The Apple Does Not Fall Far From the Tree, or Does It? A Meta-Analysis of Parent–Child Similarity in Intergroup Attitudes

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Understanding the formation of prejudice, stereotypes, and discrimination has long been a core topic of social psychology. Since the seminal theorizing by Allport in 1954, different views on childhood origins of prejudice have been discussed, in which the role of parental socialization varies on a scale from fundamental to negligible. This meta-analysis integrates the available empirical evidence of the past 60 years and critically discusses the current state of knowledge on parental socialization of intergroup attitudes. A random-effects model analysis of data from 131 studies on over 45,000 parent–child dyads indicated a significant medium-sized average effect size for the correlation between parental and child intergroup attitudes. The average effect size was related to study-specific variables, such as the source of parental attitude report (self vs. child reported), the conceptual overlap between measures, and the privacy of assessment. We also found significant moderations by ingroup status and size as well as child age. The latter was, however, mediated by measurement overlap. No significant effect size differences were found in relation to different components of intergroup attitudes (i.e., affective, cognitive, behavioral), nor to child or parent gender. The results unequivocally demonstrate that parent–child attitudes are related throughout childhood and adolescence. We discuss in detail whether and to what extent this interrelation can be interpreted as an indicator of parent–child socialization to allow a critical evaluation of the available contradicting theories. We furthermore address limitations of the available research and the current meta-analysis and derive implications and suggestions for future research.

Keywords: intergroup attitudes, prejudice, stereotypes, parent–child socialization, meta-analysis

Young children often express prejudiced attitudes and stereotypes; use racist, sexist, or other derogatory language; and discriminate against other children based on social group memberships. A high number of studies have demonstrated that the first signs of intergroup biases can be observed in children as young as 4 years old (for recent meta-analyses, see Doosje et al., 2011; Raabe & Beelmann, 2011). Laypeople typically assume that children have learned the underlying beliefs and attitudes from close others, primarily their parents. This article focuses on this assumption: Is childhood prejudice actually socialized, and particularly socialized within the family? To answer this question, we carried out a meta-analysis of studies assessing the relationship between parents' and their children's intergroup attitudes, enabling us to (a) determine the average effect size of parent–child similarity in intergroup prejudice, (b) explore potential mediators of this rela-

tionship, and (c) discuss whether and to what extent parent–child similarity can be interpreted as indicating parent–child socialization. We thereby included various facets of intergroup attitudes, such as prejudice as an evaluation of social groups and their members, stereotypes as associated beliefs and expectations regarding characteristics, roles and behaviors of social groups and their members, and intergroup behavior, including discriminatory intentions and interaction behaviors.

Early Theorizing on Prejudice Formation in Childhood

In his seminal book *The Nature of Prejudice*, Gordon Allport emphasized that, especially in the preschool years, "the home influence has priority, and that the child has excellent reasons for adopting his ethnic attitudes ready-made from his parents" and that "up to puberty, children's prejudices are mostly secondhand," "parroted views of the parents," and "reflected ethnocentrism of the immediate culture" (Allport, 1954, pp. 297, 312). Allport distinguished thereby two socialization processes: a process of adopting parental prejudice as the direct transfer of parental words and gestures, along with their concomitant beliefs and views on the one side, and a process of developing prejudice through parents' creation of an atmosphere in which prejudice forms in their offspring. For example, parental behavior focusing on discipline, threat, and punishment, together with a focus on obedience and unquestioned authority, and the suppression of the child's anger can create an atmosphere that leads the child to acquire a hierarchical view of social relations along with suspicions, fears, and

hatred that sooner or later may fix on minority groups. The later assumptions relate to theorizing on the authoritarian personality, which was quite popular at that time (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950). These postulates surely match our lay intuition that it is the tree that the apple does not fall far from, and according to Allport, we would indeed expect children to be rather similar to their parents in their level of intergroup prejudice, either because they imitate parents' attitudes or because parents recreate environmental circumstances in which the child forms the same attitudes his or her parents have. Interestingly, although Allport's work has triggered a vast amount of research on intergroup attitudes in social psychology, the topic of prejudice formation in childhood has rarely been taken up, and so far social psychology is rather mute on childhood development.

Social Psychology's Theories of Prejudice Formation

Understanding the origins of intergroup bias, prejudice, stereotypes, and discrimination has always been one of the core topics of social psychological theorizing and research. Throughout the decades, a number of affective, cognitive, and motivational structures and processes have been identified to underlie intergroup biases (for excellent recent overviews, see Dovidio, Hewstone, Glick, & Esses, 2010; Nelson, 2009). Although social psychological theories have been very effective in explaining prejudice, stereotypes, and discrimination in adults, it is typically beyond their scope to explain the formation of these structures and processes in childhood and adolescents. Interestingly, when addressing children's intergroup attitudes, social psychology tends to attribute them to early socialization processes via parents, peers, the school, or the media (e.g., E. R. Smith & Mackie, 2007), but does not investigate these assumptions specifically. For example, in her seminal model on automatic versus controlled components of intergroup attitudes, Devine (1989) assumed that children acquire stereotypes through common socialization experiences before developing the cognitive ability or flexibility to critically assess the validity or acceptability of these beliefs. Focusing on the cognitive structures and processes underlying intergroup biases, Devine's model does not contain any clarification about the timing, nature, or the sources of these early socialization experiences.

The current meta-analysis specifically focuses on the presumably most important of these assumed socialization sources: parents. Do children indeed acquire intergroup attitudes from their parents? To what extent does such acquisition depend on the children's age? Is it only the young children that take over parents' attitudes, and are adolescents thus more independent from the home but more influenced by peers? How would a possible finding of parent–child socialization effects be integrated into our adult theories on the formation of intergroup prejudice, stereotypes, and discrimination? Shifting our focus of attention toward developmental psychology shows that the topic of prejudice formation during childhood has received some more attention here. Specifically, sociodevelopmental theories have weighed in on the role that parents and primary caregivers have in the formation of intergroup prejudice, with strong focus on racial and ethnic prejudice formation in early or middle childhood (e.g., Aboud, 1988; Bigler & Liben, 2006) or early adolescence (e.g., Nesdale, 2004).

Social-Developmental Theories of Prejudice Formation

In the mid-1980s Frances Aboud reviewed the available empirical evidence on the occurrence of intergroup prejudice in children, which at the time appeared to be characterized by two main findings. First, consistent age differences were observed with an increase of prejudice expression between the ages of 3 and 7 years and a later decrease until the age of 12. Furthermore, the few available studies that assessed prejudice in children and their parents did not find reliable correlations between the two. In an attempt to explain these findings, Aboud developed the social-cognitive developmental theory (SCDT; Aboud, 1988, 2008), a neo-Piagetian theory that states that prejudice at different ages in childhood is based on parallel developmental changes in children's dominant mode of information processing (from affective to perceptual to cognitive) and their dominant focus of attention (from the self to groups to individuals). The main conclusion for our current interest is that because of their attentional focus and cognitive limitations, young children are inevitably prejudiced up to the age of 7, independent of the input they receive from parents or others in their environment. Because children's prejudices did often not correlate with their parents', Aboud assumed that children cannot have adopted them from their parents, implying that parent–child transmission or socialization would be irrelevant (Aboud, 1988). A further argument for this assumption was that children would lack the cognitive capacities enabling them to understand and internalize their parents' attitudes before the age of 7 years, thus ruling out early parent–child transmission effects. Interestingly, however, children appeared to think that their parents hold attitudes similar to their own—an effect that Aboud and others explained as projection or false-consensus effect: Children may erroneously believe that their parents do see the world the way they do (e.g., Gniewosz & Noack, 2006). Since Aboud's theory, further recent accounts have been presented that adopt a more multifaceted approach to children's development of intergroup attitudes while yet encompassing the possibility of a parental influence.

For example, a more complex perspective on children's prejudice formation is presented in Drew Nesdale's social identity development theory (SIDT; Nesdale, 1999, 2001a, 2004; Nesdale et al., 2007), which draws heavily on social identity theory (Tajfel & Turner, 1979) and self-categorization theory (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987). Nesdale points out that expressions of intergroup biases in early childhood are driven by mere ingroup preferences and do not imply negative outgroup evaluations. He suggested that these early ingroup preferences are based on self-categorization, ingroup identification, and biased social comparisons, just as in adults, and accordingly emerge in children as soon as they develop the social and cognitive abilities for social categorization, self-identification, and social comparison around the age of 5 or 6 years (Nesdale & Flesser, 2001). Thus, as in social identity theory, no specific socialization input—neither from parents nor other socialization agents—is expected to play a role for the formation of ingroup preferences, because it appears as an inevitable consequence of self-categorization and motivated (positive) social comparison. Nesdale, however, clearly distinguished the formation of prejudice in terms of outgroup negativity and rejection from mere ingroup preference. He assumed that negative outgroup prejudice only crystallizes starting at the age of

7 years—but not for all children. Dislike or even hatred for outgroups and their members only form if children adopt negative ethnic attitudes prevailing in their social ingroup—significant others in their environment whom they value and identify with. Most importantly, this has to be construed as an active process in which the child decides to adopt specific attitudes as his or her own only when the child has or forms a view of him- or herself as belonging to a group with these particular set of attitudes, beliefs, and behaviors. The adoption of prejudice is facilitated if children (a) highly identify with an ingroup holding a norm of negative outgroup attitudes, (b) perceive tensions and threat between social groups or their members, and/or (c) perceive open conflict between groups or their members.

The implication here is that children may have similar intergroup attitudes to those of their prejudiced or unprejudiced parents, but that similarity is not guaranteed, because the particular attitude adopted by children rather reflects their own interests and social group identifications that may or may not overlap with their parents' interests and attitudes. Especially during late childhood and adolescence, peer groups might play a more important role as providers of ingroup norms of outgroup prejudice than parents. Accordingly, Nesdale concluded that "a positive correlation between the intergroup attitudes of parents and their children is not necessarily to be expected, particularly as children increase in age and their social environment expands" (Nesdale, 2001a, p. 69). Thus, whereas SCDT assumes that parental input does not matter in the early childhood formation of intergroup attitudes, SIDT is compatible with some parental input decreasing in later childhood and adolescence and being only one of many possible influences.

Both previously presented theories focus on age-related developmental changes in selected processes assumed to drive the formation of intergroup attitudes in children, be it either a focus on cognitive processes in SCDT or a focus on cognitive-motivational processes in SIDT. In the following, we discuss two more integrative theories that include both cognitive and social-motivational processes and additionally emphasize the role of a multiplicity of different factors in the development of children's intergroup attitudes: the developmental intergroup theory (DIT) by Rebecca Bigler and Lynn Liben (Bigler & Liben, 2006, 2007) and the related societal–social–cognitive–motivational theory (SSCMT) by Martyn Barrett (Barrett, 2007; Barrett & Davis, 2008).

The main assumption of DIT and SSCMT concords with SCDT (Aboud, 1988) and SIDT (Nesdale, 2001a, 2004) in that social stereotypes and prejudice in children are assumed to result from children's active and constructive interaction with their social environments rather than a passive learning of ready-made attitudes provided by parents as assumed by Allport (1954) or Devine (1989). DIT specifies that children assign social meaning to those categories that are salient to them, based on their perceptual distinctiveness (e.g., visibility of group differences), proportional group size (e.g., minority vs. majority status of groups), and perceptions of adults' explicit labeling and functional use of social categories. Thus, parental explicit labeling (e.g., "boys," "African American") or functional use (e.g., "only girls wear dresses") renders some social categories more likely to be perceived as socially meaningful by children than other categories. In this process parents are not assigned any specific socialization role different from other agents. Once children form and apply these

categories (based on individual and developmental differences in their motivation and ability to classify), they start constructing meaning by attaching beliefs and evaluations to the categories. This process is partly internally driven, as children tend toward essentialist thinking (Gelman, 2003)—assuming that individuals who share visible characteristics (e.g., skin color) must also share invisible characteristics (e.g., traits)—and have a self-related motive to see the own group as positive. The process is also externally driven by information available in the social environment of the child. Children detect contingencies in their environment, such as social roles (e.g., most bus drivers are male); receive explicit messages from significant others (e.g., "boys don't play with dolls"); and observe verbal and nonverbal behaviors of others toward members of social groups. These internal and external sources interactively and reciprocally enforce the construction of links of social categories with attributes (traits, behaviors, roles) and affect (liking), thus the formation of stereotypes and prejudice, respectively. It is important to note, however, that whereas each of the described factors has an important role for the formation of stereotypes and prejudice, any single factor alone is unlikely to lead to the development of intergroup attitudes. Accordingly, Bigler and Liben (2006) conceptualized parental attitudes as being only one among many influences on the formation of intergroup attitudes in children, concluding that "children's social stereotypes and prejudices are unlikely to arise directly and automatically from those held by adults in the environment" (p. 52). They assumed that it is principally possible to learn intergroup bias via direct teaching and explicit instruction, but argued that directly taught attitudes would be less complex, less stable, and less deeply held than self-construed attitudes. According to DIT, parent–child similarity is thus not necessarily to be expected or expected to be of small effect size.

A similarly multifaceted theory is the SSCMT (Barrett, 2007), which focuses in more detail on the various potential environmental sources from which children can potentially acquire information about the pattern of intergroup relationships and attitudes in their immediate and broader social environment, such as parents, teachers or peer groups, school environments, school textbooks, contents of the media, television, books, and the Internet. The most important difference from DIT is the assumption that parents are assigned a particularly influential role in that they potentially influence their children not only directly based on their own discourse and practices in relation to social groups but also indirectly via their choices of the child's environment (e.g., neighborhood, school, media access), thereby selectively providing potential influences that the child is exposed to and can attend, process, and assimilate to. According to SSCMT, parental socialization thus includes not only a direct but also an indirect transfer of intergroup attitudes. This theory is compatible with Phyllis Katz's view that

> it is parents, after all, who determine much about their children's world, including the neighborhoods they live in, the amount and type of television they view, the people who surround them, and also who their friends are—and all of these seem to matter. (P. A. Katz, 2003, p. 907)

A similar perspective is found in the socialization literature.

Socialization Perspectives

Traditional conceptions of socialization assume that families—specifically parent–child relationships—are the major context in which socialization occurs whereby the role of parents is the one to guide and assist children to incorporate attitudes, values, beliefs, and behaviors of the larger culture into their actions. Thus, parents are perceived as the principal agent of socialization in childhood. For example, in political or religious socialization research, parental attitudes have repeatedly been found to be main determinants of offspring attitudes (e.g., Dalhouse & Frideres, 1996; Glass, Bengston, & Dunham, 1986; Granqvist, 1998, 2002; Jennings, Stoker, & Bowers, 2009; Valentino & Sears, 1998). Children and adolescents adopt their parents' political and religious orientations to a certain degree, and this early parental transmission appears to influence the character of later adult religious or political development even though direct parental influences often appear to decrease with offspring age (e.g., Francis & Brown, 1991; Jennings et al., 2009; Myers, 1996). Assuming that intergroup attitudes undergo comparable socialization processes like other attitudes, values, and beliefs, one would thus expect a major parental influence on children's intergroup prejudice, stereotypes, and discriminative behaviors. Grusec (2011) summarized different domains or mechanisms by which parental socialization takes place, such as protection and secure attachment, mutual reciprocity, power and control over resources used to modify child action, guided learning and teaching, and group participation and identification. These domains contain different parental behaviors that can lead (with different effectiveness) to children's internalization of parental and societal standards, attitudes, values, beliefs, and behaviors. For our current interest, the domain of group identification seems to be especially relevant, as parents are assumed to provide ingroup–outgroup differentiations, as well as ingroup norms and values of appropriate attitudes and behaviors. Learning through observation (Bandura, 1977) and parental management of children's environments are thereby discussed as primary and most efficient ways of socialization (Grusec, 2011). Socialization, however, is not a one-way road but a bidirectional process in which children are active agents who process, accept, or discard incoming information, impose own frameworks on parental influence, and also socialize their parents by modifying their beliefs and values—increasing parent–child similarity. Thus, on the basis of socialization research, we would expect that parents do play a significant role in the direct and indirect shaping of their offspring's intergroup attitudes, leading to a considerable level of parent–child similarity. It should, however, be noted that some developmentalists have questioned this primacy of parental socialization—especially in the domain of intergroup relations and attitudes. Group socialization theory (J. R. Harris, 1995, 1998, 2000), for example, assumes that group processes are primary for socialization. Thus, attitudes, values, beliefs, and behaviors might be learned from parents but are retained by children only if they are approved and shared by their peer groups. Additionally, children's peer groups create their own culture by selecting and rejecting various aspects of the adult culture and by making cultural innovations of their own. Applying this perspective to the formation of intergroup attitudes, one would thus expect none or only a small correspondence of parent and child attitudes but a higher child–peer correspondence. We are thus once more confronted with contradicting expectations for the average effect size of parent–child similarity in intergroup attitudes.

