and intergroup attitudes, controlling for the contribution of direct friendship ($k = 185$); (b) the bivariate correlation between direct friendship and intergroup attitudes ($k = 189$); and (c) the partial correlation between direct friendship and intergroup attitudes, controlling for the contribution of extended contact ($k = 182$). From experimental studies, we calculated Cohen's d from the means and standard deviations of intergroup attitudes in the extended contact and control conditions before converting it

into Pearson's *r*. A few of the experimental studies in this meta-analysis also included a direct friendship condition, in which case we calculated a secondary effect size for it using the procedure above; this effect size is equivalent to the bivariate correlation between direct friendship and intergroup attitudes. We calculated and converted these effect sizes along with their corresponding sampling variances in R 3.4.3 using the compute.es package (Del Re, 2013). The sign of each Pearson's *r* was finally adjusted so that a positive value indicates a positive relationship between extended contact or direct friendship and intergroup attitudes.

Coding Potential Moderators

We coded several variables to test their moderating influences on extended contact's relationship to intergroup attitudes. The first author and an undergraduate research assistant coded each study, doing so together to ensure consistency before beginning independent coding. These coders resolved disagreements in the independent codes via discussion and amended the coding procedures if interrater reliability was too low.

Publication status. We coded each study or set thereof as being from a *journal* article, a *dissertation or thesis*, a *conference presentation*, or an *unpublished* document.

Time. This was the year of publication for journal articles, of defense for dissertations or theses, of presentation for conference talks or posters, or of data collection for unpublished studies.

Location. We coded each study as having taken place in the *United Kingdom, Europe* (*not United Kingdom*), *North America*, or *other*.[6]

Age. This was the mean age (in years) of participants in each sample or subsample. When this information was unavailable, we used that of another sample from the same population if possible or estimated it from the educational levels of child and adolescent samples. We estimated missing age data in this manner because doing so was more precise and would bias the results less than statistically imputing them.

Gender. This was the proportion of females in each sample or subsample, which could range from 0 (*all males*) to 1 (*all females*).

Group domain. We coded each pair of in-group and out-group as being based on *ethnicity, nationality, religion, sexual orientation*, or *other*.[7]

In-group status. For each pair of in-group and out-group, we coded the in-group's status relative to the out-group as *majority, minority*, or *combined or equal*. The latter coding category was for cases in which the in-group and out-group were relatively equal or in which data from majority-group and minority-group respondents were combined.

Experimental design. We coded each study design as *correlational* or *experimental*.

Control condition. From experimental studies, we coded the control condition as *in-group, nonsocial*, or *none*. In-group control conditions featured friendships between two in-group members, instead of between an in-group and out-group member. Nonsocial control conditions featured animals (e.g., nature videos) or inanimate objects, but no people. "None" control conditions did not include any experimental procedures and had participants complete only the intergroup attitude measures.

Extended contact definition. From correlational studies, we coded each operational definition of extended contact as *traditional* or *alternative*. Traditional operational definitions of extended contact matched its original version (Wright et al., 1997, Studies 1 and 2), asking straightforwardly about the number of in-group members who have any cross-group friends (e.g., "How many in-group members do you know who have out-group friends?"). As extended contact was measured according to this traditional method in most (83%) cases in this meta-analysis, we coded anything outside those parameters as *alternative*. Note that alternative operational definitions of extended contact differed from their traditional counterparts in two ways: They asked almost exclusively about the number of cross-group friends that respondents' in-group friends had, emphasizing extended relationship quantity, and used a two-step psychometric approach whereby participants first list their in-group friends and then enumerate the cross-group friends of each in-group friend they listed. In one such operational definition, German and Turkish elementary students listed their three best friends and then enumerated the friends that each of these friends had from the other country; across the respondent's three listed friends, their number of friends from the other country was averaged as an index of extended contact (Feddes et al., 2009).

Perceived or actual extended contact. From correlational studies, we coded each operational definition of extended contact as looking at *perceived* or *actual* extended contact. Operational definitions of perceived extended contact asked respondents about the cross-group friendships of other people in their group, whereas those of actual extended contact asked these questions of the respective in-group members themselves. Note that the two potential moderators relevant to measuring extended contact overlapped because traditional operational definitions thereof always looked at perceived extended contact.

Closeness of the extended contact relationships. For each operational definition of extended contact, we coded the relationship between participants and the in-group member (direct

relationship) plus that between the in-group and out-group members (extended relationship). For correlational studies, we coded both relationships based on the item or items measuring extended contact. In the example item "How many close in-group friends do you have who have out-group friends?", "close in-group friends" indicates that the self and in-group member are close friends while "have out-group friends" indicates that the in-group and out-group members are friends. Using this procedure, we coded the direct and extended relationships from each extended contact item as an *acquaintanceship; friendship*; or *close friendship, familial relationship, or romantic relationship*. For experimental studies, we coded these relationships based on the people featured in the extended contact condition: We classified participants' relationship to the in-group member in this condition (direct relationship) plus the relationship between the in-group and out-group members therein (extended relationship) using the options above. Noting that participants did not know this in-group member in most cases[8], we added *no relationship* as a coding option for the direct relationship in experimental studies. We then quantified these discrete categories (*no relationship* = 0, *acquaintanceship* = 1, *friendship* = 2, and *close friendship, familial relationship, or romantic relationship* = 3) to form two continuous variables that could each range from 1 to 3, averaging across multiple items in a measure.

Intergroup attitude type. We coded each measure of intergroup attitudes as primarily looking at *affective, behavioral*, or *cognitive* intergroup attitudes. Affective intergroup attitudes were feelings toward the out-group, such as liking, warmth, sympathy, and happiness during contact. Behavioral intergroup attitudes were actions toward the out-group or members thereof, such as helping or harming, and included participants' actual behavior during the study as well as their expressed attitudes toward and intentions to perform target behaviors. Cognitive intergroup attitudes were beliefs, evaluations, opinions, or thoughts about the out-group, such as stereotypes and support for anti- or proimmigration policies.

Direct friendship. This was the mean rating on each direct friendship measure within each sample or subsample standardized against its potential range. We specifically transformed each raw direct friendship mean such that it became 0, 0.5, and 1.0 if it were at the scale's lowest end, midpoint, and highest end, respectively. Note that some of these measures used an open-ended response scale, which allowed participants to enter any response without restriction; without a potential range, we could not standardize direct friendship means based on open-ended response scales and therefore coded them as *missing*.

Analyses and Results

Table 1 reports the 248 full extended contact effect sizes in this meta-analysis, along with the groups and index of intergroup attitudes associated with each of them. The full extended contact effect size was the primary one of interest and is therefore the referent of "effect size" below unless it states otherwise. We conducted aggregate analyses on all four effect sizes but publication bias, heterogeneity, and moderation analyses only on the full extended contact effect size. We conducted all analyses in R 3.4.3, specifically using the rma.mv function of the metafor package (Viechtbauer, 2010) to run a multilevel meta-analytic model that nests effect sizes within studies and studies within articles.[9] The data, analysis script, and supplemental materials for this project are on Open Science Framework at https://osf.io/4fygb.

We checked the full extended contact effect sizes for univariate outliers before beginning data analysis. This procedure did not reveal the presence of any such outliers. Specifically, the range of these effect sizes (–.17 to .61) were within the boundaries of nonextreme scores (–.18 to .62), which was 1.5 times the interquartile range below the first quartile (Q_1 = .12) and above the third quartile (Q_3 = .32).

Publication Bias

We conducted Egger's test manually by examining moderation of the effect sizes by their corresponding inverse sampling errors. This result was not significant, r = .001, 95% confidence interval (CI) = [–.001, .003], z = 0.83, p = .41, indicating that the funnel plot (Figure 2) was not asymmetrical. This result suggests that publication bias did not strongly influence the effect sizes in this meta-analysis.

We also conducted a cumulative meta-analysis in which we tracked the cumulative aggregate effect size with each additional study in order of decreasing sample size (Hunter & Schmidt, 1990). We conducted this analysis manually, adding the effects sizes of each successive study into the aforementioned multilevel meta-analytic model, because the function that conducts cumulative meta-analyses in metafor does not currently accept rma.mv models. A forest plot depicting these results is in the supplemental materials (Figure S1): The cumulative aggregate effect size did increase with the addition of studies using smaller samples at the beginning, but it reached and held steady at its eventual final value around one third of the way through this process. These results, again, suggest that publication bias did not strongly influence the effect sizes in this meta-analysis.

We finally tested moderation of the effect sizes by publication status, the analytical procedures of which are described below. Overall moderation by publication status was not significant, $Q(3)$ = 2.07, p = .56, indicating that effect sizes did not differ across journal articles (r = .26, 95% CI = [.23, .29]), dissertations or theses (r = .19, 95% CI = [.12, .26]), conference presentations (r = .18, 95% CI = [.04, .32]), and unpublished studies (r = .26, 95% CI = [.17, .35]). A contrast specifically comparing effect sizes from studies published in journals with those from the other three publication

Table 1. Extended Contact Effect Sizes.