The Need for a Meta-Analysis

To summarize, we are faced with a situation in which different models and theories in social and developmental psychology provide rather opposing predictions regarding the role assigned to parents in the formation of intergroup prejudice in children. Whereas Allport's (1954) assumptions on the priority of parental influence, especially in early childhood, fit traditional socialization theories (Grusec, 2011) and suit the layperson's assumption that the apple does not fall far from the tree, social-developmental theories predict either no influence (Aboud, 1988, 2008; see also J. R. Harris, 1995, 1998) or, if any, a marginal or moderate influence (Barrett, 2007; Bigler & Liben, 2006; Nesdale, 2004) that is expected to be moderated by a number of external and internal factors (e.g., ingroup identification; Nesdale, 2004).

Interestingly, the empirical evidence cited in the social and developmental literature to underline assumptions of no, little, or varying parental socialization of children's intergroup attitudes refers to the same relatively small set of correlational studies in which no or only small significant relations between parent and child prejudice were found (Aboud & Doyle, 1996; Bird, Monachesi, & Burdick, 1952a; Branch & Newcombe, 1986; J. M. Carlson & Iovini, 1985; Davey, 1983; H. D. Fishbein, 2002; Frenkel-Brunswik & Havel, 1953; Goodman, 1952; D. B. Harris, Gough, & Martin, 1950; Horowitz & Horowitz, 1938; P. A. Katz, 1976; P. A. Katz & Kofkin, 1997; Mosher & Scodel, 1960; Pushkin & Norburn, 1983; Radke & Trager, 1950; Radke-Yarrow, Trager, & Miller, 1952). This is striking because by far more evidence is available, although scattered across multiple disciplines. It is also problematic because some of these studies appear outdated, presenting research conducted at times when the open expression of negative intergroup attitudes, such as racist or sexist prejudice, was socially accepted and rather the norm. Also, several studies relied on small samples that are inadequate for detection of small effects in correlational research. Nevertheless, this limited selection of studies seems to build the empirical foundation of the argument that parents play no or only an insignificant role in the formation of childhood prejudice.

A meta-analysis systematically reviewing all available evidence is thus desired to provide reliable information whether and to what extent parents and children are similar in their intergroup attitudes. The most widely available effect size in this field of research is the correlation coefficient. In a first step, we therefore identified the average effect size and its significance level of parent–child correlations. In the next step, we further explored whether we can identify methodologically and/or theoretically relevant moderators of that effect size. Below, we propose a number of hypotheses formulated to explain how different sample-, study-, and method-specific factors might moderate the relationship between parental and children's intergroup attitudes, along with the empirical evidence supporting or contradicting them. These first steps are essential to allow drawing a firm conclusion whether and under which conditions we do find children's intergroup prejudice to resemble their parents' attitudes. On the basis of such findings, we will debate whether we can interpret such correlations as indicators of parental socialization, and thus whether we should include

parental socialization in our models and theories of prejudice formation in childhood and adolescence, and discuss implications for further research.

Possible Moderators of Parent–Child Similarity in Intergroup Attitudes

Measurement-Related and Study-Specific Moderators

The way in which prejudice is measured is a possible source of variability in the correlations between parental and children's intergroup attitudes. Below we discuss several hypotheses concerning the moderating roles of the characteristics of the study and specific measurements of intergroup attitudes in children and parents.

Source of parent information. When researching the similarity of parents' and their children's intergroup attitudes, the ideal procedure may be to obtain direct measures with the two groups of participants. However, practical limitations often do not allow such a direct approach. Therefore, a number of researchers opt to obtain direct measures from the more easily attainable participant group (children can often be easily approached in school or kindergarten settings) and rely on indirect information about the parents—that is, by asking the children to indicate their parents' attitudes. This approach, however, risks to bias results, as children's reports of their parents' attitudes might be inaccurate because children (a) might simply not know their parents' attitudes and (b) might assume that their parents have the same attitudes as themselves. Aboud and Doyle (1996) addressed such false consensus biases, assuming that children incorrectly and egocentrically believe that their parents hold similar attitudes to their own. Furthermore, Gniewosz and Noack (2006) demonstrated that such projection processes do indeed occur when children lack the knowledge of their parents' attitudes, but seem to play a more pronounced role in younger childhood, whereas older adolescents have more accurate representations of their parents' attitudes. If projection or false consensus processes do occur when children serve as informers of their parents' attitudes, parent–child similarity might be overestimated in these studies. We therefore conducted a moderator analysis testing whether the average effect size is elevated in studies using child reports of parental attitudes as compared to studies relying on parental self-reports.

Measurement overlap. Developing valid and reliable measures of intergroup attitudes and stereotypes is a challenging endeavor, especially when targeting young children who might lack the cognitive or verbal abilities to complete measures that are typically used in adult research (e.g., Nesdale, 2001b). Unfortunately, child-friendly measures, adapted to the specific cognitive, linguistic, and social stage of children's development, often differ from adult measures used with parents and thereby often risk assessing different underlying concepts. For example, in the seminal study by Aboud and Doyle (1996), racist attitudes in children were assessed by trait assignments to individual group members with the Multi-Response Racial Attitude Measure (MRA; Doyle & Aboud, 1995) and the Preschool Racial Attitude Measure (Williams, Best, Boswell, Mattson, & Graves, 1975), whereas attitudes in parents were assessed with a typical prejudice scale including pro-Black and anti-Black items (e.g., I. Katz & Hass, 1988). In this

example, both measures focus on racial attitudes; however, they assess different components of these attitudes: They focus on the outgroup as a whole in relation to the entire society in the parental measure and focus on possible traits in individual group members in the child measure. Using different instruments to measure intergroup attitudes in children and parents generally decreases the likelihood of finding significant correlations (e.g., Nesdale, 2001a). In extreme cases, measures might assess entirely different concepts in parents and children, such that little correspondence can be expected. We therefore coded the conceptual similarity of measures employed with parents and children to enable us to analyze whether this methodological factor moderates the reported child–parent correspondence.

Studied concept. We explored whether average effect sizes differ in studies on prejudice (affective and evaluative component) stereotypes (cognitive component) or discrimination (behavioral component), although there are no theories explicitly predicting such differences. However, given various theories about the "primacy of affect" (e.g., Zajonc, 1980), one may assume that affective components of intergroup attitudes are learned better or more directly than cognitive components. For example, C. S. Crandall, Bahns, Warner, and Schaller (2011) recently demonstrated that learned valence associations can form the basis of stereotype formation in response to new groups. On the other hand, one could also assume that cognitive components (i.e., stereotypes) are learned first and form the basis of intergroup evaluations and emotions (e.g., Fiske, Cuddy, Glick, & Xu, 2002). In relation to observational learning processes and imitation (Bandura, 1977), one may also assume that intergroup behaviors are directly imitated, but not the underlying beliefs and evaluations, because the child cannot understand them yet (e.g., Aboud, 1988; Nesdale, 1999, 2004). According to the latter view, one would expect higher parent–child correlations when assessed with behavioral measures as compared to measures of affective or cognitive components of intergroup attitudes.

We had considered additionally coding whether the measures focused on the mere knowledge or awareness of intergroup attitudes as compared to personal beliefs and endorsement of those attitudes (e.g., Devine, 1989). One may assume that children's knowledge of intergroup attitudes might be stronger related to their parents' attitudes as compared to the personal endorsement and internalization of such attitudes. However, in the majority of studies, this information could not be coded unanimously, and we therefore could not conduct this moderator analysis.

Study domain (target group). It is generally conceivable that parent–child similarity might differ in different intergroup domains. For example, group memberships that are more salient (e.g., race), addressed more frequently (e.g., gender), or discussed more openly (e.g., religion) might lead to higher parent–child correspondence than memberships that are less salient or openly discussed (e.g., sexual orientation), although none of the theories makes an explicit prediction in this direction (but see Bigler & Liben, 2006). We therefore conducted an explorative moderation analysis testing whether the average effect size of child–parent similarity differs across intergroup domains.

Assessment situation. A further possible study-related moderator is the assessment situation—in relation to social desirability effects based on currently widely accepted norms of tolerance and egalitarianism in most Western societies. Especially adults (par-

ents) might be motivated to conceal their levels of agreement with prejudiced intergroup attitudes when they perceive these as socially undesirable. When individuals act according to such shared social desirability standards, variance in attitude expression might be reduced, thus reducing the possibility of finding significant parent–child correlations. Social desirability concerns are typically considered to be less important in young children, who are assumed to express their attitudes uninhibited (e.g., Doyle, Beaudet, & Aboud, 1988). However, we know that social desirability concerns are at least somewhat present by age 6 (e.g., Rutland, Cameron, Milne, & McGeorge, 2005) and are considered to increase in children with increasing age (e.g., Nesdale, 2001b). It is, however, possible that such concerns may be more profound in parents. If children and/or parents mask their attitudes, the observed parent–child attitudinal similarity would be reduced. Unfortunately, only very few studies include direct measures of social desirability for children (e.g., V. C. Crandall, Crandall, & Katkovsky, 1965) or adults (e.g., Crowne & Marlowe, 1960) and thus do not allow a direct examination of its possibly biasing influence. However, social desirability concerns would be expected to be stronger when people are directly asked to openly express their attitudes, for example, in interviews, as compared to more private assessment situations, such as filling in a questionnaire on one's own (e.g., Devine, 1989). In absence of more direct markers of social desirability concerns, we therefore used the assessment situation as an indirect indicator for the possible influence of social desirability concerns and tested whether it was a significant moderator of parent–child correspondence in intergroup attitudes.

Sample-Specific Moderators

Child age. A further possible moderator, child age, is interesting from a theoretical as well as from a methodological perspective. From a theoretical perspective, there is ample reason to assume that parent–child similarity might differ depending on the age of the child because of different potential of parents' influence on their offspring. Child age in this regard serves as an indirect indicator of age-related developmental variables such as cognitive skills, social skills, the frequency and intensity of parent–child interaction, and interaction with other socialization agents such as peers or teachers. Predictions of age differences based on theories of prejudice formation, however, are rather contradictive. For example, Allport (1954) assumed that prejudice in early childhood would mainly be based on parental input, which decreases with increasing age of the child. We would thus expect a significant inversed relation of parent–child similarity with child age. Aboud (1988), on the contrary, noted that below the age of 7 years, no parent–child similarity can be expected (a) because young children lack the social and cognitive abilities to perceive, understand, and internalize their parents' attitudes and (b) because their intergroup attitudes stem from a normative sociocognitive developmental course and do not depend on external influences. Nesdale (1999, 2004) provided a further different perspective, pointing out that some parent–child similarity might be expected in very young age, for example, if children "ape" racist language or behavior from their close environment without truly adopting them as their own. In later childhood and adolescence, however, parent–child similarity would not necessarily be expected to be observed because attitude formation would be more influenced by various peer

groups and school experiences (Nesdale, 2004) but also by one's own interracial encounters (P. A. Katz, 1976). In this line, a number of researchers have suggested reduced general susceptibility to parental orientations in adolescence because the parent–child relationship tends to change during the transition to adolescence. Teenagers' assertion of independence is likely to lead to renegotiations of parents' rules, norms, and values and thus even intentional dissimilarity in their attitudes (e.g., Jaspers, Lubbers, & de Vries, 2008; P. A. Katz, 1976; McGue, Elkins, Walden, & Iacono, 2005). However, despite the popular beliefs that adolescents turn away from their parents, most empirical research in other domains of value socialization reveals a striking concordance between worldviews of parents and those of their adolescent children (e.g., Vollebergh, Iedema, & Raaijmakers, 2001). Additionally, one could propose that the accumulation of enduring direct and indirect parental influence throughout the years of childhood might only be unveiled during adolescence or young adulthood when personal attitudes form and stabilize as part of identity development (e.g., Meeus, 1996). We are thus faced with contradictive predictions regarding possible age differences of the parent–child similarity of intergroup attitudes.

Child age moderations of parent–child similarity might also be expected based on a methodological perspective. As mentioned earlier, developmental research on intergroup attitudes faces the challenge of using measures adapted to the cognitive and linguistic abilities of the children. Therefore, researchers working with younger children might see a higher need to use child-adapted measures that are inevitably less similar to parental measures. Studies with older children or adolescents on the other hand are more likely to employ the same or highly similar measures with parents and children. Thus, the conceptual overlap of parent and child measures might be positively related to child age, and higher levels of parent–child similarity can be found in higher child age merely based on increased measurement similarity. We will therefore test for mediated moderations, exploring whether (a) the relation between child age and parent–child correspondence might be mediated by measurement overlap and/or (b) the relation between measurement overlap and parent–child correspondence is mediated by child age.

Parent and child gender. Several hypotheses can be put forward regarding possible influences of parent and child gender. On the one hand, one might expect stronger similarity between mothers and their children as compared to fathers, simply because in traditional family settings, mothers tend to spend more time with their children (e.g., Bailey, 1994) and only modest increases in paternal involvement in child care have occurred in recent years (Coltrane, 2000). Because of higher child care involvement, mothers may have more opportunities to communicate own values and attitudes that might lead to higher mother–child than father–child similarity in intergroup attitudes (e.g., Jaspers et al., 2008; Tenenbaum & Leaper, 2003). We are not aware of any explicit assumptions regarding girls or boys being more or less influenced by parental attitudes, although it is possible that based on parents' differential treatment of boys and girls, differences in attitude similarity occur. A meta-analysis by Lytton and Romney (1991), however, indicates that in most socialization areas, parents do not make systematic differences in their rearing of boys and girls. Gender-based differences in socialization behavior appeared only in regard to sex-typed activities and physical punishment. Because

these areas might be directly or indirectly related to intergroup attitudes (e.g., gender attitudes, social dominance orientations), we conducted an exploratory moderation analysis of child gender. Last but not least, the interaction of parent and child gender might play a role as moderator of parent–child similarity, for example, such that parent–child correspondence would be higher when child and parent gender match than if they do not match. Especially in the domain of gender attitudes, it has been repeatedly argued that children's understanding of gender and their gender attitudes might be shaped more by the same-gender parent (e.g., Russell & Saebel, 1997), although the empirical evidence is rather mixed (e.g., Burt & Scott, 2002; Cunningham, 2001; Tenenbaum & Leaper, 2002). We will therefore test not only the independent moderating roles of parent or child gender but also their interacting effect on parent–child similarity in intergroup attitudes.

Own group status. It is conceivable that correspondence between parental and children's intergroup attitudes differs between social groups of different status, be it because of different socialization practices in relation to intergroup attitudes or because of general cultural differences in parenting. For example, research on ethnic identity development documents that racial or ethnic minorities tend to use explicit teaching strategies to teach their children about their cultural heritage and intergroup attitudes, and prepare them for possible discrimination (e.g., Hughes & Chen, 1997; Hughes et al., 2006). In high-status majority groups, however, such explicit teaching is rather rare, and a high number of parents simply prefer not to mention or discuss intergroup attitudes (e.g., P. A. Katz, 2003). Group status might be based on numbers alone (e.g., typically majorities have higher status), but not necessarily so, with minorities sometimes having higher status and power (e.g., the White minority in South Arica) or equal size groups having different degrees of power (e.g., men and women). To analyze whether group status does indeed moderate parent–child similarity of intergroup attitudes, two possible moderator variables are thus available, the one being relative size of the ingroup and the other being relative group status of the ingroup. We expected parent–child similarity to be higher in lower status minority groups as compared to higher status majority groups.

Method

Literature Search Procedure

To survey the literature on the relationship between parental and offspring intergroup biases, we performed computer searches of the following databases: PsycINFO, Dissertation Abstracts International, PsycARTICLES, PSYNDEX, Web of Science, Science Direct, and Educational Resources Information Center, added by a search via Google Scholar. We used a combination of a wide array of terms referring to intergroup biases and terms referring to parental attitudes. The specific search terms were (*prejudice OR stereotype OR racism OR ethnocentrism OR xenophob* OR ((intergroup OR intergroup OR ingroup OR in-group OR outgroup OR out-group OR ethnic OR immigra* OR foreign* OR minority OR gender OR sex OR women OR homosexual OR lesbian OR gay OR weight OR anti-fat OR religion) AND (attitude OR bias OR preference OR favoritism OR favouritism OR evaluation OR attitude OR rejection OR exclusion OR dynamic)) AND (parent OR mother OR father OR intergeneration* OR transmission OR*

socialization OR similarity). Our search covered all the published articles that were available in the databases up until December 2010. Prior to acceptance of this article, this research was updated in order to include recent publications between January 2011 and July 2012. In the initial literature research we retrieved more than 5,000 references that were submitted to a brief review of the titles and abstracts. This review resulted in 686 references that appeared to report at least one study in which intergroup attitudes were measured in both children and parents, thus matching the inclusion criteria for the meta-analysis explained below. In addition, we sent a request for unpublished studies to the e-mail lists of the Society for Personality and Social Psychology, the European Association of Social Psychology, and the European Sociological Association. A further call for papers was published on the web page of the Society for Research in Child Development. We also contacted the European Society for Developmental Psychology and the International Sociological Association asking for publications of the call for papers, but did not receive affirmative responses. These calls for papers received 13 responses, providing, however, only one published article and two unpublished reports that were not already retrieved during the previous database searches.[1]

We then read all articles that were available and written in Dutch, English, French, German, Italian, Portuguese, or Spanish to evaluate eligibility of each article for the current project. Sixteen references that were judged as potentially eligible from the titles and/or abstracts were not available and therefore not included in further searches or analyses. Finally, we conducted a forward and backward reference research using both the Web of Science database and Google Scholar to identify any publications that referred to or were referred by any article that was judged eligible for inclusion. This procedure allowed us to identify 32 further references that were not retrieved during the previous searches.

Inclusion Criteria

We established study eligibility using the following inclusion criteria:

1. The study must report measures of intergroup attitudes and have assessed both parents' and their offspring's attitudes. For the purpose of the current meta-analysis, we defined intergroup attitudes as including affective, cognitive, and/or behavior components, thus including measures of group-related evaluations (e.g., prejudice, ingroup pride), beliefs (e.g., stereotypes, expectations, role assignments), relations (e.g., intergroup contact, intergroup friendship, dating), and/or behaviors (e.g., discrimination, aggressive behaviors, intergroup helping). In this regard, *intergroup* refers to socially categorized groups, for example, based on ethnicity, religion, and body weight. Although we had planned to include studies with novel groups as well, no such study fulfilling our inclusion criteria appeared during our literature research. We included direct (e.g., self-report in questionnaires) as well as indirect measures (e.g., behavioral observations) of child attitudes. Measures of parental attitudes could have been obtained either from the parents directly or from children's or others' perception of parental attitudes (e.g., children's judgment how their mothers

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[1] Note that this search procedure inevitably favored inclusion of research conducted in North America and Western Europe, as research from other populations is less likely to be listed in the used sources.

or fathers would have filled out a questionnaire on intergroup attitudes).

We eliminated 154 references that did not include measures of intergroup attitudes and thus did not conceptually fit into the current research project. We further excluded 98 studies reporting measures of concepts that although related to intergroup relations, did not measure intergroup attitudes as defined above (i.e., perceived discrimination and stereotype consciousness, gendered toy or activity preferences, household task distribution, parental occupation status, parenting values and behavior, attitudes toward school subjects, attitudes toward sexuality or sexual behavior, general social values such as conservatism, traditional family values, political partisanship, or party preferences). As a decision criterion, we carefully evaluated whether the reported concepts included attitudes targeting socially meaningful groups or their members and excluded them when this criterion was not fulfilled. Furthermore, we eliminated 19 studies that reported parental but no child measures of intergroup attitudes and 140 studies that reported child measures but no parental measures of intergroup attitudes.

2. Data must have been collected from nonclinical samples with the mean age of offspring participants being below or equal to 21 years. Although this meta-analysis focuses on attitude socialization in childhood and adolescence, we included studies with a broad age range up to 21 years. This allows comparisons of parent–offspring similarity for different age groups. It is also possible that attitude transmission relies on accumulative processes during childhood and adolescence and appears only in late adolescence or young adulthood. Thus, studies including young adults are similarly informative. On the basis of this criterion, eight studies with samples of adults and their parents (mean age ranging from 23 to 44 years), one study in which offspring age was not specified, and two studies with clinical samples were excluded.

3. Parents were the biological parents. On the basis of this criterion, we excluded two references reporting samples of stepparents or international adoption parents.

4. The study is not based on experimental manipulation of child or parent intergroup attitudes. From the few experimental or intervention studies that we found ($k = 4$), we only included effect sizes of control groups or preintervention measures of experimental groups.

5. The study must report original empirical data. Editorials, reviews, keynotes, or book chapters and other theory-oriented publications ($k = 112$) were excluded if they did not report original data. To avoid duplication, data provided from samples that were published in other, already included reports were excluded from analyses ($k = 18$). When the reported sample was a subset of participants from a larger sample reported in a different article, the effect size from the larger sample was included.

6. The study had to include a test of the relationship between parent and child attitudes. For inclusion, appropriate statistical information about the relationship between parent and child attitudes had to be available. This information includes r values, t tests, F tests, chi-square values, or proportions. In cases of studies that included measures of intergroup attitudes in parents and their children without reporting their relationship, we contacted the authors of the article and asked for the missing data or information. If reported statistics were obtained from analyses that included additional variables (e.g., multiple regressions, structural equation

models), authors were contacted to provide zero-order correlations to include in the meta-analyses. Altogether, we contacted 49 authors or groups of authors for information about 66 references. The majority of authors ($n = 42$, 85.7%) responded with an average response time of 9 days (ranging from 0 to 125, $Mdn = 1$) and 37 (75.5 %) provided the needed information. This procedure allowed us to include 48 references that would otherwise have been excluded from the meta-analysis.

From longitudinal studies reporting several parent–child tests of relationships over time ($k = 12$), only the first wave statistics were included in order to maintain comparability with the majority of studies reporting cross-sectional data. Studies were excluded if no test of relationship was reported and not available from authors ($k = 33$). When results were reported selectively (e.g., "all other correlations were nonsignificant"), missing tests were coded conservatively as $r = .00$ ($k = 11$) instead of excluding the reference (see discussion below). Eventually, 129 publications met all inclusion criteria.

Coding Procedures

Eligible studies published in Dutch, English, French, or German were coded by either the first or the second author; other language studies (Portuguese, Spanish, Italian, Croatian) were coded with the help of the authors of the publications or other native speakers. To structure the coding process, we developed a clearly arranged coding manual that listed all relevant variables together with a brief explanation and the respective category assignments (see below). Coding was accomplished with a computerized entry form via Qualtrics software (Qualtrics Labs, Inc., Provo, Utah). If the information given did not allow for a definite coding judgment, data were marked as missing.

To establish reliability of the coding process, both authors coded 34 publications (29%) to determine interrater agreement. Agreement for categorical variables was indexed by kappa coefficients, which ranged from .80 to 1.00. Agreement for rating scales was determined with intraclass correlations, ranging from .88 to 1.00. Cases with doubts or disagreement during the initial coding phase were resolved through discussion, and all further publications were coded by either one of the two coders.

Recorded Variables

Most publications reported a single study, and only two articles reported two studies. A considerable number of studies reported results separately for subsamples (e.g., boys and girls, minority and majority children, children of different age groups). It was also common that studies reported more than one measure of children's intergroup attitudes (e.g., a thermometer scale and trait assignments), and some studies reported more than one measure of parents' attitudes from one or both parents. As a result, the included reports often reported several effect sizes for subsamples and/or different measures. In these cases each effect size was coded, along with all necessary information. Number of samples ranged between one and six per report ($M = 1.34$, $SD = 0.786$) and number of effect sizes ranged between one and 16 per report ($M = 2.39$, $SD = 2.04$), providing a total of 333 initial effect sizes coded for the meta-analysis.

Publication. We recorded the year of publication,[2] the form in which the report was published (published or in press article, submitted manuscript, or unpublished study), the publication type (journal article, book chapter, book, thesis or doctoral dissertation, technical report, conference paper, or other), the publication field (developmental psychology, social psychology, educational psychology, political psychology, health psychology or psychiatry, personality psychology, other psychology, sociology, anthropology, or other), and the nationality of the first author's affiliation.

Sample. Sample characteristics included sample size (number of child–parent pairs), sample nationality, sample race and ethnicity, sample representativeness (e.g., convenience or population-based sampling), sample homogeneity (in relation to the target categorization basis, see below), percentage of parental consent for study participation, child mean age and range, child gender (boys only, girls only, mixed) along with gender proportion in mixed samples, parent mean age and range, parent gender and/or proportion, parents' socioeconomic status, and educational level. A number of reports did not state the precise mean age and/or age range of the children. In these cases, we used all other available information to estimate the average age, mostly school grade (e.g., "fifth graders"), or other school-related terms (e.g., "preschool age," "elementary school," "high school") that we compared with information about the educational system of the respective countries. Information for several variables was more frequently missing than present (i.e., parent age, socioeconomic status, educational level, percentage of parent consent) such that these variables were not included in the analyses.

Study descriptions. We recorded the study design (cross-sectional, longitudinal) and how children and parents were approached for data collection. Therefore we coded in which locations participants were approached for data collection (in schools or child care centers, community centers, home visits by researchers, laboratory invitations, or letters) and whether data were recorded in a private (e.g., filling questionnaires out alone) or public situation (e.g., researcher interview, family discussion). To record the studied intergroup domain, we coded the categorization basis of the target group (i.e., gender, race, ethnicity, nationality and immigration, sexual orientation, religion, body weight, attractiveness, age, disease, disability, social class, language, political affiliation, region, social network, and ad hoc groups created in a laboratory situation). We furthermore coded the relative group size (ingroup smaller, equal size, outgroup smaller) separately for the immediate (e.g., school or neighborhood composition) and for the general societal or national context. Similarly, the relative social status of the target group (higher, equal, lower than ingroup), the general societal evaluation of the target group (negative stigma, no stigma/neutral evaluation, positive stigma, ambivalent), and the identity relevance of group membership[3] were coded. When a study assessed attitudes toward several target groups simultaneously, we coded these variables for each target group separately. Because information regarding relative group size and group status in the immediate context was rarely available, we later dropped these variables from analyses.

Measures. Parent and child measures employed to assess intergroup attitudes were categorized according to the conceptual components of intergroup attitude: prejudice, stereotypes, and discrimination. Unfortunately, terminology in intergroup theorizing and research is not always without ambiguity, and these terms are not used uniformly in social psychology. For example, in some cases the term *prejudice* refers to evaluations and affective reactions toward social groups and their members, in other cases *prejudice* is used as an umbrella term, containing evaluations as well as specific beliefs and/or discriminative behaviors toward social groups and their members. For the scope of the current meta-analyses, we followed a strict social-cognitive conceptualization of intergroup attitudes, with affective and evaluative components as prejudice, semantic associations, beliefs, and attributions as stereotypes, and behavioral intentions and actual behavior as discrimination. Measures focusing on affective and/or evaluative responses were coded as affective. Exemplars are feeling thermometer scales (Monaci & Trentin, 2008), liking or pleasantness ratings (Vittrup & Holden, 2011), or good versus bad judgments in the doll test (Branch & Newcomb, 1986). Measures focusing on stereotype contents, beliefs, attributions, norms, or roles related to social group memberships were coded as cognitive. Exemplars are trait assignments (Acock & Bengston 1980), role expectations (e.g., Kulik, 2002), or activity lists (Meyer, 1980). Frequently, measures contained both cognitive and affective components and were thus coded as mixed cognitive–affective. For example, children were asked to assign stereotypic and counterstereotypic evaluative traits to group members (e.g., Castelli, Carraro, Tomelleri, & Amari, 2007). This category also contains a variety of measures that are usually termed prejudice scales while assessing beliefs or trait assignments in associations to social groups (e.g., I. Katz & Hass, 1988; Pettigrew & Meerten, 1995). Measures that contained behavioral self-reports or behavior observations (e.g., by researchers, teachers, or parents) or assessed intergroup contact or intergroup friendships were coded as behavioral. Exemplars are allocations of rewards (e.g., coins, gifts) to target individuals from different groups (e.g., Monteiro, de França, & Rodrigues, 2009) or intergroup contact measures (e.g., Ata, Bastian, & Lusher, 2009). Finally, measures that focused on behavioral intentions or envisaged or fictional behaviors were coded as behavior intentions (e.g., Mezei, 1971).

We furthermore coded the target focus of the measure as either on only outgroup, on only ingroup, or on an explicit ingroup–outgroup comparison. The directness of the attitude measurement was coded as direct measure (e.g., self-reports of intergroup attitudes) or indirect measure (e.g., deduced attitudes from observed playground behavior, response latency measures, "implicit" measures). Furthermore, each measure was coded according to its response format as continuous (e.g., Likert scales) or dichotomous (e.g., yes vs. no answers). The latter were further classified into

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[2] Originally, we had recorded year of publication in order to conduct moderation analyses testing whether earlier studies report higher or lower effect sizes. This analysis yielded a marginally significant moderation effect ($B = .003$, $SD = .001$, $p = .063$), pointing toward higher effect sizes in earlier studies. However, this relation was entirely driven by one outlying high effect size in a study from the 1950s and disappeared entirely after exclusion of this one effect size.

[3] Identity relevance of group membership was associated with a relatively low, though still acceptable, interrater agreement ($\kappa = .74$). Identity relevance turned out to be difficult to code because it was frequently not explicitly addressed in the publications and appeared to be difficult to code for intergroup contents unfamiliar to both coders (e.g., being non-Gypsy for Hungarians). This variable was therefore also excluded from further analysis.

forced-choice or free-choice formats. Finally, we recorded item number and reliability information for each measure if available. These measurement characteristics were recorded for all child and parent measures. For parent measures, we additionally coded the information source (i.e., self-report, child report, other report). Additionally, we coded the conceptual overlap for each child–parent measurement pair on a 5-point rating scale. The highest overlap score (5) was given when the identical measure was employed with parents and children. Conceptual overlap was coded as 4 either when two highly similar measures of the identical underlying concept were used (e.g., children's contact to Muslims and perceived parental approval of that contact; Ata et al., 2009) or when the same measure with age-appropriate reformulations of items were employed with parents and their children (e.g., the Attitudes Toward Women Scale, Spence, Helmreich, & Stapp, 1973; and the Attitudes Toward Women Scale for Adolescents, Galambos, Petersen, Richards, & Gitelson, 1985; see Shebloski & Gibbons, 1998). Codes 3 and 2 were given when parent and child measures targeted the same intergroup context but differed regarding the underlying components of intergroup attitudes. For example, an effect size focusing on two affective components of racial attitudes obtained with the MRA (Doyle & Aboud, 1995), which measures evaluative trait assignments in children, and the Intergroup Threat Scale (Stephan & Stephan, 1985; Stephan, Ybarra, & Bachman, 1999) with parents was coded as 3 (Sinclair, Dunn, & Lowery, 2005). An effect size focusing on behavioral and affective components of racial attitudes obtained asking children for playmate preferences via photographs of Black and White children and conducting a racial Implicit Association Test with parents was coded as 2 (e.g., Castelli, Zogmaister, & Tomelleri, 2009). Finally, the lowest score was given when child and parent measures had extremely small to no conceptual overlap. For example, in a study by Cossman (2004), the effect size represents the correlation between children's preferred social distance toward people with AIDS and parents' scores on two heterosexism items of the authoritarian *F* scale (Adorno et al., 1950).

Individual effect sizes. The effect size used in this meta-analysis was Pearson's correlation coefficient *r*, because it was the most frequent effect size reported in the majority of studies. Correlation coefficients were always coded such that a positive value indicates that a higher or lower level of parental intergroup prejudice was associated with a higher or lower level of child prejudice. For a small number of studies, values for *r* were not available. In these cases we calculated correlation coefficients from means, standard deviations, *t* values, *F* values, chi-square values, or proportions using the effect size determination programs by Wilson (Lipsey & Wilson, 2001; Wilson, 2001, 2011). Effect sizes from multiple regressions were not included because betas from regressions with more than one predictor cannot be directly interpreted as *r* values (the same applies for other multivariate methods of analyses, such as path models and structural equation models). When correlations were reported as nonsignificant without giving the specific value for *r* and no further information was available from the authors, the effect size coded for the meta-analysis was $r = 0$ ($n = 11$ effect sizes from $k = 6$ samples). Excluding these imprecisely reported nonsignificant results would have biased the meta-analysis toward the inclusion of significant results (that were always reported precisely) and would have led to

an overestimation of the mean effect size. Finally, the significance status associated with each effect size was recorded.