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
*Aboud, Friedmann, and Smith (2015), Study 2	Anglophone/Francophone	Friendship expectations	64	.04	
	Francophone/Anglophone		84	.08	
*Abrams, Eller, and Bryant (2006)	elderly/young adult	Evaluations	97	.21	
Adesokan (2014), Study 3	Yoruba/Hausa	Warmth	363	.30	
	Yoruba/Igbo			.23	
Adesokan (2014), Study 4	Yoruba/Hausa	Evaluations	227	.33	
	Yoruba/Igbo			.33	
*Andrighetto, Mari, Volpato, and Behluli (2012)	Kosovar Albanian/Serbian	Stereotypes	151	.30	
*Becker, Wright, Lubensky, and Zhou (2013), Study 1	LGBT/heterosexual	Warmth	340	.14	
		Positive affect		.15	
		Social distance, behavioral intentions		.25	
Boykin, Aron, Wright, Zhou, and Spector (2014)	Christian/Jewish	Warmth	225	.30	
	Christian/Muslim			.12	
	Jewish and Muslim/Christian		43	.04	
	Jewish and Muslim (combined)			.20	
Cameron, Rutland, and Brown (2007), Study 1	Non-disabled/learning and physically disabled	Stereotypes	49	.40	
		Behavioral intentions		.13	
Cameron et al. (2007), Study 2	British/refugee	Stereotypes	98	.36	
		Behavioral intentions		.20	
*Capozza, Falvo, Favara, and Trifiletti (2013)	Northern Italian/Southern Italian	Out-group trust	251	.32	
*Capozza, Falvo, Trifiletti, and Pagani (2013)	Heterosexual/homosexual	Out-group trust	202	.11	
Cernat (2011)	Romanian/Roma	Beliefs	71	.26	
		Stereotypes		.20	
		Positive affect (admiration)		.29	
		Negative affect (disgust)		.35	
Christ et al. (2010), Study 1	German/foreigner	Political beliefs	1,024	.29	
Christ et al. (2010), Study 2	Northern Irish Catholic and Protestant (combined)	Behavioral intentions	404	.36	Time 1
				.32	Time 2
Comeau (2004)	European and Chinese Canadian/South Asian South Asian/European Canadian (combined)	Warmth	353	.34	
		Neo-racism		.15	
		Behavioral intentions		.30	
Cummings (2013)	European American/African American	Actual behavior (physical distance)	194	.07	
		Social distance		.02	
		Prejudice		−.08	
		Warmth		.10	
		Affective priming		.13	
Davies (2006)	East Asian, European, and Latin American/African American	Evaluations	60	.12	
		Affective prejudice		.16	
Davies, Aron, Wright, Eberhardt, and Bergsieker (2006), Study 1	same-ethnic civilian/police officer	Positive affect (respect)	57	.28	
Davies et al. (2006), Study 2	African American/East Asian	Positive affect	75	.39	

(continued)

Table 1. (continued)

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
	African American/Latin American			.16	
	African American/European American			.20	
	East Asian/African American		331	.30	
	East Asian/Latin American			.24	
	East Asian/European American			.22	
	Latin American/African American		87	.29	
	Latin American/East Asian			.01	
	Latin American/European American			.02	
	European American/African American		409	.15	
	European American/East Asian			.15	
	European American/Latin American			.17	
De Tezanos-Pinto (2010), Study 2	Chilean/Peruvian immigrant	Prejudice	311	.14	
		Positive affect		.25	
De Tezanos-Pinto (2010), Study 3	British/immigrant	Political beliefs	122	.19	
		Positive affect		.33	
De Tezanos-Pinto (2010), Study 4	British/immigrant	Political beliefs	142	.25	
		Positive affect		.41	
De Tezanos-Pinto (2010), Study 5	Norwegian/Indian, Pakistani, and Turkish	Warmth	827	.24	
De Tezanos-Pinto (2010), Study 6	Chilean/Peruvian	Prejudice	698	.20	Time 1
		Positive affect		.27	
		Prejudice		.23	Time 2
		Positive affect		.32	
de Tezanos-Pinto, Bratt, and Brown (2010)	Norwegian/Indian, Pakistani, and Turkish	Warmth	665	.30	
Dhont, Roets, and Van Hiel (2011), Study 2	Belgian/immigrant	Beliefs	294	.41	
Dhont and Van Hiel (2011)	Dutch/immigrant	Prejudice	1,238	.37	
Drury, Hutchison, and Abrams (2016), Study 1	Young adult/elderly	Evaluations	70	.34	
Drury et al. (2016), Study 2	Young adult/elderly	Evaluations	110	.35	
Drury et al. (2016), Study 3	Young adult/elderly	Evaluations	95	.22	
*Eller, Abrams, and Gomez (2012), Study 1	American/Mexican	Affective prejudice	69	−.09	
*Eller et al. (2012), Study 2	American/Mexican	Self-reported behavior (out-group media consumption)	97	.37	
*Eller et al. (2012), Study 3	British/French	Self-reported behavior (out-group media consumption)	72	.28	Time 1
				.40	Time 2
Eller, Abrams, Viki, and Imara (2007)	European British civilian/ European British police officer	Beliefs	67	−.10	
		Social distance		−.17	
	African British civilian/African British police officer	Beliefs	63	.17	
		Social distance		.09	
Eller, Abrams, and Zimmermann (2011)	30 non-British national groups/ British	Evaluations	134	−.01	
Feddes, Noack, and Rutland (2009)	German/Turkish	Stereotypes	76	.36	Time 1
				.50	Time 2

(continued)

Table 1. (continued)

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
	Turkish/German		73	.31	Time 1
				.37	Time 2
Gómez and Huici (2008)	Student fans of a high school basketball team/a rivaling basketball team	Stereotypes	68	.28	
Gómez, Tropp, and Fernández (2011)	Spanish/immigrant	Warmth	187	.39	
		social expectations		.59	
	Immigrant/Spanish	Warmth	135	.47	
		Social expectations		.51	
Hayward, Barlow, and Tropp (2013)	European American/African American	Symbolic racism	374	.11	
		Evaluations		.22	
	African American/European American		194	.32	
	Latin American/European American		169	.44	
*Hewstone, Judd, and Sharp (2011), Study 1	European British/South Asian	Evaluations/warmth	123	.13	Traditional E.C.M.
				.08	Social network E.C.M.
*Hewstone et al. (2011), Study 2	European British/South Asian	Warmth	136	.32	Traditional E.C.M.
				.00	Social network E.C.M.
	Heterosexual/homosexual men			.13	
Hodson, Harry, and Mitchell (2009)	Heterosexual/homosexual	Beliefs	115	.39	
Husnu, Mertan, and Çiçek (2016), Study 1	Turkish Cypriot/Greek Cypriot	Stereotypes	86	.47	
		Behavioral intentions		.30	
Husnu et al. (2016), Study 2	Turkish Cypriot/Greek Cypriot	Stereotypes	75	.02	
		Behavioral intentions		.02	
		Out-group trust		.05	
Hutchison and Rosenthal (2011), Study 2	Non-Muslim/Muslim	Evaluations	60	.37	
		Behavioral intentions		.41	
Ioannou (2009)	Greek Cypriot/Turkish Cypriot	Warmth	26	.16	
Joyce and Harwood (2014)	U.S. citizen/illegal immigrant	Stereotypes	75	.42	
*Kamata (2008), British Study	British/American	Evaluations/warmth	209	.25	
	British/Chinese			.16	
*Kamata (2008), Japanese Study	Japanese/American	Evaluations/warmth	214	.28	
	Japanese/Chinese			.30	
[1]Koball and Carels (2015)	Normal weight/overweight	Stereotypes	156	.19	Measure of E.C.
		Prejudice		.26	
		Positive stereotypes	77	−.09	Manipulation of E.C.
		Negative stereotypes		.05	
		Prejudice		−.01	
		Beliefs		.05	
		Behavioral intentions		−.17	
Liebkind and McAlister (1999)	Finnish/foreigner	Prejudice	1,480	−.01	
Liebkind, Mähönen, Solares, Solheim, and Jasinskaja-Lahti (2014)	Finnish and immigrant (combined)	Warmth	770	.00	
Lienemann and Stopp (2013)	European American/African American	Evaluations/emotions	218	.15	Media E.C.M.
				.15	Traditional E.C.M.