Effect Size Transformations

We followed the convention for product–moment correlations and transformed effect sizes to Fisher's *z* scale (Hedges & Olkin, 1985), with the mean effect size being $ES_{Zr} = .5 \log_e(1+r/1-r)$, its standard error being $SE_{Zr} = 1/\sqrt{n-3}$, its inverse variance weight $\omega_{Zr} = n-3$, and its 95% confidence intervals (CIs) being based on the standard error of the mean and a critical value from the *z* distribution: $\overline{ES}_{L/U} = \overline{ES} \pm Z_{1\pm\alpha}(SE_{\overline{ES}})$. For ease of interpretation, we report back-transformed standard correlation coefficients. Because measurement error in terms of restricted reliability of measures typically decreases the estimate of the average population correlation and increases the estimated variance of the correlations, we additionally corrected for such attenuations using the correction method proposed by Hunter and Schmidt (1990) before transforming effect sizes to Fisher's *z* scale. Correction for potential unreliability in this method is reached by dividing each observed correlation by the product of the square root of the reliabilities in the child and parent measures $r_{cor} = (r_{cp}/\sqrt{r_{cc}r_{pp}})$. Unfortunately, a number of reports did not contain any information about measurement reliability. Therefore, values for the child measurement were imputed for 121 effect sizes, and values for parent measures were imputed for 112 effect sizes following two approaches. First, we adopted average reliability values from other reports for measures that were repeatedly used in the data set. Accordingly, reliability values for the Attitudes Toward Women Scale (Spence & Helmreich, 1972) were imputed once for a child measure and five times for adult measures, and reliability values of the MRA (Doyle & Aboud, 1995) were imputed five times for child measures. After imputing these values, the average reported reliability of child measures was .783 (ranging from .43 to .96; $n = 116$), and mean reported reliability of parent measures was .793 (ranging from .34 to .95; $n = 112$). Second, we used these average values as reliability estimates for the remaining cases in which reliability information was missing, because reliability was not related to test length ($r = -.08$, *ns*, and $r = -.07$, *ns*, for parent and child measures, respectively). Note that this method can potentially lead to an overestimation of the population effect size. For example, in cases of very low measurement reliability, this correction can lead to an effect size above 1.00 (which was reset to 1.00) and should thus be interpreted with caution. To allow a comparison between corrected but potentially overestimated and uncorrected but potentially underestimated average effect sizes, we conducted all analyses twice and report results from analyses with reliability-corrected and uncorrected effect sizes, respectively.

Independence of effect sizes. Following the requirement of independence of effect sizes, we used the sample as the main unit of analysis, assuming that statistical dependencies of subsamples from the same study and studies from the same research team are likely to be small and negligible. Therefore we included the effect size for each sample or calculated the average effect size for samples of which several correlations were reported (e.g., based on more than one measure or more than one target group). Following this procedure results in a huge loss of information for moderation analyses. For example, when comparing child–parent correlation for mothers versus fathers, studies that measured both parents

would not be included because of violation of interdependence, and this moderator analysis would be restricted to studies that delivered effect sizes for only mothers or only fathers. We therefore applied a shifting units of analysis approach (Cooper, 1989) for all moderation analyses, in which each independent sample can contribute one (and only one) data point within a given aggregation. The aforementioned exemplar study would thus contribute the average of the father–child and the mother–child correlation for the calculation of the overall population effect size. For a moderation analysis testing whether parent gender is a moderator of the parent–child correlation, the study would contribute both the father–child and mother–child correlation independently. This approach offers a good compromise between minimizing violations of the assumptions of independence of effect sizes and retaining a maximum amount of information from each study.

Statistical Methods and Analytical Procedures

We analyzed the data using a random-effects approach (Hedges & Vevea, 1998) because it is preferable when cases are highly heterogeneous, or when effects are likely to be multidetermined (Lipsey & Wilson, 2001). This approach allows the assumption that underlying true effect sizes might vary between research contexts, and takes within-study error and between-study variation into account when calculating mean effect sizes. Recently, previous concerns about random-effects models overestimating the average effect size for correlations (Field, 2001, 2005b) have been demonstrated as unfounded (Hafdahl & Williams, 2009). Thus, random-effects analyses are suitable for the current meta-analysis. The average effect size was computed by weighting each sample effect size by its inverse variance weight ($n - 3$) with its 95% CI being based on the standard error of the mean and a critical value from the z distribution and a direct test of significance (above zero) being obtained by a z test. An added benefit of the random-effects approach is that it allows findings to generalize beyond the cases included in the analysis (see Hedges & Vevea, 1998). Random-effects models assume that a portion of the differences in effects across samples is essentially random and pertains to sources that one cannot identify (Lipsey & Wilson, 2001; Raudenbush, 1994). Correspondingly, relative to fixed-effects models, random-effects models provide relatively conservative tests, as they typically produce wider ranges for estimates of confidence limits and reduce the probability of achieving statistical significance (Hedges & Vevea, 1998).

For each analysis, the leverage statistics was used to identify outliers within that set of effect sizes. Detected outliers were Winsorized to their nearest effect size (Lipsey & Wilson, 2001). Each mean effect size analysis was accompanied by a homogeneity analysis involving the computation of the homogeneity statistics Q (Hedges & Olkin, 1985), which is distributed as a chi-square with $k = 1$. The Q statistic tests the null hypothesis that all the individual effect sizes estimate the same population effect size (Field & Gillett, 2010). Homogeneity of the effect size distribution was based on the Q statistic (Hedges & Olkin, 1985), which is distributed as a chi-square with $k - 1$. We performed moderator analyses using weighted regression analyses to assess the relationship between effect size and continuous and discrete moderator variables. Using the inverse variance weight ensures that the influence of each single effect size on the outcome of the analyses

is related to its sample size. For all analyses we used the macros provided by Field and Gillet (2010).

Results

Descriptive Data

The literature searches yielded 129 publications containing 131 studies with 177 independent samples (see Appendix for a detailed overview). In total, 47,036 parent–child pairs participated in these studies; single sample sizes ranged from 17 to 6,974 ($M = 266$, $Mdn = 131.5$, $SD = 579$). Children were aged between 3 and 21 years ($M = 13.3$, $SD = 5.0$). Most of the effect sizes involved gender attitudes ($k = 62$, 39.7%) and race-related or ethnicity-related attitudes ($k = 58$, 37.2%; see Moderator Analyses). The majority of the included samples are from Western industrialized countries ($k = 166$, 93.6%), with a strong dominance of U.S. ($k = 98$, 55.4%) and West European samples ($k = 54$, 30.4%). Non-Western samples were clearly underrepresented, with eight East European samples (4.9%) and three non-Western/non-European samples (1.8%) from Costa Rica, South Africa, and Turkey, respectively. Thus, when interpreting the results of this meta-analysis, we have to keep in mind that generalizability is restricted mainly to Western industrialized countries.

Mean Effect Size Analyses

We first computed the mean effect size based on the average effect size of each independent sample. Four of the 177 effect sizes for the mean analysis were outliers: three on the upper level ($r = .90$, $r = .76$, Gniewosz, Noack, Funke, & Wentura, 2003; and $r = .73$, R. Epstein & Komorita, 1966b) that were Winsorized to their nearest effect size of $r = .69$ and one on the lower level ($r = -.28$, Branch & Newcomb, 1986) that was Winsorized to its nearest effect size of $r = -.16$. The weighted population effect size was $r = .286$, 95% CI [.259, .312], which significantly differed from zero, with $z = 19.744$, $p < .001$. The weighted mean effect size based on reliability corrections was $r = .382$, 95% CI [.347, .416], $z = 19.592$, $p < .001$. Of the 177 average effect sizes, the vast majority of 166 (93.78%) were positive, two (1.13%) were null, and nine (5.08%) were negative. Stem and leaf displays of the average effect sizes for each sample are shown in Table 1. The data thus indicate a significant moderate positive relationship between parents' and their children's intergroup attitudes, which is considerably larger in size when controlling for low reliability in measures.

To estimate the influence of possible publication bias based on the file-drawer effect, we calculated Rosenthal's fail-safe N, which was 186.944. This implies that more than 180,000 unpublished studies with an underlying effect size of zero would need to exist to turn the significant population effect size estimate nonsignificant. The presence of a potential publication bias was further assessed with a funnel plot (see Figure 1). The approximate symmetry of the funnel plot around the population effect size indicates that small or nonsignificant correlations were not less likely to be included in the current meta-analysis. In fact, small effect sizes appeared to be equally likely to be reported by studies with small and big sample sizes. This symmetry indicates that a bias toward nonpublication of small, negative, or nonsignificant correlations is

Table 1

Stem and Leaf Display of 139 Independent Effect Sizes Based on Self-Reported Parent Measures and 50 Independent Effect Sizes Based on Child-Reported Parent Measures

Self-reported parent measures		Child-reported parent measures	
Stem	Leaf	Stem	Leaf
+1.0		+1.0	
+.9		+.9	0[a]
+.8		+.8	0[a]8[a]
+.7		+.7	336
+.6		+.6	0689
+.5	01114567	+.5	49
+.4	01111223778	+.4	2234568899
+.3	11122233334444555556666667777889	+.3	01225566789
+.2	00000001111122334445566677888888999	+.2	00134555578
+.1	001111233344455567777888999	+.1	
+.0	0011123344555567899	+.0	0399
−.0	23334	−.0	16
−.1	04556	−.1	
−.2	8[a]	−.2	
−.3		−.3	
−.4		−.4	
−.5		−.5	
−.6		−.6	
−.7		−.7	
−.8		−.8	
−.9		−.9	
−1.0		−1.0	

[a] Indexed outlying effect sizes were Winsorized to their nearest effect sizes for analyses.

unlikely. Thus, selection bias is not likely to have influenced the results, at least not to a strong degree.

Moderator Analyses

For all moderator analyses, we applied the aforementioned shifting units approach, which allowed us to make maximal use of all provided information by minimizing the threat of interdependence of effect sizes. Thus, for each moderator analysis, each sample could provide a maximum of one (averaged) effect size within each level of the moderator variable. Moderator variables were not interrelated unless explicitly stated below.

Source of parent attitude information. In a first step, we explored whether the magnitude of the effect size significantly differed depending on the source of information of parental attitudes (child reported vs. parent reported). Whereas $k = 139$ effect sizes were based on parents' self-reported attitudes, $k = 50$ effect sizes were based on child reports of parents' attitudes. Three references were excluded from this analysis because they reported four effect sizes that were based on other-reported parental attitudes. Table 1 depicts the stem and leaf plot separately for parent- and child-reported effect size.

Source of information of parent attitudes emerged as a significant moderator of effect size, $Q(1) = 36.026$, $p < .001$, with the residual heterogeneity being nonsignificant, $Q(187) = 193.097$, $p = .365$. The weighted population effect size based on self-reported parent attitudes was $r = .240$, 95% CI [.213, .267], which significantly differed from zero, with $z = 16.934$, $p < .001$ (corrected $r = .326$, 95% CI [.290, .361], $z = $

16.692, $p < .001$). The weighted population effect size based on child-reported parent attitudes was considerably higher, with $r = .414$, 95% CI [.343, .480], $z = 10.469$, $p < .001$ (corrected $r = .557$, 95% CI [.460, .641], $z = 9.356$, $p < .001$). These results indicate that children's intergroup attitudes are positively related to their parents' attitudes, whatever the source of information of parental attitudes. The magnitude of the correlation, however, depends on the source of information of parental attitudes and is higher when children report their parents' attitudes as compared to parents' self-reports. Given that the theoretical interest and background of the current meta-analysis is related to the effects that parental intergroup attitudes might have on the formation and socialization of children's attitudes, we restrict further moderator analyses to the self-reported parental attitudes only. However, interested readers can obtain further analyses of effects sizes based on child-reported parental attitudes upon request from the authors.

Conceptual overlap of measures of intergroup attitudes. Only a small number of effect sizes were based on parent and child measures with a low or very low conceptual overlap, whereas the majority of effects sizes were based on moderate to very high conceptual overlap. We conducted a weighted regression analysis using conceptual measurement overlap as continuous variable to predict the population effect size. This analysis yielded a significant moderation effect, $B = .094$, $SE = .005$, $\beta = .574$, $t(150) = 18.419$, $p < .001$. As can be seen in Table 2, the positive linear moderation effect indicates that congruent with our hypothesis, higher parent–child correspondences were found in studies using measures that were characterized by a higher conceptual overlap between measures.

Studied concept. In this moderation analysis, we tested whether the average effect size differs for affective, cognitive, and behavioral components of children's intergroup attitudes, thus comparing correlations of prejudice, stereotypes, or discriminative behaviors. We had added a mixed affective–cognitive category because a number of measures assess evaluations and beliefs simultaneously; thus outcomes represent a mix of prejudice and stereotypes. We performed this analysis twice, once focusing on the conceptual category of the child measure and once focusing on the conceptual measure of the parental measure. However, neither analysis yielded a significant moderator effect, with $Q(4) = 3.978$, $p = .264$; residual variation, $Q(146) = 141.468$, $p = .591$, for the conceptual category of the child measure and $Q(4) = 2.297$, $p = .681$; residual variation, $Q(149) = 155.465$, $p = .342$, for the conceptual category of the parent measure, respectively. It thus appears that parent–child correlations are similar in size for all components of intergroup attitudes, indicating that parents are no more or less influential on their children's prejudice, stereotypes, or intergroup behaviors.

Studied domain (target group). We further tested whether parent–child overlap differed in different attitudinal domains, but included only target groups for which at least three effect sizes were available (see Table 3 for the full information). This analysis yielded a significant moderator effect, $Q(7) = 18.673$, $p = .009$; residual variation, $Q(146) = 163.063$, $p = .158$. As can be seen in Table 3, effect sizes were significantly different from zero in all intergroup attitude domains; however, their magnitude varied
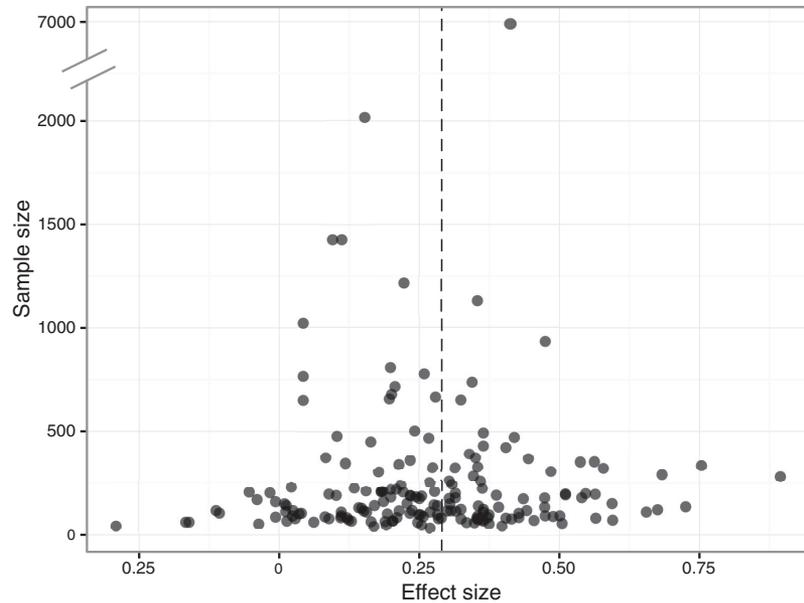
Figure 1.   Funnel plot of effect size against sample size.

$(.062 \leq rs \leq .347)$.[4] The most frequently researched domains, gender and race/ethnicity, were characterized by comparable effect sizes of parent–child correspondence. Table 3 further documents that parent–child correspondence in attitudes toward several target groups are slightly higher, such as for prejudice focusing on nationality and immigration, attitudes toward people of different sexual orientations (homosexuals, lesbians), and generalized outgroup attitudes. On the other hand, parent–child correspondence in attitudes toward people with disabilities or diseases (HIV, AIDS), religious outgroups and attitudes toward overweight individuals appeared to be less pronounced. Note, however, that the latter are based on low numbers of effect sizes $(3 \leq k \leq 15)$ and might therefore be less representative for the true population effect sizes.

Assessment situation.   We had coded whether data collection with children and parents took place in private versus public situations in order to obtain an indirect indicator of situational demands of social desirability. We first report the analyses of privacy of parent and child assessment separately and later the combined analyses. Information for coding the assessment situation was available for $k = 102$ effect sizes for parental self-reports and for $k = 108$ effect sizes for children's self-reports. Assessment situation for parental self-report was a significant moderator of the parent–child correlation, $Q(1) = 15.396$, $p < .001$; residual variation, $Q(100) = 108.592$, $p = .262$. The average population correlation in studies in which parents gave a public self-report $(k = 32)$ was $r = .181$, 95% CI [.144, .219], $z = 9.254$, $p < .001$ (corrected $r = .308$, 95% CI [.205, .404], $z = 5.658$, $p < .001$) and thereby significantly smaller than the average effect size based on private parental self-reports $(k = 70)$, with $r = .297$, 95% CI [.261, .332], $z = 15.259$, $p < .001$ (corrected $r = .375$, 95% CI [.334, .414], $z = 16.545$, $p < .001$). It thus appears that studies in which parents are asked to publicly express their intergroup attitudes (mostly in front of a researcher) might underestimate the true effect size of parent–child correspondence in intergroup attitudes.

In a further step we tested whether the situation of the child assessment was a comparable moderator of the parent–child correspondence in intergroup attitudes. Indeed, a comparable effect was found related to the assessment situation for children's intergroup attitudes, $Q(1) = 25.099$, $p < .001$; residual variation, $Q(106) = 110.601$, $p = .360$. When expressed in public, children's attitudes appeared less similar to parental attitudes, $r = .171$, $k = 53$, 95% CI [.140, .202], $z = 10.584$, $p < .001$ (corrected $r = .258$, 95% CI [.184, .330], $z = 6.631$, $p < .001$), as compared to attitudes expressed in private situations, $r = .305$, $k = 55$, 95% CI [.264, .346], $z = 13.715$, $p < .001$ (corrected $r = .388$, 95% CI [.341, .432], $z = 15.069$, $p < .001$).

Finally, we tested for the combined moderation effect of assessment situation for children and parents, which again proved to be a significant moderator of the population effect size, $Q(3) = 36.538$, $p < .001$, $k = 95$; residual variation, $Q(91) = 90.525$, $p = .494$. Lowest child–parent correspondences were found when the assessment situation differed between children and parent (see Table 4). When the assessment situation was public for both parents and children, a somewhat higher correspondence was found. The highest child–parent correspondence, however, was found when the assessment situation was private for both parents and children. It thus appears that public investigation of intergroup attitudes leads to a possible underestimation of the relation between parental attitudes and child attitudes, presumably because social desirability norms become more salient in such situations

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[4] Results remained fundamentally equivalent after we excluded the 10 samples that provided more than one effect size assessing attitudes toward different target groups within the same sample, thereby minimizing violation of the assumption of effect size independence. With the reduced number of cases, the moderation effect remained significant, $Q(6) = 16.223$, $p = .013$, $k = 115$, with a remaining residual variation of $Q(108) = 126.030$, $p = .113$.

Table 2

Weighted Means of Reliability-Corrected and Uncorrected Effect Sizes, Along With Sample Sizes, Confidence Intervals, Significance Tests, and Fail-Safe ks for Each Level of Conceptual Overlap Between Parent and Child Measures

Overlap	k	N	r	95% CI	z	p	Fail-safe k	$r_{corrected}$	95% CI
1	2	429	.167	[−.018, .340]	1.770	.077	5	.219	[−.020, .435]
2	9	1,815	.054	[.007, .100]	2.276	.023	7	.075	[.028, .128]
3	36	6,056	.132	[.094, .171]	6.715	.000	875	.169	[.119, .217]
4	29	8,092	.196	[.149, .243]	7.973	.000	2,211	.263	[.201, .323]
5	77	27,759	.293	[.263, .323]	18.128	.000	50,520	.405	[.363, .445]

Note. CI = confidence interval.

and prevent participants from freely expressing their personal attitudes.

Child age. To explore whether child age was a significant moderator of parent–child correspondence in intergroup attitudes, we conducted a weighted regression analysis using child mean age as continuous variable to predict the population effect size. We than conducted a mediation analysis to explore whether effects of child age are mediated by measurement overlap. Results of the weighted regression analysis demonstrated a significant positive linear relation between child age and parent–child correspondence, $B = .016$, $SE = .003$, $\beta = .377$, $t(134) = 4.714$, $p < .001$, indicating that parent–child correspondence increases with children's age. As depicted in Table 5, the average parent–child correlation was significantly above zero in all age groups. Its amplitude, however, increased with increasing child age.[5]

When interpreting the moderation effect of child age, it is important to keep in mind that child age was highly correlated with the conceptual overlap between child and parent measures, $r(146) = .622$, $p < .001$. This high correlation is not particularly surprising, as researchers working with younger children might see a higher need to use child-adapted measures that are inevitably less similar to parental measures, whereas studies with older children or adolescents are more likely to employ the same measures with parents and children. However, this high correlation between measurement overlap and child age indicates the likelihood that the two variables might mediate each other's moderation effect on the mean population effect size: The positive relation between measurement overlap and parent–child correspondence might be mediated by child age or, reversed, the relation between child age and parent–child correspondence might be mediated by measurement overlap. We therefore repeated the two previous weighted regression analyses reported above including child age and measurement overlap as second predictor, respectively. Results document that the relation between measurement overlap and child–parent correspondence was not mediated by children's age: The predictive value of measurement overlap on the population effect size was not significantly reduced when controlling for child age, $B = .078$, $SE = .015$, $\beta = .465$, $t(145) = 5.194$, $p < .001$, whereas child age had no significant additional predictive value. A Sobel test confirmed that child age did not mediate the effect of measurement overlap ($z = 1.240$, $p = .215$). The reversed model, however, documented a significant mediation effect of measurement overlap on the relationship between child age and the population effect size, in which the predictive value of child age became nonsignificant, $B = .005$, $SE = .004$, $\beta = .104$, $t(145) = 1.162$, $p = .247$. A Sobel test confirmed that measurement overlap significantly

mediated the relationship between child age and parent–child similarity ($z = 4.572$, $p < .001$). It thus appears that the studies in our meta analysis find lower parent–child correspondence in younger children because of a lack in conceptual overlap between the measures used with parents and children, whereas measures used in studies with older children and adolescents employ measures with parents and children that are conceptually more similar. Figure 2 depicts this mediated moderation, sorting measures as low, medium, or high in conceptual overlap.

Gender. We first report separate moderation analyses for child gender and parent gender, respectively. The report of the moderation analysis of parent–child gender match follows.

Child gender. We conducted a moderation analysis to explore whether girls' and boys' attitudes do correspond with their parent attitudes to a different or same degree. This analysis revealed that child gender had no significant moderating effect on parent–child correlations, $Q(1) = 0.246$, $p = .620$. The average population effect size for boy samples was $r = .229$, 95% CI [.153, .302], $k = 22$, $z = 5.803$, $p < .001$ (corrected $r = .334$, 95% CI [.235, .426], $z = 6.320$, $p < .001$), and the average population effect size for girl samples was $r = .253$, 95% CI [.194, .309], $k = 34$, $z = 8.323$, $p < .001$ (corrected $r = .385$, 95% CI [.272, .487], $z = 6.270$, $p < .001$). Effect sizes of girls-only and boys-only samples are thus basically equal and also do not differ significantly from effect sizes in mixed samples, $r = .241$, 95% CI [.210, .271], $k = 83$, $z = 14.715$, $p < .001$ (corrected $r = .304$, 95% CI [.269, .338], $z = 16.309$, $p < .001$). It thus appears that girls are neither more nor less similar in their expression of intergroup attitudes to their parents than boys are.

Parent gender. Out of the 166 publications with self-reported parent measures that specified the gender of the parent, 117 collected data from fathers and mothers of each participating child, 20 collected data from either mother or father, 27 collected only mother data, and two collected only father data—thus providing 166 effect sizes for the moderation analysis. Replicating results for child gender, parent gender had no significant moderation effect on parent–child correlations, $Q(1) = 0.001$, $p = .970$.[6] The mean population effect for child–mother pairs was $r = .244$, 95% CI [.211, .276], $k = 96$, $z = 14.107$, $p < .001$ (corrected $r = .399$, 95% CI [.286, .440], $z = 8.448$, $p < .001$), not differing from the

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[5] No such moderation was found for the child-reported parental attitudes, $B = −.006$, $SE = .012$, $\beta = −.069$, $t(48) = −0.479$, $p = .634$.

[6] Conducting this analysis with independent effect sizes only (thus excluding samples that contained mother and father data) replicated this result, with $Q(1) = 0.296$, $p = .586$.

Table 3

Weighted Means of Reliability-Corrected and Uncorrected Effect Sizes, Along With Sample Sizes, Confidence Intervals, Significance Tests, and Fail-Safe ks for Each Target Group

Target group	k	N	r	95% CI	z	p	Fail-safe k	$r_{corrected}$	95% CI
Gender	62	13,662	.214$_a$	[.183, .245]	13.143	.000	10,285	.327	[.262, .389]
Race/ethnicity	58	15,994	.245$_a$	[.198, .291]	9.859	.000	15,422	.308	[.255, .359]
Nationality/immigration	15	5,660	.307$_b$	[.262, .351]	12.560	.000	2,700	.396	[.335, .453]
Body weight	5	1,720	.062$_d$	[.015, .109]	2.566	.010	10	.087	[.040, .134]
Generalized/unspecified	4	1,394	.347$_b$	[.249, .438]	6.613	.000	184	.432	[.322, .531]
Sexual orientation	4	1,264	.293$_b$	[.197, .384]	5.785	.000	140	.413	[.240, .561]
Religion	3	242	.153$_c$	[.026, .276]	2.358	.018	3	.195	[.069, .315]
Disease/disability	3	2,439	.152$_c$	[.112, .190]	7.526	.000	39	.205	[.166, .243]
Social class	2	1,077	.270$_b$	[.086, .437]	2.844	.004	54	.346	[.105, .549]

Note. Different subscript letters indicate significant differences of effect sizes between different target groups. CI = confidence interval.

mean effect of child–father correlations, $r = .243$, 95% CI [.205, .281], $k = 70$, $z = 12.036$, $p < .001$ (corrected $r = .337$, 95% CI [.285, .387], $z = 11.928$, $p < .001$). Thus, in general, children are not more similar or dissimilar to their mothers than they are to their fathers in their expression of intergroup attitudes.

Parent–child gender match. The previous analyses have shown that neither parent gender nor child gender had significant main effects on parent–child attitude correspondence. These results, however, do not rule out an interaction effect of child and parent gender, for example, such that parent–child correspondence would be higher when child and parent gender match than if they do not match. We therefore created a moderator variable coding whether child gender was the same as or different from parent gender. For this analysis, 86 effect sizes were available. The effect of gender match was not significant, $Q(1) = 1.489$, $p = .222$; residual $Q(90) = 92.954$, $p = .395$, although there was a numerical trend toward higher parent–child correspondence for gender matching pairs: girls–mothers, $r = .274$, 95% CI [.209, .337], $k = 32$, $z = 8.012$, $p < .001$ (corrected $r = .483$, 95% CI [.272, .651], $z = 4.153$, $p < .001$); boys–fathers, $r = .280$, 95% CI [.177, .376], $k = 20$, $z = 5.203$, $p < .001$ (corrected $r = .401$, 95% CI [.270, .518], $z = 5.633$, $p < .001$); as compared to nonmatching pairs: girls–fathers, $r = .230$, 95% CI [.143, .313], $k = 21$, $z = 5.078$, $p < .001$ (corrected $r = .345$, 95% CI [.216, .461], $z = 5.044$, $p < .001$); boys–mothers, $r = .224$, 95% CI [.138, .306], $k = 19$, $z = 4.982$, $p < .001$ (corrected $r = .326$, 95% CI [.216, .427], $z = 5.607$, $p < .001$). These results are in line with findings of a meta-analysis by Tenenbaum and Leaper (2002), in which gender match was also found to be insignificant for the size

of the correlation between parents' and children's gender schemata.

Group status. To analyze whether group status moderates the effect size, two possible moderator variables were available from our coding: relative group size—the relative size of the ingroup in relation to the target group (smaller, same size, bigger)—and relative group status, coded as the relative status of the ingroup in relation to the target group (lower, same status, higher). Both variables had been coded for the general societal context as well as for the immediate child environment (e.g., school, neighborhood). However, because information for coding the immediate environment was more often unavailable than available, the present analyses are based on societal status and relative group size only. Unsurprisingly, both variables were highly correlated ($r = .819$). We nevertheless decided to run moderation analyses with both variables, mainly because we could not code both variables univocally for all entries. For example, with regard to gender attitudes, ingroup size was always coded as equal to outgroup size. Ingroup status on the other hand was coded as higher in male samples and lower in female samples. Only in mixed-gender samples, ingroup status could not be coded univocally, as it differs for male (higher) and female (lower) participants.

Information allowing to code group size was available for $k = 136$ samples. Ingroup size turned out to be a significant moderator of the parent–child correlation, $Q(2) = 7.441$, $p = .024$; residual variation, $Q(133) = 136.044$, $p = .411$. When the ingroup was smaller than the outgroup ($k = 11$), the mean correlation was $r = .130$, 95% CI [.040, .219], $z = 2.821$, $p = .005$ (corrected $r = .167$, 95% CI [.050, .280], $z = 2.776$, $p = .006$). For groups of

Table 4

Weighted Means of Reliability-Corrected and Uncorrected Effect Sizes, Along With Sample Sizes, Confidence Intervals, Significance Tests, and Fail-Safe ks for Each Level of Privacy of the Assessment Situation for Parents and Children

Assessment									
Parent	Child	k	N	r	95% CI	z	p	Fail-safe k	$r_{corrected}$	95% CI
Public	Public	29	9,055	.185	[.144, .225]	8.690	.000	2,176	.319	[.208, .422]
Public	Private	17	2,269	.160	[.098, .220]	5.056	.000	288	.200	[.120, .277]
Private	Public	4	575	.143	[.061, .223]	3.414	.001	12	.188	[.107, .266]
Private	Private	45	16,888	.340	[.297, .381]	14.622	.000	21,631	.428	[.380, .473]

Note. CI = confidence interval.

Table 5

Weighted Means of Reliability-Corrected and Uncorrected Effect Sizes, Along With Sample Sizes, Confidence Intervals, Significance Tests, and Fail-Safe ks for Each Age Group

Age group (years)	k	N	r	95% CI	z	p	Fail-safe k	$r_{corrected}$	95% CI
4–6.5	22	2,930	.130	[.071, .188]	4.273	.001	165	.178	[.096, .257]
6.5–9.5	18	2,223	.183	[.082, .280]	3.515	.000	485	.225	[.099, .343]
9.5–12.5	12	2,756	.199	[.113, .281]	4.502	.000	324	.252	[.150, .350]
12.5–15.5	29	6,151	.256	[.197, .313]	8.232	.000	3,546	.329	[.256, .398]
15.5–18.5	36	20,633	.275	[.230, .319]	11.527	.000	15,478	.351	[.304, .396]
18.5–21.5	19	3,488	.304	[.255, .352]	11.513	.000	1,899	.530	[.381, .651]

Note. CI = confidence interval.

equal size (k = 62), the mean correlation was r = .214, 95% CI [.183, .245], z = 13.143, p < .001 (corrected r = .327, 95% CI [.262, .389], z = 9.372, p < .001). In cases in which the ingroup was bigger than the target group (k = 63), the mean correlation was r = .261, 95% CI [.220, .302], z = 12.010, p < .001 (corrected r = .332, 95% CI [.287, .377], z = 13.381, p < .001).

Information to code group status was available for k = 84 effect sizes (the lower number of sample being mainly explained by the impossibility to code ingroup status in mixed-gender samples in studies on gender attitudes). Repeating the moderation analysis with the variable ingroup status basically replicated aforementioned results: Group status significantly moderated parent–child correlations, Q(2) = 6.973, p = .031; residual variation, Q(81) = 82.743, p = .425. Small effect sizes were found in groups of lower status as compared to the target group (k = 18), with a mean effect size of r = .144, 95% CI [.083, .204], z = 4.571, p < .001 (corrected r = .192, 95% CI [.113, .268], z = 4.727, p < .001). Only two effect sizes were found from groups with equal status, with an average effect size of r = .325, 95% CI [.191, .448], z = 4.592, p < .001 (corrected r = .470, 95% CI [.273, .630], z = 4.338, p < .001). Finally, parents' and children's attitudes from higher status groups (k = 64) also showed a higher parent–child correspondence, r = .267, 95% CI [.225, .307], z = 12.213, p <

.001 (corrected r = .339, 95% CI [.293, .383], z = 13.550, p < .001). In light of these results, we conclude that in minority/low-status groups, parent–child similarity rather tends to be smaller than in majority and/or high-status groups or groups of equal size and/or status—which is contrary to our hypotheses.