(continued)

Table 1. (continued)

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
Lyons (2011)	American/Arab, European, and Latin immigrant	Warmth	90	.12	Measure of E.C.
	American/Arab immigrant	Warmth, funding deservingness, actual behavior (resource allocation)		.13	Manipulation of E.C.
Lytle (2016), Study 1	Young adult/elderly	Beliefs	299	.11	
		Positive stereotypes		.03	
		Negative stereotypes		.03	
		Warmth		−.05	
		Behavioral intentions		−.02	
Lytle (2016), Study 2	Young adult/elderly	Beliefs	172	.09	
		Positive stereotypes		.19	
		Negative stereotypes		.15	
		Warmth		.08	
		Behavioral intentions		.11	
		Actual behavior (petition signing)		.10	
Mazziotta, Mummendey, and Wright (2011), Study 1	German/Chinese	Warmth	48	.34	
		Behavioral intentions		.39	
Mazziotta et al. (2011), Study 2	German/Chinese	Evaluations	36	.35	
		Behavioral intentions		.36	
Mazziotta, Rohmann, and Wright (2014)	Heterosexual/homosexual men	Warmth	279	.36	
		Competence stereotypes		.13	
		Warmth stereotypes		.09	
	Heterosexual/homosexual women	Warmth		.44	
		Competence stereotypes		.14	
		Warmth stereotypes		.26	
Mazziotta, Rohmann, Wright, and De Tezanos-Pinto (2014), Study 1	German/Polish	Warmth	146	.15	
		Competence stereotypes		.05	
		Warmth stereotypes		.08	
Mazziotta, Rohmann, Wright, and De Tezanos-Pinto (2014), Study 2	German/Turkish	Political beliefs	150	.22	
		Warmth		.31	
Mazziotta, Rohmann, Wright, De Tezanos-Pinto, and Lutterbach (2015), Study 1	German/Turkish	Warmth	286	.25	
Mazziotta et al. (2015), Study 2	Non-Muslim/Muslim	Evaluations	237	.28	
Meeusen (2014)	Belgian/immigrant	Political beliefs	563	.11	Children's E.C. via mothers
			563	.18	Mothers' E.C. via spouses
			563	.06	Fathers' E.C. via children
Mereish and Poteat (2015)	Heterosexual/homosexual and bisexual	Beliefs	439	.32	
		Self-reported past behavior		.05	
Openshaw (2015)	European South African/African South African	Warmth	37	.05	
		Out-group trust		.26	

(continued)

Table 1. (continued)

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
Paolini, Hewstone, and Cairns (2007), Study 2	Men and women (combined)	Evaluations/warmth	141	.24	
Paolini et al. (2007), Study 3	Northern Irish Catholic and Protestant (combined)	Warmth	798	.23	
		Out-group trust		.43	
		Behavioral intentions		.23	
*Paolini, Hewstone, Cairns, and Voci (2004), Study 1	Northern Irish Catholic and Protestant (combined)	Evaluations/warmth	341	.48	
*Paolini et al. (2004), Study 2	Northern Irish Catholic and Protestant (combined)	Social distance, beliefs	735	.34	
*Paterson (2012), Study 1	European British/African British	Intergroup anxiety	110	.27	Friendship E.C.M.
				.11	Romantic E.C.M.
	European British/South Asian			.26	Friendship E.C.M.
				.24	Romantic E.C.M.
Paterson, Turner, and Conner (2015)	European British/South Asian	Positive affect	99	.28	
Pettigrew, Christ, Wagner, and Stellmacher (2007)	German/foreigner	Political beliefs	1,383	.34	
*Schmid, Ramiah, and Hewstone (2014)	European British/ethnic minority	Warmth	793	.23	
	Ethnic minority/European British		726	.14	
*Schmid, Hewstone, Küpper, Zick, and Wagner (2012), British Study	British/immigrant	Political beliefs	768	.32	
*Schmid et al. (2012), Dutch Study	Dutch/immigrant	Political beliefs	866	.23	
*Schmid et al. (2012), French Study	French/immigrant	Political beliefs	656	.36	
*Schmid et al. (2012), German Study	German/immigrant	Political beliefs	829	.27	
*Schmid et al. (2012), Hungarian Study	Hungarian/immigrant	Political beliefs	611	.18	
*Schmid et al. (2012), Italian Study	Italian/immigrant	Political beliefs	860	.24	
*Schmid et al. (2012), Polish Study	Polish/immigrant	Political beliefs	611	.22	
*Schmid et al. (2012), Portuguese Study	Portuguese/immigrant	Political beliefs	842	.08	
Sharp, Voci, and Hewstone (2011)	European British/South Asian	Warmth	149	.22	
	European British/homosexual men			.28	
Slone, Tarrasch, and Hallis (2000)	Jewish Israeli/Arab	stereotypes	208	.32	
Tam, Hewstone, Kenworthy, and Cairns (2009), Study 2	Northern Irish Catholic and Protestant (combined)	Out-group trust	175	.30	
		Stereotypes		.37	
		Self-reported negative behavior		.32	
		Self-reported positive behavior		.40	
Tausch, Hewstone, Schmid, Hughes, and Cairns (2011)	Northern Irish Catholic and Protestant (combined)	Out-group trust	424	.38	E.C. via neighbors and colleagues
				.49	E.C. via friends
				.47	E.C. via family members
Turner, Hewstone, and Voci (2007), Study 2	European British and South Asian (combined)	Positive affect	96	.60	
		Implicit evaluations		.09	
Turner et al. (2007), Study 3	European British/South Asian	Stereotypes	164	.26	
		Implicit evaluations		.05	
Turner, Hewstone, Voci, and Vonofakou (2008), Study 1	European British/South Asian	Warmth	142	.30	

(continued)

Table 1. (continued)

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
Turner et al. (2008), Study 2	European British/South Asian	Stereotypes	120	.52	
Turner, Tam, Hewstone, Kenworthy, and Cairns (2013)	Northern Irish Catholic and Protestant (combined)	Stereotypes	128	.52	
Vezzali, Brambilla, Giovannini, and Paolo Colucci (2017)	Heterosexual/homosexual	Behavioral intentions	639	.34	
Vezzali and Giovannini (2010)	Italian/immigrant	Acculturation beliefs	93	.17	
*Vezzali, Giovannini, and Capozza (2012)	Italian/immigrant	Implicit evaluations	30	−.13	
Vezzali, Hewstone, Capozza, Trifiletti, and Di Bernardo (2017)	Italian and immigrant (combined)	Social distance	308	.21	
		Stereotypes		.14	
		Behavioral intentions		.14	
Vezzali, Saguy, Andrighetto, and Giovannini (2013), Study 1	Italian/immigrant	Behavioral intentions	195	.31	
		Warmth		.61	
Vezzali et al. (2013), Study 2	Italian/immigrant	Behavioral intentions	392	.32	
		Stereotypes		.34	
	Immigrant/Italian	Behavioral intentions	165	.19	
		Stereotypes		.23	
Vezzali, Stathi, Giovannini, Capozza, and Visintin (2015)	Italian/immigrant	Warmth	108	.23	
		Behavioral intentions		.20	
Visintin, Brylka, Green, Mähönen, and Jasinskaja-Lahti (2016), Study 1	Bulgarian Turkish/Roma	Warmth	320	.27	
		Social distance		.43	
	Roma/Bulgarian Turkish	Warmth	320	.46	
		Social distance		.44	
Visintin et al. (2016), Study 2	Estonian/Russian	Warmth	212	.29	
		Social distance		.32	
	Russian/Estonian	Warmth	246	.30	
		Social distance		.30	
Visintin, Voci, Pagotto, and Hewstone (2017), Study 1	Italian/immigrant	Evaluations	199	.31	
Visintin et al. (2017), Study 2	Italian/immigrant	Evaluations	330	.50	
Vittrup and Holden (2011)	European American/African American	Stereotypes	43	.25	
Walker and Scior (2013)	Non-disabled/intellectually disabled	Social distance	925	.08	
West and Turner (2014)	Mentally healthy/schizophrenia	Evaluations	45	.41	
		Behavioral intentions		.33	
		Actual behavior (non-verbal)		.39	
*Wölfer, Schmid, Hewstone, and van Zalk (2016), Study 1, German Study	German/immigrant	Warmth	2,005	.00	
*Wölfer et al. (2016), Study 1, Dutch Study	Dutch/immigrant	Warmth	2,536	.04	
*Wölfer et al. (2016), Study 1, Swedish Study	Swedish/immigrant	Warmth	1,916	.07	
Wölfer et al. (2016), Study 2, Cohort 1	Swedish/immigrant	Political beliefs	1,221	.19	Time 1
				.12	Time 2
				.12	Time 3
				.20	Time 4
Wölfer et al. (2016), Study 2, Cohort 2	Swedish/immigrant	Political beliefs	1,207	.20	Time 1
				.02	Time 2
				.12	Time 3
				.18	Time 4

(continued)

Table 1. (continued)

Study	In-group/out-group	Intergroup attitude	*n*	*r*	Other
Wölfer et al. (2016), Study 2, Cohort 3	Swedish/immigrant	Political beliefs	598	.11	Time 1
				−.02	Time 2
				.08	Time 3
Wölfer et al. (2016), Study 2, Cohort 4	Swedish/immigrant	Political beliefs	601	.09	Time 1
				−.02	Time 2
				.02	Time 3
Wright, Aron, McLaughlin-Volpe, and Ropp (1997), Study 1	European American/ African, East Asian, and Latin American	Affective prejudice, evaluations	84	.30	
Wright et al. (1997), Study 2	European American/ African, East Asian, and Latin American	Affective prejudice, evaluations	132	.22	
	African, East Asian, and Latin American/African, East Asian, Latin, and European American		107	.34	
Wright, Davies, and Saunders (2007)	Heterosexual/homosexual	Warmth	121	.29	
		Evaluations		.34	
		Beliefs		.24	

Note. Asterisks mark studies for which the corresponding effect sizes came from the researcher and were not available in the published article or supplemental material. [1]Effect sizes from the measured extended contact variable were from the dissertation version of this article; the sample that yielded the experimental effect sizes was a subset of the one that yielded the correlational effect sizes. LGBT = lesbian, gay, bisexual, and transgender; E.C.M. = extended contact measure.