Discussion

The main aim of the current meta-analysis was to quantitatively synthesize available research to find out whether there is a substantial relationship between parent and child intergroup attitudes. Our first step was to determine the average effect size of the correlation between parents' and their children's levels of intergroup prejudice and to assess its significance. Our second objective was to test moderators of the parent–child relationship to determine whether variability in domains, methods, study, or sample characteristics account for some of the variation in the correlations. We will first give a summary and discussion of the results of this meta-analysis and then turn to a more thorough discussion of the implications of the findings for our understanding of parental socialization of intergroup prejudice in children.
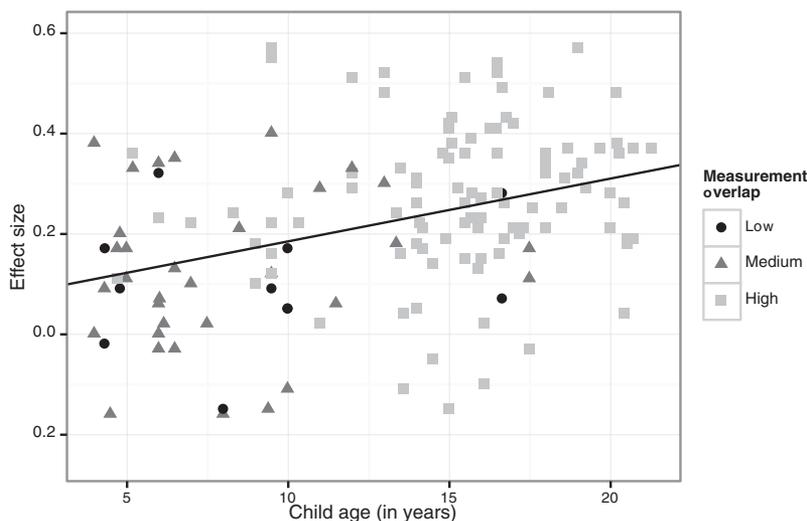

Figure 2. Scatterplot depicting the correlation between child age and average effect size mediated by the conceptual overlap of parent and child measures.

Overall Effect Size

This meta-analysis, encompassing 131 studies with a total sample of over 45,000 parent–child dyads, unequivocally demonstrates that parent–child intergroup attitudes are related throughout childhood, adolescence, and young adulthood. Our meta-analysis revealed a significant positive relationship with an overall mean effect size of .29. Using a disattenuated effect size to control for low reliability of measures, we observed a considerably elevated average effect size of .38, implying a moderate effect. The general argumentation that parental attitudes are irrelevant for prejudice formation in children because of an absence of a parent–child correlation can thus no longer be maintained.

Moderator Analyses

There was a considerable degree of heterogeneity among effect sizes, supporting the premise that the magnitude of observed effect sizes might be related to various methodological, sample-specific, and study-specific moderating variables. To recapitulate, our results indicate that parent–child similarity appeared to be generally higher in studies relying on child reports of parental attitudes as compared to parental self-reports. Summarizing the results of the further moderation analyses of studies including parental self-reports only, we observed higher effect sizes when studies used identical or conceptually highly overlapping measures in children and parents, when the assessment situation was private, and in samples with older children and/or children belonging to higher status majority groups. On the contrary, lower effect sizes were reported in studies with younger children, in lower status minority groups, when attitude measurements were conceptually dissimilar, and/or when attitudes were measured in public assessment situations. Also, the degree of parent–child similarity unsystematically differed across domains and target groups. Finally, we can conclude that neither parent nor child gender, nor parent–child gender match, was related to differences in effect sizes, and effect sizes were also comparable across affective, cognitive, and behavioral components of intergroup attitudes. Below we discuss each result of these moderation analyses in detail.

Source for parental attitudes. We had hypothesized that parent–child correlations would be higher if they rely on child reports of parental attitudes as compared to parental self-reports. This hypothesis was supported. Children thus appear to perceive their parents' attitudes as considerably more similar to their own attitudes than they are when parents report their attitudes themselves. This result can be interpreted in terms of egocentric false-consensus beliefs and projection processes (e.g., Aboud & Doyle, 1996; Gniewosz & Noack, 2006). Children might simply not know their parents' attitudes very well, either because they are rarely openly expressed or discussed or because general cognitive limitations cause children not to know, misperceive, and/or misinterpret their parents' attitudes in light of their own attitudes.

However, there is an alternative explanation that does not rely on the assumption that children's perceptions of their parents' attitudes are inaccurate projections anchored in their own attitudes. On the contrary, they might reflect accurate behavioral observations, which, however, differ from the parents' self reported attitudes. Thus, children may know little about how their parents respond to a prejudice questionnaire, but they may observe how their parents behave in direct or indirect interactions with members

of different groups and infer and even internalize parents' attitudes (see Bigler & Liben, 2006). This possibility gains credence when we remember that parental behaviors, like all adult behavior in this domain, are not necessarily consistent with their self-reported attitudes (e.g., Ajzen & Fishbein, 1977; Ajzen & Madden, 1986; Eagly & Chaiken, 1993, 1998; Fazio, 1990; M. Fishbein & Ajzen, 1975; LaPiere, 1934; Wicker, 1969). When reporting their attitudes, concerns of impression management and social desirability might motivate parents to varying degrees to conceal their true attitudes, thus deflating parent–child correlations. The finding that the privacy of the assessment situation was a moderator of parent–child correlations supports this assumption. A related possibility is that different attitude components guide parents' intergroup behaviors and their responses to questionnaires on intergroup attitudes. Whereas the responses to a self-report measure may be based to a stronger degree on deliberate considerations (i.e., so-called explicit attitudes), actual behaviors may be more influenced by spontaneous activations of evaluative or stereotypical associations (so-called implicit attitudes) that are often dissociated from self-reports (e.g., Dovido, Kawakami, & Beach, 2001; Fazio, Jackson, Dunton, & Williams, 1995). The little evidence currently available is at least broadly in support of this contention: Young children rely more on adults' nonverbal behavioral cues rather than verbal expressions of like or dislike when forming attitudes about outgroup members (Castelli, De Dea, & Nesdale, 2008), and higher correlations are found between pre- and middle school children's prejudice expression and their mothers' implicit as opposed to explicit prejudice (Castelli et al., 2009; Sinclair et al., 2005). Thus, although further research is needed, it is too early to conclude that the larger parent–child correlation observed when children report both their own and their parents' attitudes is solely a matter of egocentric projection or other form of systematic error.

Measurement overlap. We had expected that parent–child correlations would increase with increasing conceptual overlap between parent and child measures. This hypothesis was supported, as conceptual measurement overlap turned out to be a significant continuous moderating variable. Higher parent–child correlations were found when both measures were highly similar or identical, whereas correlations decreased with decreasing similarity. Further analyses established that this relation was not mediated by children's age. This result indicates that conceptual similarity of measures is a crucial factor for finding significant parent–child correlations in intergroup attitudes and that researchers risk underestimating it when they use measures that do not tap the same underlying concepts. Researchers thus face the challenge of developing measures of intergroup attitudes that are adapted to the specific cognitive, social, and linguistic competencies of the developing child but still assess the same underlying concepts as in adults. This is further impeded by our current lack of a clear understanding of whether, when, and under which circumstances intergroup attitudes actually do occur in equivalent forms in childhood and adulthood. This result not only demonstrates that differences in methods can sometimes be systematically related to important conceptual and theoretical aspects. It also highlights the importance and the need of clear definitions and operationalizations of intergroup attitudes to ensure validity of our research. We can only highlight the need for a more systematic development of measures that, while being adapted to different age groups, tap the same underlying constructs of intergroup attitudes. For example,

Liben and Bigler (2002) presented a suite of scales for the assessment of gender attitudes for use in middle childhood up to young adulthood. Similarly, Galambos et al. (1985) developed an adolescent version of the Attitudes Toward Women Scale (Spence & Helmreich, 1972), the Attitudes Toward Women Scale for Adolescents.

Studied concept. Overall, the exploratory analysis showed no significant moderation effect of studied concept, indicating that parent–child similarity is comparable across different aspects of intergroup attitudes. The available data thus do not support assumptions that children acquire specific components of intergroup attitudes better or more directly from parents than others, possibly indicating rather similar socialization mechanisms.

Studied domain (target group). The exploratory moderation analysis of intergroup domain documented significant differences in parent–child similarity between domains, indicating that overlap was highest for generalized prejudice and national or immigration-related attitudes as compared to gender, race, and ethnicity. These results should, however, be interpreted with caution because of the low number of effect sizes that contributed to any target domain besides gender, race, and ethnicity. The average parent–child correlations in gender attitudes and racial attitudes represent a high variety of studies and thus quite heterogeneous data. Given the small numbers of available effect sizes, the average correlations in other domains might be biased by confounding variables. For example, the studies focusing on immigration and national attitudes were almost entirely conducted with adolescents and young adults and are characterized by high levels of conceptual overlap between parental and child measures. It is unlikely that topics related to immigration, national attitudes, or generalized prejudice are more openly or frequently addressed and discussed within the family as compared to attitudes related to gender or race issues, leading to higher parental influence on children's attitude formation. One may, however, assume that these attitudes, especially generalized prejudice, reflect broader value orientations that also tend to be more closely related to right-wing authoritarianism and social dominance orientation (e.g., Ekehammar, Akrami, Gylje, & Zakrisson, 2004). Thus, although children might be less influenced by parents' specific attitudes toward specific social groups, they nevertheless adopt a similar ideology and similar overall attitudes toward outgroups in general. Clearly, more systematic research directly comparing attitudes in different intergroup domains is needed to come to a conclusion (see O'Bryan, Fishbein, & Ritchey, 2004).

Assessment situation. We had hypothesized that the privacy versus publicity of the assessment situation would moderate parent–child similarity. These expectations were confirmed in that parent–child similarity was indeed significantly higher when parents' attitudes were assessed in private settings as compared to public expressions. A comparable moderation effect was also found for the privacy of assessment of children's prejudice. Eventually, the highest average effect size occurred in studies in which both parents and children responded in private as compared to studies with mixed assessment situations or studies with public assessments of both parent and child attitudes. These results can be explained assuming that the open expressions of intergroup attitudes might be distorted in both adults and children, either because of self-impression management or social desirability concerns (e.g., Gaertner & Dovido, 1986; Rutland et al., 2005) or because of

other possible experimenter effects or situational demand effects (e.g., Orne, 1969; Rosenthal, 1966). The finding that effect sizes remain small when both parents and children express their attitudes publicly suggests that different biases play a role in parents and children. If parents and children construe social norms or situational demands differently, their attitude expressions might be biased in different directions, thus limiting the possibility of finding significant parent–child correlations. Clearly, we need further research to fully understand what these biases and distortions are, how they affect responding, and how they may affect socialization processes.

Child age. We had inferred conflicting predictions from theories of prejudice formation regarding the possible role that children's age might play in moderating child–parent similarity. Allport's (1954) assumptions about the developing child contain the prediction of a negative relation between child age and parent–child similarity. Although both SCDT (Aboud, 1988) and SDIT (Nesdale, 1999, 2004) explicitly state that parent–child similarity was not to be expected, they each contain remarks suggesting possible age effects. According to SCDT, one would expect a switch around the age of 7–8 years, with no parent–child similarity below that age (because prejudice is generated solely by endogenous cognitive factors) and (if any) low similarity above that age as broader social concerns are assimilated. According to SDIT, an opposite pattern would be expected; with some parent–child similarity in younger ages because young children might imitate parents' attitudes (without adopting them as their own), but no consistent similarity in later childhood and adolescence because of higher socialization influences by various peer groups. Last but not least, neither DIT (Bigler & Liben, 2006) nor SSCMT (Barrett, 2007) offers clear predictions regarding the effect of children's age.

Unfortunately, the results of our analyses do not provide much help in clarifying this issue. Although we do find a significant moderation effect of child age, indicating increasing parent–child similarity with increasing child age (and thus supporting SCDT's assumptions on first glance), this effect was mediated by the conceptual overlap between measures. Thus, at the moment it is impossible to know whether the lower correlation between young children's and their parents' prejudice is indeed related to children's young age and developmental status or merely based on the low conceptual similarity of measures in most of the studies including young children. These results demand future research with a wide age range and improved measures that tap the same underlying concepts in both parents and their children. Only when more studies become available that employ conceptually comparable parent and child measures, especially in young children samples, can we draw firm conclusions on early parent–child similarity and socialization.

The results of our moderation analysis do, however, contradict assumptions that intergroup attitudes in adolescents are clearly distinguished from parental attitudes, thus challenging the view of adolescents active detachment efforts regarding their values and attitudes (e.g., Jaspers, Lubbers, & de Vries, 2008; P. A. Katz, 1976; McGue, Elkins, Walden, & Iacono, 2005). The available data clearly demonstrate that adolescents and young adults demonstrate a considerable similarity to their parents' intergroup attitudes—despite the assumption that peer influences might gain higher importance during adolescence (e.g., Nesdale, 1999, 2001a,

2004). Of course, our data do not allow a direct comparison of adolescents' similarity to their parents with the similarity to their peers. Further research is needed that directly compares parental and peer influences, taking into account that they might not necessarily be independent or contradictory and that attitude formation might be based on the interaction and possible additive or multiplicative effects of parental and peer attitudes.

A further open question remains regarding the temporal conditions of the found parent–child similarity in adolescence and young adulthood. Given that the current analyses are almost entirely based on cross-sectional data, it is impossible to know whether parent–child similarity in adolescence is a prolonged result of stable protracting socialization processes during earlier childhood or whether qualitatively different socialization mechanisms come into play during adolescence, leading to a sudden emergence of parent–adolescent similarity. Clearly, future research needs longitudinal designs in order to develop a clearer picture of these processes.

Parent and child gender. We had hypothesized a possible difference in children's similarity to mothers as compared to fathers based on the typically higher involvement of mothers in child care. We further conducted exploratory analyses to explore whether child gender was significantly related to parental similarity (i.e., girls being more similar to their parents than boys, or vice versa) and whether similarity of intergroup attitudes was higher in gender-matching parent–child dyads (i.e., mothers–daughters, fathers–sons) than nonmatching dyads (i.e., mothers–sons, fathers–daughters). Results of our analyses indicate that children are no more similar to their mothers than fathers; nor did we find any indication that boys or girls differed in their similarity to their parents. The interaction analysis also did not show significant effects of gender match, although a numerical trend was observed, indicating that parent–child similarity was numerically slightly higher in gender-matched dyads as compared to nonmatching dyads. These results confirm earlier findings in a meta-analysis on gender schema including gender role attitudes (Tenenbaum & Leaper, 2002) in which no effects of parent or child gender were found. It thus seems that mothers are not more influential than fathers—despite the assumed difference in involvement in child care—and girls are not more or less susceptible to parental influence than boys. One might explain these findings with the aforementioned low likelihood that intergroup topics such as prejudice, stereotypes, or intergroup interactions are actually openly expressed or discussed within the family—at least in majority families. If intergroup attitudes are no topic of communication, the amount of time spent together or the degree of involvement in child care would not matter. An alternative possibility is that the degree of identification with the parent matters (e.g., Sinclair et al., 2005), but given the assumption that children might generally be more attached and identified with their same-gender parent (e.g., Diener, Isabella, Behunin, & Wong, 2008), our data do not offer much support of this contention either. However, the analysis of the role of gender match comes with the caveat of relatively low case numbers and might thus simply lack test power. It thus might be premature to conclude that gender match does or does not play any role for the socialization of intergroup attitudes, especially in relation to different patterns of child–parent identification.