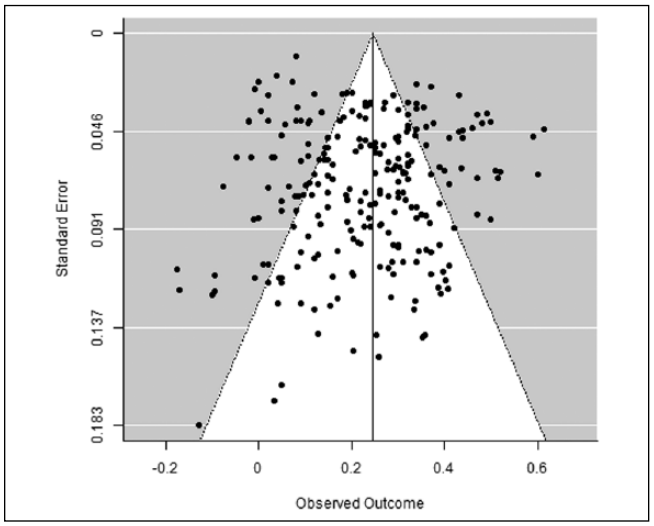

Figure 2. Funnel plot of Fisher's *z* transformed effect sizes by standard error.

categories was also not significant, $r = .05$, $z = 1.05$, $p = .29$, indicating that effect sizes did not differ between studies that were formally published and those that were informally published or unpublished. This result suggests that the publication process did not specifically favor studies that found larger effects, again suggesting that publication bias did not strongly influence the effect sizes in this meta-analysis or the extended contact literature.

Aggregate Analyses

In conducting the aggregate analysis, we handled the multiple effect sizes in this meta-analysis in two ways. We first conducted the aforementioned multilevel meta-analytic model, which nested effect sizes within studies and studies within articles, on each effect size separately. We conducted these univariate analyses instead of a single multivariate one for two reasons. The primary reason is that the multivariate analysis would have excluded the 26 studies that did not also look at direct friendship, among which are most (all but three) of the experimental studies, because this analysis retains only the studies that contribute every relevant effect size. The second reason is that the two partial effect sizes controlled for each other, and were therefore orthogonal and did not require a multivariate analysis. Recognizing that comparability between correlated effect sizes requires a multivariate meta-analysis, we did conduct it on the full effect sizes of extended contact and direct friendship for this purpose. To conduct this analysis, we aggregated effect sizes within studies to run a traditional random effects model. The formula to calculate covariances between effect sizes was from Steiger (1980). The univariate and multivariate models both used the restricted maximum likelihood (REML) estimator and unstructured variance–covariance matrix. Again, these analyses used the rma.mv function of the metafor package.

Figure 3 presents the univariate aggregate results. The aggregate relationship between extended contact and

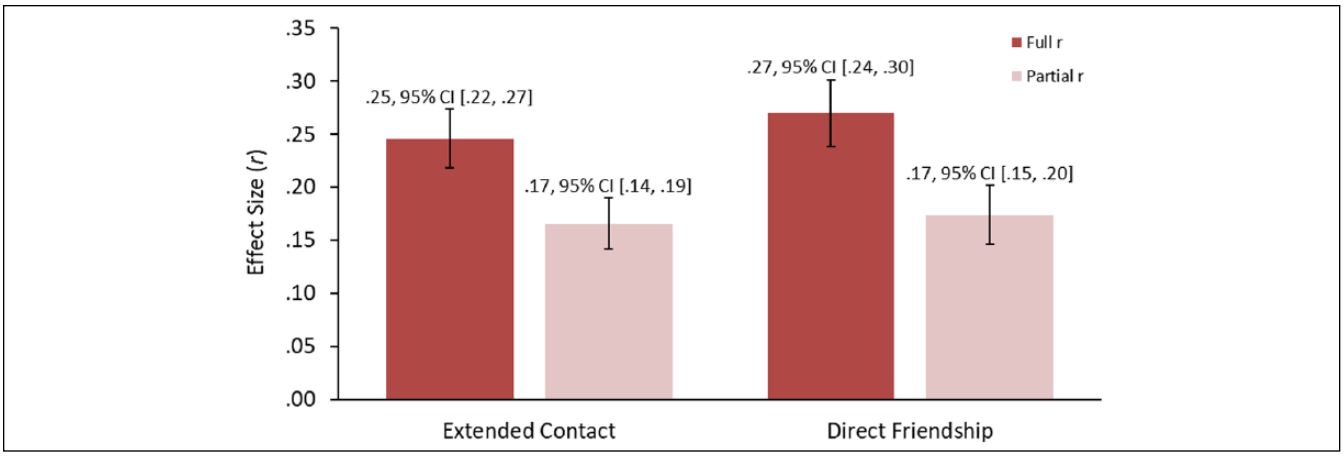

Figure 3. Aggregate full and partial extended contact and direct friendship effect sizes.
Note. Error bars represent 95% confidence intervals.

intergroup attitudes was r = .25, 95% CI = [.22, .27], z = 17.36, p < .001, which reduced to r = .17, 95% CI = [.14, .19], z = 13.44, p < .001 after removing the contribution of direct friendship. The aggregate relationship between direct friendship and intergroup attitudes was r = .27, 95% CI = [.24, .30], z = 16.89, p < .001, which reduced to r = .17, 95% CI = [.15, .20], z = 12.13, p < .001 after removing the contribution of extended contact. As evidenced by their nonoverlapping and overlapping CIs, respectively, the two extended contact effect sizes did not differ from their direct friendship counterparts and both the full effect sizes were larger than their partial counterparts.

The aggregate extended contact effect sizes support the extended contact hypothesis, showing a positive relationship between extended contact and intergroup attitudes. This relationship was small-to-medium when considering extended contact alone, and small but reliably above zero after removing direct friendship's contribution. These results suggest that extended contact relates to positive intergroup attitudes, and that it does so partly through as well as independently of direct friendship.

Remember that we also conducted a random effects multivariate meta-analysis to compare the aggregate full effect sizes of extended contact and direct friendship, which was a secondary goal of this meta-analysis. Results of this analysis indicated that the effect sizes of extended contact, r = .26, 95% CI = [.23, .28], z = 18.80, p < .001, and direct friendship, r = .27, 95% CI = [.24, .29], z = 18.48, p < .001, did not differ from each other, r = .01, z = 1.16, p = .25, suggesting that these two forms of intergroup contact relate to intergroup attitudes to a similar degree.[10] The aggregate size of our full extended contact effects was also similar to its comparable direct friendship counterparts in other meta-analyses: The direct friendship meta-analysis (Davies et al., 2011) yielded r = .26, 95% CI = [.24, .28] and friendship indices of intergroup contact related to prejudice at r = –.25 in the direct contact meta-analysis (Pettigrew & Tropp, 2006). Comparing our own aggregate partial effect sizes replicated this result. Accordingly, the contact intervention meta-analysis (Lemmer & Wagner, 2015) did not find a difference between the effect sizes of direct and indirect contact. Contradicting theory and some within-study findings favoring the efficacy of direct friendship over that of extended contact (Paolini et al., 2007, Study 1; Paolini et al., 2004, Studies 1 and 2), these results all suggest that these types of cross-group contact relate to intergroup attitudes at parity.

Noting that the studies finding larger effects from direct friendship over extended contact used mostly religious groups, we explored the possibility that group domain can partially explain why these studies found this differential effect while our meta-analysis did not. To test this idea, we conducted the multivariate meta-analytic model with group domain as the moderator and without an intercept to attain the effect sizes of extended contact and direct friendship by group domain. We then compared the two effect sizes within each group domain using the anova function. This comparison was indeed significant for religion, r = .06, z = 2.26, p = .02, but it was not significant for ethnicity, r = .003, z = 0.13, p = .89, nationality, r = .01, z = 0.43, p = .67, sexual orientation, r = .02, z = 0.65, p = .52, or other group domains, r = –.04, z = −0.77, p = .44. These results suggest that direct friendship is more effective than extended contact for religious out-groups and that their efficacy is at parity for the other group domains. So the studies finding larger effects from direct friendship over extended contact did so because they used a group domain for which it is true, but this meta-analysis did not find these effects to differ because it spanned several group domains for which these contact types are similarly effective.

The univariate aggregate results on direct friendship are also noteworthy. First, their full-to-partial decrease was meaningful and similar to its extended contact counterpart. This pattern of effect sizes indicates that extended contact and direct friendship shared variance in their relationships to

Table 2. Descriptive Statistics of and Correlations Among the Continuous Potential Moderators.

Potential moderator	*M*	*SD*	1	2	3	4	5	6
Time	2011	4.11	—					
Age	25.77	13.53	.01	—				
Gender	.61	.17	.10	.08	—			
Direct relationship	1.71	0.91	.25	.06	−.02	—		
Extended relationship	2.02	0.43	.05	−.12	.08	.02	—	
Direct friendship	.34	.19	.16	−.02	−.01	−.11	−.07	—

intergroup attitudes, which is unsurprising because they also correlated strongly with each other, $r = .46$, 95% CI = [.42, .51], $z = 21.47$, $p < .001$. In other words, it suggests that extended contact contributes to direct friendship's relationship to intergroup attitudes and that it does so just as much as vice versa. Second, the partial direct friendship effect size is the first estimate of the "pure" relationship between direct friendship and intergroup attitudes that is independent of extended contact. Being such, it is also an important addition to the direct friendship literature and complements the meta-analysis thereon (Davies et al., 2011). The aggregate results of direct friendship therefore highlight the importance of extended contact in direct friendship effects, which the direct friendship literature has yet to consider.

Heterogeneity

Heterogeneity results are available from the rma.mv output. This result from the aggregate model indicated that effect sizes varied more than what is attributable to chance, $Q(247) = 1994.65$, $p < .001$, making these data especially suitable for moderation analyses. This heterogeneity reduced but remained high after simultaneously controlling for the moderations that we tested, $Q(191) = 888.05$, $p < .001$. Such results suggest that these moderations accounted for a good amount, but not all, of the variability in the effect sizes.

Tests of Moderation

We conducted moderation analyses by simultaneously entering orthogonal contrasts for each categorical potential moderator and the grand-mean centered[11] version of each continuous potential moderator into the aforementioned multilevel meta-analytic model. This moderation model contained most of the potential moderators that this meta-analysis tested but excluded control condition, direct friendship, extended contact definition, and perceived or actual extended contact because they did not apply to all the observed effect sizes. Remember that control condition applied only to experimental studies, and that direct friendship as well as the potential moderators based on measuring extended contact applied only to correlational studies. Applying to nonoverlapping subsets of effect sizes, placing these potential moderators into the same model would not retain any observations for analysis. This moderation model therefore excluded these potential moderators and included only the ones that applied to both experimental designs. To test moderations by control condition and direct friendship, we conducted simple models that contained only the potential moderator under inquiry. To test moderations by the two potential moderators based on measuring extended contact, we placed them into the same model because they are so closely tied together. We tested the omnibus moderation of each categorical potential moderator by specifying its contrasts via the btt argument of the rma.mv function and looked to the corresponding contrast result for specific comparisons.

Table 2 reports the means and standard deviations of as well as correlations among the continuous potential moderators that we tested. Table 3 reports the interrater reliability as well as the sample and effect sizes by coding category of each categorical potential moderator that we tested. We also analyzed each potential moderator on its own and found very similar results, which Table 4 reports.

Time. Moderation by time was not significant, $Q(1) = 0.13$, $p = .71$, indicating that effect sizes did not vary with study dates. This result suggests that the extended contact effect has been linearly stable over time, at least in the past 20 years since research on this idea has been taking place.

Location. Overall moderation by location was not significant, $Q(3) = 3.72$, $p = .29$, indicating that effect sizes did not differ across studies from the United Kingdom ($r = .30$, 95% CI = [.25, .34]), other European countries ($r = .25$, 95% CI = [.21, .29]), North America ($r = .18$, 95% CI = [.13, .23]), and other locations ($r = .26$, 95% CI = [.18, .35]). A contrast specifically comparing effect sizes from studies conducted in the United Kingdom or another European country with those from studies conducted in North America was marginally significant, $z = 1.86$, $p = .06$, however, indicating that effect sizes might have been larger in European versus North American studies. This result suggests that extended contact may work better in Europe than in North America.[12]

This moderation by location also emerged in the direct friendship meta-analysis. Davies et al. (2011) attributed this finding to the fact that European friendships are closer and therefore more influential than North American friendships (Davies et al., 2011). Extending this argument to extended

Table 3. Interrater Reliabilities as well as Sample and Effect Sizes by Coding Category of the Categorical Potential Moderators.

Potential moderator	Cohen's kappa	*k*	*r*, 95% CI
Publication status	.84		
Journal		157	.26, [.23, .29]
Dissertation/thesis		48	.19, [.12, .26]
Conference		18	.18, [.04, .32]
Unpublished		25	.26, [.17, .35]
Location	.97		
United Kingdom		62	.30, [.25, .34]
Europe (not United Kingdom)		98	.25, [.21, .29]
North America		70	.18, [.13, .23]
Other		16	.26, [.18, .35]
Group domain	.77		
Ethnicity		66	.24, [.20, .29]
Nationality		103	.24, [.20, .28]
Religion		22	.35, [.27, .42]
Sexual orientation		19	.24, [.17, .32]
Other		37	.17, [.10, .24]
In-group status	.70		
Majority		176	.23, [.21, .26]
Minority		29	.22, [.16, .28]
Combined or equal		42	.32, [.27, .38]
Experimental design	1.00		
Correlational		199	.27, [.24, .30]
Experimental		49	.17, [.11, .22]
Control condition	.87		
In-group		14	.24, [.09, .40]
Nonsocial		13	.25, [.07, .43]
None		15	.11, [–.01, .24]
Extended contact definition	.81		
Traditional		163	.28, [.25, .31]
Alternative		36	.15, [.09, .21]
Perceived or actual extended contact	.81		
Perceived		174	.28, [.25, .30]
Actual		25	.08, [.02, .14]
Attitude type	.83		
Affective		105	.26, [.22, .29]
Behavioral		38	.23, [.18, .27]
Cognitive		100	.25, [.21, .28]

contact, better cross-group friendships in Europe would translate into better extended relationships in the extended contact that these friends give the other members of their respective groups. Given this link between the qualities of cross-group friendships and extended contact within a geographic location, the current moderation by location and its similarity to its counterpart from the direct friendship meta-analysis are both unsurprising.

Age. Moderation by age was not significant, $Q(1) = 0.02$, $p = .88$, indicating that effect sizes did not vary with participants' mean age. This result suggests that extended contact works to a similar degree among people of different ages. Note that studies in this meta-analysis used participants who covered a wide age range, from 6 to 75 years, which shows that extended contact works from an early age and steadily throughout most of the life span.

This null moderation by age also emerged in most of the relevant research on the extended contact hypothesis and in the direct friendship meta-analysis (Davies et al., 2011). Specifically, two studies found that younger (6-8 years old in Cameron, Rutland, & Brown, 2007, Study 2; 5-8 years old in Cameron et al., 2006) and older (9-11 years old) British elementary students reported similar improvements in stereotypes and behavioral intentions toward refugees from hearing extended contact stories featuring this out-group. Another

Table 4. Results of Moderation Tests Without Controlling for the Other Potential Moderators.

	Omnibus moderation			Contrast		
Potential moderator	*Q*	*df*	*p*	Description	*z*	*p*
Publication status	3.96	3	.27	journal vs. dissertation/thesis, conference, and unpublished	1.73	.08
Time	1.12	1	.29			
Location	10.30	3	.02	United Kingdom and Europe (not United Kingdom) vs. North America	2.96	<.01
Age	1.45	1	.23			
Gender	0.19	1	.66			
Group domain	12.00	4	.02	religion vs. ethnicity, nationality, sexual orientation, and other	2.96	<.01
In-group status	10.03	2	<.01	majority vs. minority	0.42	.67
Experimental design	9.49	1	<.01			
Control condition	2.31	2	.32	in-group vs. nonsocial and "none"	−0.65	.52
Extended contact definition	14.60	1	<.001			
Perceived or actual extended contact	35.29	1	<.001			
Direct relationship	2.65	1	.10			
Extended relationship	0.48	1	.49			
Attitude type	1.50	2	.47	cognitive vs. affective	−0.51	.61
Direct friendship	0.10	1	.75			

study in this series, contrarily, found improvements in behavioral intentions only among the younger participants while using the same procedures and population, but Indian British people as the out-group (Cameron et al., 2011).

Gender. Moderation by gender was not significant, $Q(1) = 0.10$, $p = .75$, indicating that effect sizes did not vary with the proportion of female participants. This result suggests that extended contact works to a similar degree among males and females.

This null moderation by gender also emerged in all of the relevant research on the extended contact hypothesis and in the direct friendship meta-analysis (Davies et al., 2011). Specifically, several studies using elementary or secondary student samples and text manipulations of extended contact have tested moderation by gender with null results (Cameron & Rutland, 2006; Cameron et al., 2007; Cameron et al., 2006; Liebkind & McAlister, 1999; Vezzali, Stathi, & Giovannini, 2012). In one such study, male and female Finnish secondary students reported similar decrements in prejudice toward foreigners from reading about a friendship between a Finnish peer model and a non-Finnish person (Liebkind & McAlister, 1999).