Ingroup status. We formulated the hypothesis that parent–child similarity might depend on the minority versus majority status of the ingroup. In particular, we expected that parent–child similarity would be higher in lower status minority groups as compared no higher status majority groups, based on findings of more intense expression and discussion of intergroup topics in minority as compared to majority groups (e.g., Hughes et al., 2006). This expectation was not confirmed in our analyses, and indeed an opposite pattern of results occurred: Children in higher status majority groups tended to be more similar to their parents than children in low-status minority groups. On the basis of these results, it appears unlikely that a high openness of expression or a higher frequency with which intergroup attitudes are discussed leads to higher parent–child similarity, although it does not represent a direct test of this assumption. However, one might also speculate that minority children who receive lots of open parental discussion also have many other additional sources of influence, thus reducing the correlation with parent attitudes. For example, minority children tend to have more frequent outgroup contact, providing them with more direct experiences as a basis of attitude formation. Thus, open parental expression of attitudes might be one of the few sources for minority children but not majority children. Also, majority children may be less likely to observe their parents' actual intergroup interactions when living in homogeneous communities, which could also explain the found group difference. One might further assume that specific cultural differences might play a role here. Most of the samples coded as low-status minority groups were African American, and one might come up with group-specific explanations related to parenting styles that could explain the lower parent–child similarity. Unfortunately, our analyses suffer from rather small sample sizes, as we had found less than 20 samples that were coded as belonging to minority and/or low-status groups. In many cases, studies from the rich racial socialization literature could not be included in our meta-analysis because they did not report measures of intergroup attitudes, focusing instead on explicit parental teaching, assessing frequency and content of statements on cultural heritage and practices, or the experience of discrimination (Hughes et al., 2006). The representativeness of the few included minority and/or low-status samples can thus be doubted. Further research is needed to explore whether the group differences we report here can be replicated and whether they can indeed be explained with different styles of parental teaching or other forms of socialization. It would be desirable if future research would focus more on socialization processes, including observational studies that permit investigators to see the mechanisms by which attitudes and beliefs actually get transmitted.

Limitations of the Current Research

As mentioned earlier, the third goal of the current article was to critically discuss whether and to what extent parent–child similarity can be interpreted as indicating parent–child socialization. During our literature research, we found that the majority of publications on the topic were based on simple bivariate correlations assessed in cross-sectional designs, which is the reason why we used the correlation coefficient r as effect size for this meta-analysis. Whereas the absence of a significant correlation between parent and child prejudice has often been used as argument for the absence of parental socialization of intergroup prejudice (e.g., Aboud, 1988, 2008), its presence cannot be interpreted as straight-

forward evidence of the presence of parent–child socialization. Below we discuss first whether correlations are the adequate measure to establish parent–child similarity. In particular, we discuss whether established parent–child similarities can be interpreted as indicators of unidirectional parent–child socialization effects given the possibility of (a) possible reciprocal effects of child–parent socialization, (b) possible socialization effects of shared environmental influences, or (c) as artifacts of shared genetic factors.

The correlation coefficient is not necessarily a measure of similarity but a measure of shared variance, in this case between two groups of individuals, parents and their children. Thus, even finding a high correlation does not imply that children and their parents express similar intergroup attitudes, as the magnitude of their actual attitude scores might differ markedly. From a (positive) correlation coefficient, we simply learn that children who score higher (lower) in their prejudice levels than other children have parents who tend to score higher (lower) in their prejudice levels than other parents. It does not imply that the children actually express similar levels of prejudice as their parents do, as their values might be higher, equal, or lower. By contrast, a simple comparison of means of parent and child prejudice only provides information on whether children on average hold similar levels of prejudice as parents on average, allowing parent–child dyads to vary in terms of their (dis)similarity. Thus, correlations neglect possible mean differences, and mean differences neglect possible correlations. Neither of these analyses would, for example, allow a comparison between parent–child similarity and peer–child similarity. If it is desirable to include mean differences in a coefficient of similarity, intraclass correlations are a possible alternative (e.g., Field, 2005a), and in some cases, dyadic profile correlations can also be of value (Gniewosz, Noack, Wentura, & Funke, 2008). Furthermore, dyadic research proposes a number of various distance indices that can be used to quantify the degree of mutual resemblance of individuals within a dyad (see Kenny, Kashy, & Cook, 2006).

Second, even if significant levels of parent–child similarity of intergroup attitudes can be demonstrated with any of the mentioned coefficients, we cannot interpret these as unidirectional indicators of socialization. It is impossible to determine whether or to what degree parent–child similarity is based on parent–child influences or child–parent influences or an interaction of both. Interestingly, directionality was rarely discussed in the publications that were included in this meta-analysis, and even when researchers acknowledge the possibility of bidirectional influences, they typically dismiss child/parent-directed influences as minor or negligible. For example, Burt and Scott (2002) "acknowledge that attitudinal influence between parent and child is unlikely to be uni-directional" but still "speculate that the direction of influence is mainly from parent to youth" (Burt & Scott, 2002, p. 244; see also O'Bryan et al., 2004). Unfortunately, most of the included studies rely on cross-sectional data, which allow no firm conclusions regarding the directionality and causality of effects. We have found only few studies that explicitly tested for bidirectionality, and their results document that bidirectional influences should not be dismissed entirely. For example, in a 3-year longitudinal study, Vollebergh et al. (2001) found a relatively stable child influence on parents' ethnocentrism for young Dutch adolescents, whereas the effect of parent–child influence decreased.

Comparing three generations (i.e., children, parents, and grandparents) in a cross-sectional design, Glass, Bengtson, and Dunham (1986) found similar results with stable child–parent influences across generations and declining parent–child influences from younger to older generations. On the other hand, Rodríguez-García and Wagner (2009) found that an unidirectional model of prejudice determination, assuming socialization effects from parents to children only, fitted their data from Costa Rican adolescents equally well as a model including reciprocal influences (implying unimportance of the child–parent link), but these data were collected in a cross-sectional study. Given the scarce empirical evidence, the question remains open whether and to what extent parent–child socialization of intergroup attitudes might be a reciprocal process. Further longitudinal studies with repeated measurements of parental and child attitudes are needed to disentangle the complex relationships between parent and child attitudes (P. A. Katz, 1976).

Further complicating the previous point, parent–child similarity may be the result of neither parent–child nor child–parent influences, but rather may reflect a number of other mediating variables (e.g., Aboud, 1988; Knafo, 2003). Shared factors in the immediate or broader family environment could cause similar levels of intergroup prejudice in both parents and their children, even without any direct transmission processes taking place within the family. In this regard, we have to distinguish between macrolevel factors and meso- or microlevel factors. Macrolevel factors include societal influences such as the socioeconomic situation, the societal value climate, shared cultural norms, sensitivity to open group conflicts, and media influences that parents and children are jointly exposed to. Each of these factors could result in similar levels of prejudice within a society and thus also within the family. Therefore, parent–child similarity (in terms of equal levels of prejudice) would not be indicative of parental influence per se. Meso- and microlevel factors such as family-specific direct (and diverse) environmental contexts, including socioeconomic status, educational attainment, and neighborhood diversity, could exert similar forces on intergroup attitudes but would lead to more within-society and between-family variance in intergroup attitudes. Thus, in samples from mixed environments, parent–child similarity in terms of similar degrees of prejudice and/or parent–child correlations would again not necessarily be indicative of parental socialization. Of course, we should also keep in mind that exposure to environments that promote attitudes consistent with parents' attitudes may not be accidental. Parents often selectively choose specific environments (e.g., neighborhood, school, leisure activities, media exposure) that promote values consistent with their own values, which in turn indirectly facilitate parent–child socialization (e.g., Barrett & Davis, 2008; Barry, 1980; Hello, Scheepers, Vermulst, & Gerris, 2004; Knafo, 2003). Even if the explicit selection criteria employed by parents are unrelated to the specific attitudes we are concerned with here (e.g., parents might focus solely on educational quality when choosing a school for their child), this is still likely to lead to value-consistent environments (e.g., more ethnically homogeneous schools). Ultimately, further research will be necessary to clarify whether or to what extent shared environmental influences are mediating variables and direct or indirect socialization effects.

Fourth and finally, we must address the question whether significant indices of parent–child similarities even in methodologically rigorous longitudinal studies can be interpreted as reflecting

socialization effects (be it parent–child, child–parent, or environment–parent/child) given the possibility of genetic influences. Because effects of parental genes and parental attitudes and behaviors are correlated, ostensible influences of parental attitudes may actually be artifacts of genetic influences. Several twin studies provide first evidence that intergroup attitudes (e.g., Eaves & Hatemi, 2008; Olson, Vernon, Harris, & Jang, 2001; Stößel, Kämpfe, & Riemann, 2006) and related attitudes, such as social conservatism, social dominance orientation, right-wing authoritarianism (Eaves et al., 1997; Stößel et al., 2006), and political attitudes (Hatemi et al., 2009), have modest to substantial heritability coefficients. Notably, these heritability indices are similar in magnitude to the effect sizes we have found in the current metaanalysis. Furthermore, the shared environment of siblings—which would include parental attitudes and behaviors—appears to be of negligible importance in these studies (as compared to the unshared environment). This could be taken to imply that parents have little direct influence on their children's intergroup attitudes, or at least that they do not affect different siblings in the same ways. Such results do not necessarily imply a direct genetic determination of intergroup attitudes; they can be interpreted as shared genetic factors in parents and children causing general predispositions (e.g., traits such as sociability; Olson et al., 2001) that in turn shape environmental experiences that trigger or support the formation of specific intergroup attitudes. We note that all but one of the available twin studies were conducted with adult samples, precluding direct conclusions regarding parental socialization effects during childhood or adolescence. Indeed, using longitudinal sibling data, Eaves et al. (1997) found that during adolescence the siblings' shared environment (i.e., parents) played a dominant role in the creation of within-family resemblance in conservatism and social group attitudes, whereas genetic factors played no significant role. These roles were, however, reversed when young adults left the parental home, with genetic influences becoming dominant in explaining sibling similarity during adulthood and shared environmental influences entirely losing relevance. Clearly the relationship between genetic transmission and parental socialization needs to receive more attention if we are to understand whether and which within-family processes play a role in the formation of intergroup attitudes during childhood and adolescence.

In summary, despite the large literature reviewed and synthesized here, inadequate methods and relatively simple models of parental influence limit the extent to which we can arrive at firm conclusions regarding the role parents play as sources of childhood prejudice. Thus, further research taking into account the aforementioned recommendations regarding measurement development, research designs, and methods of statistical analyses is direly needed. Even if we could confirm that the parent–child relationship identified in our meta-analysis stems from parental socialization influences on children's intergroup attitudes, further questions remain about the nature of the transmission between parents and their offspring. Which roles do processes such as direct teaching, indirect parental messages, admonitions, prohibitions (e.g., Bird et al., 1952a; Edmonds & Killen, 2009), concrete examples and modeling, observational learning and imitation (e.g., Bandura, 1977; Bussey & Bandura, 1999; Castelli et al., 2009), operant learning and social reinforcement (e.g., Bigler & Liben, 2006, 2007), child-rearing techniques and parenting styles (e.g., Allport,

1954; Cossman, 2004; Ex & Janssen, 1998), and inheritance and selective exposure to social and structural conditions (e.g., Hello, Scheepers, Vermulst, & Gerris, 2004; Jaspers et al., 2008) play in children's acquisition of intergroup attitudes? And how do they relate to active constructivist processes in children such as those suggested by Aboud (1988) and Bigler and Liben (2006)? Answering these questions will help us understand how prejudiced attitudes and behaviors are transmitted and whether or why they may be differently adopted by children of varying age and identity (Aboud, 2008). The widely replicated findings from social psychology that group attitudes can be learned in specific ways in adulthood (e.g., paired association learning, illusory correlations) does not eliminate the possibility that prejudice may first be learned very differently during childhood and adolescence (e.g., Dunham & Degner, 2010). Recent trends in social cognitive research focusing on implicit or automatic components of intergroup attitudes can provide the theorizing and methodology to help understanding the perception and acquisition of these types of intergroup attitudes—under circumstances when parents do not express overt prejudice (e.g., Baron & Banaji, 2006; Castelli et al., 2009; Degner & Wentura, 2010; Degner, Wentura, Gniewosz, & Noack, 2007; Dunham, Baron, & Banaji, 2006, 2007; Rutland et al., 2005; Sinclair et al., 2005). In return, developmental research using social-cognitive paradigms can weigh in on current debates on the origin of implicit attitudes and automatic intergroup behaviors and the often-found dissociation between implicit and explicit attitudes (Dunham & Degner, 2010; Dunham & Olson, 2008). Thus, besides focusing on the amplitude of possible parent–child socialization, our research should also try to investigate the possible mechanisms and processes that underlie socialization effects.

Conclusion

In conclusion, our meta-analysis has uncovered that children's and their parents' intergroup attitudes are significantly related, with small to moderate effect sizes. This relation may possibly be underestimated based on methodological issues and aspects of the assessment situations of the available studies. On the basis of these findings, we have furthermore discussed possible shortcomings of previous research and offer a number of improvements that should be implemented in further research to allow clear conclusions regarding the role of parental socialization of childhood prejudice. We would like to stress that a closer collaboration between disciplines and subdisciplines, such as social, developmental, and cognitive psychology, but also sociology and education research, not only is required to further our understanding of the origins of intergroup attitudes, but also can provide important insights for main research questions in the respective fields.

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(*Appendix follows*)

Appendix

Characteristics of Studies Included in the Meta-Analysis

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
Aboud & Doyle (1996)		9.4	M	R/E	M	C	21	.54*
		9.4	M	R/E	M	P	39	−.15
Acock & Bengtson (1978)		18.0	M	R/E	M and F	P	378	.41^nt
				SC	M and F	P	374	.28^nt
Acock & Bengtson (1980)		19.0	M	R/E	M and F	P	417	.28*
							417	.42*
				SC	M and F	C	414	.36*
							414	.42*
Ajdukovic & Biruski (2008)	Sample 1	14.0	M	R/E	M and F	P	169	.18*, .30*
	Sample 2	14.0	M	R/E	M and F	P	165	.37*, .15*
Antill et al. (2003)		10.0	M	G	M	P	191	.31*
				G	F	P	191	.26*
Ata et al. (2009)		16.9	M	R	M and F	C	916	.78*, .19*, .45*, .50*
Bacher (2001)		18.0	M	I/N	M and F	C	353	.45*
Bagby-Young (2008)		4.3	M	R/E	M or F	P	47	.17
Barak et al. (1991)	Sample 1	6.0	B	G	M	P	46	−.05, .31*
				G	F	P	42	.17, −.16
	Sample 2	6.0	G	G	M	P	63	.00, .20*
				G	F	P	57	.01, −.08
Bird et al. (1952a)	Sample 1	9.5	B	R/E	M	P	72	.16
				R/E	F	P	62	.36*
	Sample 1	9.5	B	R	M	P	72	.17
				R	F	P	62	.19
	Sample 2	9.5	G	R/E	M	P	79	.24*
				R/E	F	P	72	.05
	Sample 2	9.5	G	R	M	P	79	.09
				R	F	P	72	.08
Bird et al. (1952b)	Sample 2	9.4	B	R/E	M and F	C	72	.49*
	Sample 2	9.4	G	R/E	M and F	C	80	.38*
Biruski & Ajdukovic (2008)	Sample 1	14.1	M	R/E	M or F	P	227	.31*, .31*
	Sample 2	14.1	M	R/E	M or F	P	223	.30*, .14*
Bliss (1988)		5.0	M	G	M	P	24	.35*
				G	F	P	24	ns
Branch & Newcombe (1980)	Sample 1	5.7	M	R/E	M or F	O	66	.25*, ns
Branch & Newcombe (1986)	Sample 2	4.5	M	R/E	M and F	P	26	−.14, −.13, −.56*, −.60*, −.50*, −.48*, .09, .05
Briscoe (2005)		6.5	M	R/E	M and F	P	50	.09, .21, .22, .30*, .05, .09, .01, .11
		8.0	M	R/E	M and F	P	45	−.29+, .02
Burt & Scott (2002)	Sample 1	14.5	B	G	M	P	209	.07, .14*, .20*
				G	F	P	157	.20*, .01, .22*
	Sample 2	13.5	G	G	M	P	193	.12, .31*, .16*
				G	F	P	141	.06, .19*, .09

(*Appendix continues*)