Group domain. Overall moderation by group domain was not significant, $Q(4) = 3.15$, $p = .53$, indicating that effect sizes did not differ across ethnic ($r = .24$, 95% CI = [.20, .29]), national ($r = .24$, 95% CI = [.20, .28]), religious ($r = .35$, 95% CI = [.27, .42]), sexual orientation ($r = .24$, 95% CI = [.17, .32]), and other out-groups ($r = .17$, 95% CI = [.10, .24]). A clear pattern emerged when examining effect sizes by these group domains, however: Religion yielded larger effects than any other group domain, the contrast of which was marginally significant, $z = 1.71$, $p = .09$. This result suggests that extended contact may work best for religious out-groups.

Finding this trend whereby religion yielded the largest effect is interesting because it is also the only condition under which we found direct friendship effects to be larger than those of extended contact. So, for religious out-groups, direct friendship effects were larger than those of extended contact even though these extended contact effects were larger than they were in any other group domain. Note that the religious groups under study in this meta-analysis were primarily Northern Irish Catholics and Protestants (54%) or non-Muslims and Muslims (31%), so whether the relatively large effect of religion holds outside these specific intergroup contexts is unclear.

For insight about whether the larger effect of religion generalizes to other interreligious contexts within direct friendship, we looked to the corresponding moderation result from the meta-analysis thereon (Davies et al., 2011). That meta-analysis also found the largest effects for religion, but this difference was merely a trend because religion, nationality, and sexual orientation yielded larger direct friendship effects than did ethnicity. Note that ethnicity is the most visible of these group domains and religion is the most hostile thereof, the latter of which is especially true of the specific interreligious relations under study in this meta-analysis (i.e., Northern Irish Catholics and Protestants plus non-Muslims and Muslims). These results therefore suggest that extended contact and direct friendship may work best for contentious intergroup relations and nonvisible groups, respectively. The former moderation makes sense because direct friendship would be low in these contexts, which is a condition under which extended contact effects should be especially large.

In-group status. Overall moderation by in-group status was not significant, $Q(2) = 3.41$, $p = .18$, indicating that effect sizes did not differ across majority ($r = .23$, 95% CI = [.21, .26]), minority ($r = .22$, 95% CI = [.16, .28]), and equal or

combined groups (r = .32, 95% CI = [.27, .38]). A contrast specifically comparing effect sizes for majority groups with those for minority groups was also not significant, z = 0.29, p = .77, indicating that effect sizes did not differ between them. This result suggests that extended contact works to a similar degree for majority-group members' attitudes toward minority groups and vice versa.

This null moderation by in-group status also emerged in the (albeit limited) relevant research on the extended contact hypothesis and in the direct friendship meta-analysis (Davies et al., 2011). One study has tested moderation by in-group status in the extended contact effect to date and found it to be null: Whether looking at Spanish majority participants and immigrant out-groups or vice versa, extended contact predicted intergroup warmth to a similar degree (Gómez et al., 2011). The direct friendship meta-analysis did not find differential effect sizes by in-group status either, but both the direct contact (Tropp & Pettigrew, 2005) and intervention (Lemmer & Wagner, 2015) meta-analyses found larger effects for majority versus minority in-groups. Note that the intervention meta-analysis consisted mostly of direct contact studies (80%), so similarity in the results of these meta-analyses is unsurprising. These results collectively suggest that intimacy, whether personal or conferred through an in-group member, is necessary for intergroup contact to benefit minority-group members' attitudes toward majority groups as much as it does vice versa.

Experimental design. Overall moderation by experimental design was marginally significant, $Q(1) = 3.08$, $p = .08$, indicating that effect sizes from correlational studies (r = .27, 95% CI = [.24, .30]) might have been larger than those from experimental studies (r = .17, 95% CI = [.11, .22]). This result suggests that extended contact may work better when it occurs naturally than when it is contrived.

This moderation by experimental design is likely attributable to the differential contribution of direct friendship. Remember that extended contact and direct friendship tend to occur together and share variance in their relationships to intergroup attitudes. Correlational studies would have picked up this contribution of direct friendship but experimental studies would have missed it, which explains why the former found larger effects than the latter.[13] Accordingly, experimental studies yielded an effect size that was nearly identical to the aggregate partial extended contact effect size, r = .17, 95% CI = [.14, .19]. The direct friendship meta-analysis (Davies et al., 2011) also found this pattern of moderation by experimental design (r = .16 vs. r = .26), perhaps because extended contact made the same differential contribution to its effect sizes, but this difference was not significant due to their small sample of experimental studies (k = 4). Indeed, their experimental studies also yielded an effect size that was nearly identical to our aggregate partial direct friendship effect size, r = .17, 95% CI = [.15, .20].

Control condition. Overall moderation by control condition was not significant, $Q(2) = 2.31$, $p = .32$, indicating that effect sizes did not differ across the in-group (r = .24, 95% CI = [.09, .40]), nonsocial (r = .25, 95% CI = [.07, .43]), and "none" (r = .11, 95% CI = [–.01, .24]) control conditions. A contrast specifically comparing effect sizes based on the in-group control condition with those based on the nonsocial and "none" control conditions was also not significant, z = −0.65, p = .52, indicating that effect sizes did not differ between cases in which control participants saw or read about in-group members and when they did not. These results are difficult to interpret due to low power, however: The few experimental studies in the extended contact literature meant that each of these coding categories contained an insufficient number of effect sizes ($k_{\text{in-group}}$ = 14, $k_{\text{nonsocial}}$ = 13, k_{none} = 15) to allow a proper moderation test.

Extended contact definition and perceived or actual extended contact. Moderations by whether the operational definition of extended contact followed the traditional (r = .28, 95% CI = [.25, .31]) or alternative (r = .15, 95% CI = [.09, .21]) approach, $Q(1) = 14.60$, $p < .001$, and whether it measured perceived (r = .28, 95% CI = [.25, .30]) or actual (r = .08, 95% CI = [.02, .14]) extended contact, $Q(1) = 35.29$, $p < .001$, were both significant. When examining their moderations together, however, only that of perceived or actual extended contact remained, $Q(1) = 8.48$, $p < .01$, indicating that the former yielded larger effects than the latter. These results suggest that perceived extended contact works better than actual extended contact, and that it does so beyond whether extended contact was measured according to its original operational definition. Note that the effect size from actual extended contact was significant, $z = 2.58$, $p < .01$, indicating that actual extended contact does work. Perceived extended contact just does so better.[14]

This moderation by perceived or actual extended contact suggests that perception matters more than reality in extended contact. Theoretically, it emphasizes the knowledge component of extended contact's conceptual definition and illustrates William Thomas's theorem that "If men define situations as real, they are real in their consequences" (Thomas & Thomas, 1928, p. 572). Empirically, this result suggests that in-group members' real cross-group friendships are unlikely to do anything if one does not know about them and that perceiving these friendships when they are not real should improve intergroup attitudes nonetheless. This result therefore has some large practical implications: Interventions based on extended contact should target perceptions by publicizing cross-group friendships and positive interactions between members of target groups.

Noting some limitations of extended contact definition is important in interpreting its null moderation result. Specifically, homogeneity in the measurement of extended contact

necessitated classifying everything different from its original version into a single category. This binomial classification offered a limited view on extended contact while confounding conceptual and operational considerations: Traditional operational definitions of extended contact emphasized the direct relationship and used a straightforward psychometric approach, whereas alternative ones tended to emphasize the extended relationship and to use a two-step psychometric approach whereby participants first list their in-group friends and then enumerate the cross-group friends of each in-group friend they listed. Perceived or actual extended contact may have overlapped specifically with this psychometric component of extended contact definition: Naming each in-group member one knows to have cross-group friends may force more respondent accountability and leave less room for perceptual bias than straightforwardly enumerating these in-group members. The current partial moderation by extended contact definition may therefore have been null because it was especially vulnerable to confounding by perceived or actual extended contact. Future research should examine the relative efficacy of extended contact's definitions more systemically, testing a larger variety of such possible definitions while separating its conceptual and operational components.

Closeness of the extended contact relationships. Moderations by closeness of the direct, $Q(1) = 1.13$, $p = .29$, and extended, $Q(1) = 0.04$, $p = .84$, relationships were not significant, indicating that effect sizes did not vary with closeness of the extended contact relationships. These results suggest that extended contact works to a similar degree regardless of how close one is to the in-group member or how close the in-group member is to the cross-group friend.

Extended contact's de facto definition emphasizes closeness in the extended relationship but not in the direct relationship. The current null moderation by direct relationship closeness therefore supports this definition, but it contradicts results that extended contact works only through in-group members who are close to oneself (Tausch et al., 2011). The current null moderation by extended relationship closeness does not support extended contact's de facto definition, however, according to which such closeness should relate positively to effect sizes. Properly interpreting this null moderation requires noting that most extended contact measures asked about friendships between in-group and out-group members: Extended relationship closeness varied very little as a result, which may have prevented the current analysis from finding its hypothesized moderation. Accordingly, the seminal study on vicarious contact supported the importance of this extended contact component (Wright et al., 1997, Study 4): Stereotypes of the other group were better after watching an interaction between an in-group and out-group member that implied friendship rather than unfamiliarity or hostility.