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
Carraro et al. (2011)		6.0	M	G	M	P	18	.29, .60*
					F	P	18	.18, .03
J. M. Carlson & Iovini (1985)	Sample 1	15.0	B	R/E	F	P	100	.41[nt]
		15.0	B	R/E	F	C	100	.48[nt]
	Sample 2	15.0	B	R/E	F	P	100	−.15[nt]
		15.0	B	R/E	F	C	100	−.06[nt]
D. L. Carlson & Knoester (2011)	Sample 1	20.7	G	G	M	P	124	.40[nt]
					F	P	82	.32[nt]
	Sample 2	20.7	B	G	M	P	115	.12[nt]
					F	P	77	.29[nt]
	Sample 3	20.5	G	G	M	P	116	.17[nt]
					F	P	8	.26[nt]
	Sample 4	20.5	B	G	M	P	129	.19[nt]
					F	P	14	.21[nt]
	Sample 5	20.4	G	G	M	P	60	.11[nt]
					F	P	25	−.12[nt]
	Sample 6	20.4	B	G	M	P	61	.28[nt]
					F	P	20	.18[nt]
Castelli et al. (2007)		5.8	M	R/E	M	C	58	.42*, .34*
					F	C	58	.22+, .23+
Castelli et al. (2009)		4.8	M	R/E	M	P	70	.29*, .08, .35*, .09, .03, −.01
					F	P	68	.07, .09, .10, −.05, .03, −.00
Chatard & Selimbegovic (2008)		21.3	M	O/G	M or F	P	93	.37*
Correia et al. (2011)	Sample 1	9.5	M	R/E	M	P	66	.33*, −.09
Correia et al. (2011)	Sample 2	6.5	M	R/E	M	P	42	.23, .48*
		9.5	M	R/E	M	P	26	.33, .48*
Cossman (2004)		9.5	M	D	M	P	330	.16*, .09
Cunningham (2001)		18.0	M	G	M	P	791	.14*, .27*
Dambrot et al. (1984)		19.1	G	G	M	P	43	.34*
Davison & Birch (2004)		9.0	G	BW	M	P	178	.05
					F	P	178	.14
Dawson (2011)		9.7	M	G	M	P	106	.37*
		12.0	M	G	M	P	106	.33*
Dhont & van Hiel (2012)		16.7	M	I/N	M or F	P	99	.54*, .46*, .46*, .43*, −.09, .07
Duckitt (1994)		19.3	M	R/E	M and F	C	291	.49*
Duncan et al. (2002)	Sample 1	17.5	G	G	M	P	462	.14[nt]
					F	P	462	.08[nt]
	Sample 2	17.5	B	G	M	P	435	.17[nt]
					F	P	435	.17[nt]
Duriez & Soenens (2009)		17.6	M	R/E	M	P	528	.28*
					F	P	447	.21*
Edmonds & Killen (2009)	Sample 1	14.5	M	R/E	M and F	P	188	−.05
	Sample 2	17.5	M	R/E	M and F	P	152	−.03
Eliason (1998)		20.7	M	SO/G	M and F	C	116	.40*
R. Epstein & Komorita (1966a)		9.5	M	R/E	M and F	C	56	.57*
		9.5	M	R/E	M and F	C	53	.63*
R. Epstein & Komorita (1966b)		10.0	M	O/G	M and F	C	118	.73*

(*Appendix continues*)

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
M. Epstein & Ward (2011)	Sample 1	15.6	G	G	M and F	C	157	.38*, .32*, .40*, .14+
	Sample 2	15.6	B	G	M and F	C	86	.31*, .26*, .18+, .20+
	Sample 3	19.5	G	G	M and F	C	131	.32*, .40*, .30*, .22*
	Sample 4	19.5	B	G	M and F	C	157	.43*, .29*, .28*, .12
Ex & Janssens (1998)		18.5	G	G	M	P	165	.25*
Fagot & Leinbach (1995)		4.0	M	G	M	P	69	ns
					F	P	69	ns
Fend (1994)		16.0	M	I/N	M and F	P	1,199	.23*
Ford (2009)		13.6	M	R/E	M	C	212	.06, .12
					M	P	212	.04, −.11
Friedman et al. (2007)	Sample 1	4.0	M	G	M	P	37	.38*
	Sample 2	6.5	M	G	M	P	36	−.03
Fulcher et al. (2008)		5.2	M	G	M and F	P	60	.38*, .28*
Garaigordobil & Jone (2011)	Sample 1	14	G	G	M	P	764	.30, .14*, .05, .16*
					F	P	648	.09, .13*, .13*, .19*
	Sample 2	14	B	G	M	P	764	.17*, .13*, .18*, .12*
					F	P	648	.04, .02, .09, .03
Glass et al. (1986)		19.7	M	G	M and F	P	478	.37*
Gniewosz & Noack (2008)	Sample 1	12.0	M	I/N	M	C	378	.89*
					F	C	370	.88*
	Sample 1	12.0	M	I/N	M	P	256	.27*
					F	P	219	.32*
	Sample 2	13.5	M	I/N	M	C	433	.82*
					F	C	416	.80*
	Sample 2	13.5	M	I/N	M	P	272	.35*
					F	P	229	.30*
	Sample 3	15.5	M	I/N	M	C	448	.75*
					F	C	433	.72*
	Sample 3	15.5	M	I/N	M	P	255	.39*
					F	P	210	.32*
Gniewosz et al. (2003)	Sample 1	11.5	M	I/N	M	C	265	.84*, .97*
					F	C	265	.85*, .94*
	Sample 2	13.5	M	I/N	M	C	319	.67*, .90*
					F	C	319	.62*, .85*
	Sample 3	15.5	M	I/N	M	C	275	.59*, .87*
					F	C	275	.48*, .82*
Gniewosz et al. (2008)		17.5	M	I/N	M	C	104	.71*
					F	C	104	.65*
Griffore & Schweitzer (1983)	Sample 1	9.5	B	R/E	M	P	246	.52*
					F	P	115	.62*
	Sample 2	13.0	B	R/E	M	P	237	.51*
					F	P	112	.53*
	Sample 3	16.5	B	R/E	M	P	202	.50*
					F	P	120	.62*
	Sample 4	9.5	G	R/E	M	P	271	.55*
					F	P	84	.63*
	Sample 5	13.0	G	R/E	M	P	235	.50*
					F	P	85	.42*
	Sample 6	16.5	G	R/E	M	P	249	.51*
					F	P	109	.53*

(*Appendix continues*)

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
Grob (2005)		16.5	M	I/N	M and F	P	309	.28*
Guerrero et al. (2011)		4.0	M	R/E	M	C	50	.23*, .51*
Hansson & Rasmussen (2010)	Sample 1	10.0	M	BW	M or F	P	1,383	.05*
	Sample 2	10.0	M	BW	M or F	P	1,383	.05*
Hello et al. (2004)		17.0	M	R/E	M	P	201	.19*
					F	P	201	.22*
Hess et al. (2006)	Sample 1	18.6	B	G	M	P	244	.26*
					F	P	244	.36*
	Sample 2	18.7	G	G	M	P	244	.41*
					F	P	244	.32*
Himelhoch (1950)		19.0	M	R/E	M or F	P	64	.57*
Hoffman & Kloska (1995)		9.0	M	G	M	P	213	.21*
					F	P	364	.17*
Holub et al. (2011)		4.8	M	BW	M	P	48	.09, .07, .32*, .30*
Hughes et al. (2009)		11.5	M	R/E	M	P	170	.06
						C	170	.42*
Jasinskaja-Lahti et al. (2011)	Sample 1	15.0	M	R/E	M and F	C	93	.66*
	Sample 2	15.0	M	R/E	M	C	126	.40*
					F	C	118	.32*
Jennings et al. (2009)		18.0	M	R/E	M or F	P	636	.33[nt]
Jorm & Wright (2008)		16.6	M	D	M or F	P	1,997	.14*, .17*
Knight et al. (1993)		8.0	M	R/E	M	P	45	−.16
Kulik (2000)		15.0	M	G	M	P	134	.36*, .40*
					F	P	134	.29*, .37*
Kulik (2002)	Sample 1	15.1	B	G	M	P	67	.29*, .31*, .34*, .46*
					F	P	67	.56*, .42*, .50*, .58*
	Sample 2	15.1	G	G	M	P	67	.22*, .34*, .56*, .51*
					F	P	67	.35*, .22*, .52*, .30*
Kulik (2004)		15.0	G	G	M	P	60	.42*
Kulik (2005)	Sample 1	15.9	B	G	M	P	51	.29*
					F	P	51	ns
				R/E	M	P	51	ns, ns
					F	P	51	.61*, .35*
	Sample 2	15.9	G	G	M	P	56	.45*
					F	P	56	ns
				R/E	M	P	56	ns, ns
					F	P	56	.33*, ns
C. B. Lam et al. (2012)		15.7	M	G	M	P	236	.28
					F	P	236	.27
V. Lam et al. (2011)		4.0	M	R/E	M	C	123	.33*, .25
Mähönen et al. (2011)		14.1	M	R/E	M and F	C	721	.48*, .20*, .31*, .39*, .37*
Marks et al. (2009)	Sample 1	16.7	M	G	M	P	191	.20*.29*
					F	P	179	.19[+], .23*
	Sample 2	14.2	M	G	M	P	191	.13[+], .26*
					F	P	180	.21*, .16*
Marantz & Mansfield (1977)		7.8	G	G	M	O	98	.33*, .28*

(*Appendix continues*)

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
Mayer et al. (2004)		14.0	G	G	M	P	1,003	.05[nt]
McHale et al. (2001)		14.9	M	G	M	P	193	.20*
McHale et al. (1999)		10.4	M	G	F	P	192	.19*
					M	P	202	.20*
					F	P	202	.23*
Meyer (1980)	Sample 1	7.0	G	G	M	P	52	.05, .14
	Sample 2	11.0	G	G	M	P	52	.33*, .25*
Mezei (1971)		18.0	M	R/E	M and F	C	80	.26[nt]
		18.0	M	R/E	M and F	C	57	.37[nt]
Montañés et al. (2012)		13.4	G	G	M	P	164	.12, .16, .19, .35*
Monaci & Trentin (2008)		10.9	M	R/E	M and F	C	133	.60*
Monteiro et al. (2009)	Sample 1	6.5	M	R/E	M and F	C	101	.24[+]
	Sample 2	9.5	M	R/E	M and F	C	100	.03
Mosher & Scodel (1960)		12.0	M	R/E	M	P	161	.32*
Mills et al. 1995		20.3	M	R/E	M and F	C	142	ns
Neblett et al. (2009)		14.0	M	R/E	NR	C	358	.09
Newman (1989)	Sample 1	20.0	B	SO	M and F	C	97	.32[nt]
	Sample 2	20.0	G	SO	M and F	C	156	.44[nt]
Newman (2007)	Sample 1	19.8	B	SO	M and F	C	53	.46*
					M	C	53	.48*
					F	C	53	.39*
	Sample 2	19.7	G	SO	M and F	C	87	.43*
					M	C	87	.50*
					F	C	87	.30[nt]
Niemi et al. (1978)		20.0	M	SO	M or F	P	663	.26[nt]
				SC	M or F	P	663	.18[nt]
				R/E	M or F	P	663	.18[nt]
Noack (2001)	Sample 1	15.7	B	I/N	M	P	142	.21*
					M	P	126	.26*
	Sample 2	15.7	G	I/N	M	P	185	.43*
					F	P	161	.35*
O'Bryan et al. (2004)		15.5	M	R/E	M	P	111	.27*
					F	P	111	.09
				G	M	P	111	.10
					F	P	111	.23*
				O/G	M	P	111	.16
					F	P	111	.20*
				SO	M	P	111	.13
					F	P	111	.22*
				D	M	P	111	.15
					F	P	111	.10
				BW	M	P	111	.16
					F	P	111	.05
O'Connor et al. (2000)		8.5	G	R/E	M	P	51	.21*
Oepke (2008)		14.8	M	I/N	M and F	P	312	.36*
					M	P	312	.34*
					F	P	305	.29*
Okagaki & Moore (2000)		20.3	M	R/E	M	P	69	.40*
					F	P	60	.31*

(*Appendix continues*)

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
Oksal (2008)		20.2	M	SO	M	P	116	.55*, .52*
					F	P	116	.48*, .38*
Olalla & Vella (2007)	Sample 1	17.3	B	G	M	P	700	.21*
	Sample 2	17.2	G	G	M	P	641	.20*
Pahlke et al. (2012)		4.7	M	R/E	M	P	84	.14, .26, .08, .08
						C	84	.11, .12, .14, .66*
Pellett & Ignico (1993)		8.3	M	G	M and F	P	345	.24*
Perloff (1977)		11.0	M	G	M	P	86	.02
					F	P	70	.01
Peterson & Duncan (1999)		18.1	M	O/G	M and F	P	75	.48*
Pushkin & Norburn (1983)		5.0	M	R/E	M	P	172	.11
Quiñones et al. (1999)		20.0	G	G	M	P	147	.29*
					F	P	107	.27*
Radke-Yarrow et al. (1952)		7.0	M	R/E	M or F	P	99	.29*
				R	M or F	P	99	.17+, .21*
Rippl (2004)		16.8	M	I/N	M	P	457	.48[nt]
					F	P	457	.37[nt]
Rivas-Drake et al. (2009)		11.5	M	R/E	NR	C	308	.32*
Rodriguez-García & Wagner (2009)		16.5	M	R/E	M or F	P	408	.37*
		16.5	M	I/N	M or F	P	408	.45*
Rollins & White (1982)		12.0	G	G	M	P	75	.55*, .43*, .54*
Sabatier (2008)		15.5	M	R/E	M	P	357	.34*, .11*, .23*
					F	P	291	.28*, .16*, .19*
Schmid (2008)		16	M	I/N	M	P	761	.29*
					F	P	761	.24*
Shebloski & Gibbons (1998)	Sample 1	15.5	B	G	M or F	P	30	.26
					M	P	17	.44+
					F	P	11	.25
	Sample 2	15.5	G	G	M or F	P	39	.51*
					M	P	27	.44*
					F	P	10	.57+
Sjongers (2007)		17.0	M	R/E	M or F	P	6,974	.42[nt]
E. P. Smith et al. (2003)		9.5	M	R/E	M or F	P	98	.16
S. T. Smith & Ross (2006)		20.2	M	R/E	M and F	P	24	.38+
		20.2	M	R/E	M	C	108	.24*, .12
					F	C	84	.21+, .26*
M. D. Smith & Self (1980)		18.0	G	G	M	P	74	.36[nt]
Sinclair et al. (2005)		10.0	M	R/E	M or F	P	66	.17, −.11
Six et al. (2009)		15.3	M	I/N	M	P	325	.26*
					F	P	325	.31*
Stephan & Rosenfield (1978)		13.0	M	R/E	M	P	65	.26*, .33*
Sutfin et al. (2008)		5.2	M	G	M and F	P	57	.36*
Tedin (1974)		18.0	M	R/E	M and F	P	183	.32[nt]
ter Bogt et al. (2001)		18.0	M	O/G	M or F	P	1,115	.36[nt]
Todosijević & Enyedi (2002)		16.5	M	R/E	M or F	P	358	.42*, .30*
Tomasetto et al. (2011)		6.2	G	G	M	P	124	.01
					F	P	124	.03
Towles-Schwen & Fazio (2001)		20.0	M	R/E	M and F	C	184	−.09, .07

(*Appendix continues*)

Study	Sample	Child age[a]	Child gender[b]	Target domain[c]	Parent gender[d]	Parent source[e]	N	r (uncorrected)[f]
Tu (2007)		4.3	M	R/E	M or F	P	87	−.08, .12
		4.3	M	G	M or F	P	87	.24*, −.12, .07
Urban & Singelmann (1997)		16.0	M	I/N	M	P	96	.31^nt
					F	P	131	.03^nt
Vaillancourt (1996)		7.5	M	G	M	P	50	−.04, .08
Verkuyten (2002)		11.0	M	R/E	M and F	C	453	.27*
Vieira (2006)	Sample 1	16.1	B	G	M	P	88	.00
					F	P	88	−.20^+
	Sample 2	16.1	G	G	M	P	132	.07
					F	P	132	−.03
Vittrup & Holden (2011)	Sample 1	6.0	B	R/E	M	P	93	−.16^nt
					F	P	93	.11^nt
	Sample 2	6.0	G	R/E	M and F	C	93	.35^nt
					M	P	93	.32^nt
					F	P	93	.36^nt
					M and F	C	93	.21^nt
Vittrup Simpson (2007)		6.0	M	R/E	M	P	93	−.11, .09, −.01, .11
					F	P	93	.13, .28*, −.12, .21
					M and F	C	93	.21*, .20
Weeks et al. (1984)		16.3	G	G	M	P	64	.41*
White & Gleitzman (2006)		19.3	M	R/E	M	P	86	.35*, .16*
					F	P	75	.29*, .36*

Note. For a more convenient presentation, different effect sizes from different measures are presented together in one row.
[a] Mean sample age in years. [b] M = mixed sample; B = boys; G = girls. [c] BW = body weight; D = disability/disease; G = gender; I/N = immigration or nationality; O/G = other/generalized; SC = social class; SO = sexual orientation; R/E = race or ethnicity; R = religion. [d] M = mother; F = father; NR = not reported. [e] P = parent reported; C = child reported; O = other reported. [f] Correlations reported to be significant ($p < .05$) are marked by an asterisk or by a plus sign when reported as marginally significant ($p < .10$). Correlations without index were reported as nonsignificant. Correlations whose significance status was not reported are marked by *nt*; nonreported results that were stated "nonsignificant" are labeled *n*s.