Intergroup attitude type. Overall moderation by intergroup attitude type was not significant, $Q(2) = 1.72$, $p = .42$, indicating that effect sizes did not differ across affective ($r = .26$, 95% CI = [.22, .29]), behavioral ($r = .23$, 95% CI = [.18, .27]), and cognitive intergroup attitudes ($r = .25$, 95% CI = [.21, .28]). A contrast specifically comparing effect sizes based on cognitive intergroup attitudes with those based on affective ones was also not significant, $z = -0.59$, $p = .55$, indicating that effect sizes did not differ between them. These results suggest that extended contact works to a similar degree on several types of intergroup attitudes, as well as specifically on cognitive and affective ones.

Extended contact is purportedly cognitive in nature, which may manifest as stronger effects on cognitive versus affective intergroup attitudes (Turner, Hewstone, Voci, et al., 2007). The current null moderation by intergroup attitude type does not support this hypothesis, but this contradictory result may be attributable to the contribution of direct friendship. Specifically, direct friendship is theoretically more affective than cognitive because of its inherent personal involvement: This claim receives support from meta-analytic findings that its effects are stronger on affective versus cognitive intergroup attitudes (Davies et al., 2011), which is a result that we replicated in the full ($r = .30$, 95% CI = [.26, .34] vs. $r = .24$, 95% CI = [.20, .28]), $z = -3.06$, $p < .01$, and partial ($r = .20$, 95% CI = [.17, .23] vs. $r = .14$, 95% CI = [.11, .18]), $z = -2.88$, $p < .01$, direct friendship effect sizes. Direct friendship may therefore be responsible for much of extended contact's affective component. To examine the hypothesized moderation if extended contact lacked this cognitive contribution, we tested it in the partial extended contact effect sizes. This result was not significant, $z = 0.83$, $p = .41$, suggesting that even "pure" extended contact does not preferentially relate to cognitive ($r = .18$, 95% CI = [.15, .21]) versus affective ($r = .15$, 95% CI = [.12, .18]) intergroup attitudes.

Extended contact is purportedly cognitive in nature because *knowledge* of friendships is cognitive and because its relationship to intergroup attitudes has several cognitive mediators. Indeed, in the aforementioned model of extended contact (Vezzali et al., 2014), the cognitive mechanisms outnumber the affective mechanisms four to one. So extended contact's cognitive nature seems to have a solid theoretical foundation as well as empirical support from findings that it works better when people hold more cognition than affect for the out-group in question (Paolini et al., 2007). The current null moderation may therefore reflect the possibility that extended contact's cognitive nature may not translate into stronger effects on cognitive intergroup attitudes, and that cognitive processes may have affective and cognitive consequences that are equally strong. For example, norms that in-group members feel certain emotions toward the out-group and vice versa would encourage one to adopt these intergroup emotions, and TIOGS grants access to the affective aspects of the out-group identity.

Direct friendship. Moderation by the standardized direct friendship means was not significant, $Q(1) = 0.10$, $p = .75$, indicating that effect sizes did not vary with participants'

direct friendship levels. This result suggests that, at the sample level, extended contact works to a similar degree among people who have varying numbers of cross-group friends on average.

Interpreting this null moderation by direct friendship to mean that extended contact works similarly across individuals who have varying numbers of cross-group friends would contradict reliable research results that this effect is stronger among people with fewer versus more cross-group friends. Noting the different levels of analysis at play is important in resolving these discrepant findings: The studies looked at individual differences in direct friendships whereas this meta-analysis looked at sample mean differences therein. These results may therefore collectively suggest that extended contact works better among individuals with fewer versus more cross-group friends and to a similar degree across groups of people with varying mean numbers thereof. Another noteworthy point pertains to the scales against which we standardized the raw direct friendship means. To maximize the range of responses on an item, its scale maximum should have high anchors if the study population tends to have more friends from the target out-group (e.g., 5 = *10 or more*) but low anchors if it tends to have fewer such friendships (e.g., 5 = *5 or more*). This issue also applies to descriptive anchors because respondents would likely have lower thresholds for reporting that they have "many" friends from out-groups with whom their group has little contact. These standardized means may therefore represent direct friendship levels relative to the respective intergroup contexts rather than objective levels thereof.

Discussion

Summary of Meta-Analytic Findings

This meta-analysis found evidence for the extended contact hypothesis, the idea that intergroup attitudes benefit from knowing that in-group members have cross-group friends. This evidence specifically indicated that extended contact related positively to intergroup attitudes and that it did so reliably, holding independently of direct friendship and across various considerations like time, location, participant demographics, groups, features of extended contact, intergroup attitude type, and direct friendship levels. In fact, extended contact's relationship to intergroup attitudes was just as strong as that of direct friendship, which was true when looking at these effects in their "pure" as well as natural forms.

Moderation analyses found extended contact effects to vary in size by only a few of the potential moderators that we tested. Effect sizes were larger when extended contact was perceived versus actual, which suggests that perception matters more than reality in extended contact. They may also have been larger (a) in European versus North American studies, (b) for religion versus other group domains, and (c) in correlational versus experimental studies. However, effect sizes did not vary by time, participants' age or gender, majority versus minority in-group status, extended contact definition, closeness of the extended contact relationships, cognitive versus affective intergroup attitudes, or direct friendship levels. The null moderation by direct relationship closeness is consistent with extended contact's de facto definition, but those by extended contact definition and extended relationship closeness are not. The null moderation by cognitive versus affective intergroup attitudes potentially contradicts the proposed cognitive nature of extended contact.

Comparing the current results with their counterparts from the direct friendship meta-analysis (Davies et al., 2011) revealed many similarities and very few differences. Foremost, the two meta-analyses found very similar aggregate results, again suggesting that these forms of cross-group contact relate to intergroup attitudes to a similar degree. We also compared their moderation results wherever possible. Both of them found larger effects from European versus North American studies and a trend whereby correlational effects were larger than experimental effects. Neither of them found moderation by age, gender, or in-group status. Their moderations by group domain were also similar in pattern, with religion yielding the largest effect in both meta-analyses, but different in where the differences lied: The largest difference was between religion and the other group domains in our meta-analysis but between the other group domains and ethnicity in theirs, which may suggest that extended contact and direct friendship are respectively most effective for contentious and nonvisible groups. Their moderations by intergroup attitude type was also different, with the current one finding it to be null but that on direct friendship finding larger effects on affective versus cognitive intergroup attitudes. These comparisons suggest that extended contact and direct friendship effects are very similar to each other, differing only in their moderations by group domain and intergroup attitude type.

Contributions

The most important contribution of this meta-analysis is determining the overall size of extended contact's relationship to intergroup attitudes. That is, although studies overwhelmingly agree that the extended contact hypothesis is valid, the size of this effect has varied widely (SD = .15). The definitive small-to-medium effect size that this meta-analysis found is therefore an important contribution to the extended contact literature. Statistically, having this effect size to target in power analyses is fundamental to the health of this active research area.

The three secondary aggregate effect sizes in this meta-analysis also offer important novel insights about how extended contact and direct friendship relate to each other. The multivariate results provided the first comprehensive comparison between extended contact and direct friendship, finding their effects to be at parity overall. Finding direct friendship

effects to be larger than those of extended contact specifically for religious groups is also important, especially because it explains a discrepancy in the literature wherein theory and individual study results favor direct friendship over extended contact (Paolini et al., 2007, Study 1; Paolini et al., 2004, Studies 1 and 2) but meta-analytic results put them at parity (Lemmer & Wagner, 2015). In addition, the partial direct friendship effect size is the first estimate of the relationship between direct friendship and intergroup attitudes that is independent of extended contact, which adds to the direct friendship literature and complements the meta-analysis thereon (Davies et al., 2011). The decrement of this effect size from its full counterpart, which suggests that extended contact contributes to direct friendship's effect, makes this finding especially important and suggests that future research on direct friendship should consider the contribution of extended contact.

The current moderation results are also important. Especially so are those relevant to measuring extended contact, which we are the first to propose and test; highlighting the importance of perception in extended contact, these results are theoretically interesting and practically useful. This meta-analysis also provided the first tests of moderation by group domain and intergroup attitude type, the latter null result of which counters theoretical predictions that extended contact should work better on cognitive versus affective intergroup attitudes. In addition to these important theoretical implications, these moderation results are also practically useful and can help target extended contact interventions for maximum effectiveness. Comparing these meta-analytic results with their direct friendship counterparts also provided interesting, albeit tentative, insight into how extended contact and direct friendship relate to each other.

The present meta-analysis is also important because it contributes to and nearly completes the series of meta-analyses on intergroup contact. It specifically complements the meta-analyses on direct contact (Pettigrew & Tropp, 2006), direct friendship (Davies et al., 2011), and imagined contact (Miles & Crisp, 2014). After adding the present meta-analysis and the one on media contact that is currently under way (Zhou et al., 2017), a meta-analysis will exist on every form of intergroup contact in the literature. Completing this series of meta-analyses would provide insight about each form of intergroup contact on its own and allow comparisons among them. In the field of intergroup contact, which consists of several hundred studies spanning over 60 years, comparing effect sizes across contact types is crucial to achieving its ultimate aim of determining the role that cross-group experiences play in prejudice.

Limitations and Future Research

As with any meta-analysis, the current one is limited by the relevant research that is available. Although recent waxes in the extended contact literature has provided a good sample size for this meta-analysis, this literature is still limited in several ways. The largest of such limitations is the few experimental studies that it contains, which limited power when testing moderation by control condition. The definition of extended contact that dominates this literature confounded conceptual and operational considerations while creating an overlap between extended contact definition and perceived or actual extended contact that disallowed independent examination of their moderations. Complications in standardizing the direct friendship means also made its moderation result difficult to interpret. Due to such limitations, we treat these moderation results as preliminary and important subjects for future research.

One important subject for future research is examining extended contact and direct friendship effects in conjunction. Research should specifically continue to investigate their relative effects on intergroup attitudes and how they change depending on various factors such as group domain. More important than comparing their effects is determining how extended contact and direct friendship work together to maximally improve intergroup attitudes. One important direction for this line of research will be to empirically test the idea that extended contact is a precursor to direct friendship, encouraging people to engage in personal intergroup contact and friendships while potentiating their effects on intergroup attitudes. Such research would speak to how extended contact and direct friendship relate to each other and determine how practitioners should use these forms of intergroup contact so they are maximally effective.

Another interesting line of inquiry pertains to perceived or actual extended contact. One related question is whether actual extended contact does anything beyond allowing other in-group members to know about it. Actual extended contact may indeed have a route to intergroup attitudes that is independent of perceived extended contact: Having a cross-group friend improves the in-group member's attitudes toward his or her friend's group, which then does the same thing for other in-group members via in-group norms. In addition, the effect of perceived extended contact that is not based in reality is also worth examining. Such research would determine the unique roles that reality and perception play in extended contact effects, which is theoretically interesting and can inform practical applications thereof.

Finally, future research should examine the relative efficacy of extended contact's various definitions more closely and systematically. Specifically, studies should use more varied conceptual definitions of extended contact that isolate its components—quantity and closeness of the direct and extended relationships—to determine which one or combination thereof affects intergroup attitudes the most. Such research would identify the "active ingredient" in extended contact and the best ways to target interventions based thereon. For example, finding direct relationship quantity to be key in extended contact would support a campaign that shows many people from the intended population with their friends from the target out-group.

Conclusion

Extended contact is relatively new to the intergroup contact literature and, perhaps for this reason, it has struggled for research attention. Although the seminal article on this topic was published 20 years ago, extended contact research really took off only in the past 10 years. We hope that this meta-analysis will encourage further research on extended contact, answering the aforementioned questions as well as others, to better understand it both alone and in its relationship with other forms of intergroup contact.

Learning more about extended contact is important for several reasons. Foremost among them is that extended contact is the first form of indirect contact and has inspired theory on other forms thereof. Its effect also has several interesting and novel mediators. Practically, extended contact disseminates easily through avenues like personal conversation, observation, and mass media; in fact, merely perceiving that in-group members have cross-group friends is enough to improve intergroup attitudes. Both theoretically interesting and practically useful is that extended contact purportedly involves low intergroup anxiety, high levels of which hinder direct contact effects, as well as high group boundary salience, which helps generalize positive regard for the out-group member to the entire out-group (Wright et al., 1997). Indeed, the current aggregate results suggest that extended contact's effect is just as strong as that of direct friendship and that extended contact contributes to direct friendship's effect just as much as vice versa. For these reasons, extended contact is an important part of the intergroup relations literature and should receive more research attention.

This meta-analysis statistically summarizes the 20 years of research that currently exists on the extended contact hypothesis, the idea that knowing about in-group members' cross-group friendships improves attitudes toward the out-group to which this friend belongs. Supporting this hypothesis, the current results determined that extended contact's relationship to intergroup attitudes is small-to-medium overall and holds across various considerations. Moderation results indicated that perceived extended contact works better than actual extended contact, and that extended contact may work better in Europe than in North America, for religious out-groups, and when it occurs naturally. Finally, comparing these results with their direct friendship counterparts revealed these forms of intergroup contact to be quite similar to each other. In addition to uncovering these unique and important insights about the extended contact effect using the relevant research from the past 20 years, we hope this meta-analysis will spur work thereon so this literature continues to burgeon over the next 20 years and beyond.

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Notes

1. Beelmann and Heinemann (2014) included studies of any intervention that aimed to improve intergroup attitudes in children; consequently, only half of the interventions in that meta-analysis were based on intergroup contact, another third of which were based on indirect contact. Lemmer and Wagner (2015) focused exclusively on interventions based on intergroup contact: most (nearly 80%) of them were based on direct contact, and half of the ones based on indirect contact were specifically about extended contact.
2. We searched the extended contact literature for delayed posttest effect sizes and found 10 of them across six studies. Given that they represent effects over time, whereas the current effect sizes represent immediate effects, combining them would only have been appropriate if we could test moderation by the time delay from measuring or manipulating extended contact to measuring intergroup attitudes. This time delay is zero for the 248 effect sizes currently in this meta-analysis and nonzero for the 10 delayed posttest effect sizes, which would not have offered sufficient variation for a proper moderation test. Unable to integrate these delayed posttest effect sizes into this meta-analysis, we conducted a separate mini meta-analysis on them. The results of this mini meta-analysis, which are fully reported in Supplemental Material A, indicated that extended contact effects weakened but remained meaningful over time.
3. The databases we searched were: Dissertations and Theses-Full Text, ERIC, Gender Studies, LGBT Life, Psychology and Behavioral Sciences, PsycArticles, PsycCritiques, PsycExtra, PsycInfo, SocIndex, and Social Sciences.
4. The two studies we excluded looked at extended contact with members of multiracial families, instead of with an individual out-group member (du Toit & Quayle, 2011), and extended contact through ancestors who were several generations removed (Stasiuk & Bilewicz, 2013).
5. We analyzed these correlations as they were, rather than applying Fisher's *r*-to-*z* transformation.
6. Locations coded as *other* were Nigeria, Chile, Japan, and Australia.
7. Group domains coded as *other* were age, gender, and civilians versus police officers.
8. Nonstranger in-group members included friends whom participants brought to the study (Ioannou, 2009; Openshaw, 2015) and popular media personalities with whom participants may have formed a parasocial relationship (Cummings, 2013).

9. Intraclass correlation coefficients (ICC) for the clustering of effect sizes within studies and of studies within articles were ICC = .71 and ICC = .50, respectively, which are both high enough to justify the multilevel structure that we adopted for this meta-analysis.
10. Acknowledging that true comparability between constructs presumes equivalent reliability in their measurements, we compared the within-study reliabilities of the extended contact and direct friendship measures whenever possible. The result of this analysis indicated that they did not differ, $t(7) = 1.68$, $p = .14$, suggesting that the reliabilities of these measures were indeed comparable within studies. Note that the power of this analysis was limited, however, because the necessary information was often either unavailable or irrelevant in the case of single-item measures.
11. We centered the continuous potential moderators around their corresponding grand means, instead of study means, because we wanted to examine variation within each predictor overall instead of within studies.
12. We acknowledge that this analysis did not control for geographic dependency, which occurs because similarity between countries generally decreases with the distance between them. We addressed this issue by running a regular multilevel model, which reproduces the current meta-analytic model, by nesting effect sizes within studies and studies within articles and weighing effect sizes by their corresponding weights from the meta-analytic model. We specifically conducted this model with and without an additional intercept for location, finding that these two models did not differ significantly, $\chi^2(1) = 2.63$, $p = .11$. This result suggests that the variance explained by study location is insufficient to warrant the additional complexity that accounting for geographic dependency would introduce into this model.
13. Considering the possibility that the relationship between extended contact and intergroup attitudes could be bidirectional, which is something else that correlational studies would pick up but experimental studies would miss, we wanted to examine the relative strengths of the two possible causal paths between them. To tentatively examine this question, we tested moderation by whether a correlational study presented the measure of extended contact before that of intergroup attitudes or vice versa. This result was not significant, $Q(1) = 0.42$, $p = .52$, but the few studies for which this methodological information was available ($k_2 = 14$, $k_1 = 24$) limits the interpretability of this null finding.
14. We acknowledge that the reverse causal direction is also plausible, and that the relationship between perceived extended contact and intergroup attitudes could be bidirectional. That is, better intergroup attitudes may improve someone's tendency to notice and remember his or her fellow group members' cross-group friendships. This increased perceived extended contact would then circle back to improve the person's intergroup attitudes as per the extended contact hypothesis.

Supplemental Material

Supplementary material is available online with this article.

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