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Examining Factors That Facilitate Young Children's Social Connections: An Experimental Approach

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Disciplines across the social and behavioral sciences have documented the critical role of early peer connections in children's development, yet mechanisms facilitating these connections have not been tested in controlled laboratory settings. The present experiment manipulated the context in which unacquainted pairs of children interacted for the first time. We randomly assigned 288 5- to 11-year-old U.S. children (girls: $n = 142$, boys: $n = 145$, nonbinary: $n = 1$; American Indian or Alaska Native: $n = 2$, Asian or Asian American: $n = 24$, Black or African American: $n = 8$, Hispanic or Latino: $n = 17$, White or Caucasian: $n = 200$, Multiracial: $n = 33$, other: $n = 2$, not provided: $n = 2$) to interact in pairs with a same- or different-gender partner who matched their age. Randomly assigned peer interactions occurred in one of three conditions: interaction without support (*baseline* condition) or interaction preceded by adult support that scaffolded children's active (*active experimental* condition) or passive (*passive experimental* condition) participation in a self-disclosure activity. Adult coders rated children's connections during the interaction, and children later rated their feelings about their partner. Results revealed that the experimental conditions facilitated children's connections relative to baseline and eliminated gender ingroup favoritism in children's connections; the *active experimental* condition was particularly effective relative to the *passive experimental* condition. With its focus on testing mechanisms of connection, this research introduces a new experimental approach that is capable of informing theories and practices surrounding the formation of young children's peer connections.

Public Significance Statement
The present research introduces a new experimental approach to test causal mechanisms that facilitate children's peer connections, highlighting the utility of adult scaffolding and active self-disclosure in facilitating connections between children meeting for the first time. Moreover, results point to causal factors that could be implemented by educators and clinicians in programs that promote positive relations among children.

Keywords: children, social connection, peer relations, gender, scaffolding

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What facilitates interpersonal connection in childhood? This question underlies a range of inquiries across the social and behavioral sciences—from psychology to education to sociology—documenting the critical role of early peer connections in children's development (Corsaro, 1997; Rubin et al., 2006; Wentzel, 2009). Children with positive peer relationships fare better in myriad domains, including mental health and academics (Corsaro, 1997; Wentzel, 2009), and the importance of peer relationships has come into even greater focus following the COVID-19 pandemic (Aral & Kadan, 2023; Office of the Surgeon General, 2021; Wong et al., 2020). At the same time, it is difficult to assess the concrete mechanisms by which initial peer-to-peer interactions are stifled or supported, due to the complexity of the social environments in which those interactions unfold. The current project takes a controlled experimental approach: manipulating potential factors that could facilitate unacquainted children's social connections, especially among children from different social groups.

Naturalistic Contexts

The majority of what we know about social connection formation and maintenance in childhood comes from correlational research. Once a friendship has been established, children exhibit specific patterns of behavior with friends that differ from their patterns of behavior with nonfriend peers. For example, children who are friends talk more with one another, share more in common with one another (e.g., identities, preferences, or behaviors), and report feeling closer to one another than children who are not friends (see Newcomb & Bagwell, 1995 and Rubin et al., 2011 for review). Existing research on children's friendships tends to examine friendships that have already formed on their own (e.g., Dunn & Cutting, 1999; Hartup et al., 1988). Thus, specific factors critical to the initial formation of peer connections are not well understood.

Limited research in naturalistic contexts—in which children interact freely without adult support—reveals that previously unacquainted pairs of children who spontaneously "hit it off" tend to initially engage in several behaviors not evident among pairs of children who fail to connect. First, initial interactions between children who spontaneously connect tend to involve reciprocity and effective communication—for instance, exchanging information (e.g., "I like yellow"; "I like green"), remaining on the same page about topics being discussed (e.g., preferred colors), and taking turns speaking (Bowker et al., 2010; Bukowski et al., 1998; Gottman & Graziano, 1983). Children who spontaneously connect also tend to share something in common, such as an activity they both enjoy (Bowker et al., 2010; Bukowski et al., 1998; Gottman & Graziano, 1983), and increasingly engage in self-disclosure (i.e., sharing personal feelings or preferences) over the course of their initial interaction (Gottman & Graziano, 1983).

Newer applied research, conducted in schools, reveals that developmental scientists can apply theory and observations from correlational research to facilitate children's social connections (e.g., Fabes et al., 2019; Guedes et al., 2023; Hanish et al., 2021; Miller et al., 2017). For example, building on Intergroup Contact Theory (Allport, 1954), Hanish et al. (2021) implemented a "Buddy Up" program in which teachers provided opportunities for preschool students to complete activities with different-gender peer partners; the program involved various activities administered in classrooms over the course of several weeks, including paired activities, songs, and lesson plans. The researchers found that children whose classrooms participated in the program were more likely to play with different-gender peers compared to children in control classrooms. Interventions such as the "Buddy Up" program provide evidence that adults can support children's formation of new peer connections; however, applied interventions tend to combine numerous factors believed to support children's friendship formation over long periods of time, leaving open questions about which factors are crucial for supporting new peer connections.

An Experimental Approach

Correlational research reveals factors related to forming and maintaining close social connections in childhood, and applied research provides evidence that interventions can improve children's peer relationships in naturalistic settings. Yet, a third form of research—namely, controlled laboratory studies—can contribute unique insights into the formation of social connections in childhood. In particular, controlled laboratory experiments are able to test the precise causal roles of different factors in supporting relationships. Further, controlled laboratory experiments can isolate specific mechanisms leading to the formation of positive peer connections—separate from the complexity and diversity of factors inherent to naturalistic contexts; in turn, such work could enable researchers in the field to design more parsimonious interventions.

The Present Study

The present project establishes a novel approach to addressing questions about how to best support the formation of social connections between children: pairing children with novel peers, carefully manipulating the scaffolding of children's interactions, and then measuring children's connections based on (a) adults' impressions of how successful the children's interaction was and (b) children's subjective ratings of their peer partner (e.g., feelings of warmth) and perceptions of their connection (e.g., ratings of closeness). In the *baseline* condition of the present experiment, two children were brought together for the first time without adult support: Two unacquainted children ("novel social partners") met virtually, and we measured their resulting connection. This *baseline* condition alone was informative, allowing us to measure children's connections in same-gender versus different-gender pairs without additional manipulations of the meeting context. In two additional *experimental* conditions, children also completed the *free talk* period and dependent measures, but first experienced scaffolding to support their initial meeting. As described further below, both *experimental* conditions included scaffolding from an experimenter who led novel social partners through a brief activity to get to know each other.

We designed the *experimental* conditions based on our prediction that adult-led scaffolding facilitates young children's ability to communicate with each other compared to leaving children to communicate without support. Children vary widely in their ability to communicate effectively with peers (Leaper & Smith, 2004; Sehley & Snow, 1992)—especially different-gender peers (Leman et al., 2005). Applied interventions have addressed these barriers by providing children with adult support (e.g., social skills training or classroom curricula; Factor et al., 2022; Guedes et al., 2023; Hanish et al., 2021). However, these field interventions typically require immense investments of time and other resources, manipulate many

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aspects of children's environments simultaneously (e.g., classroom curricula and parent training), and employ business-as-usual control conditions. Thus, these interventions are difficult to implement and preclude distinctions between factors that facilitate children's connections and factors that are superfluous or harmful to children's connections. The present experiment adopts a leaner and more controlled approach: Participants in matched and tightly controlled *experimental* conditions experienced a brief (approximately 5-min) period of adult-led communication with a novel social partner, which they could build upon independently in a subsequent interaction.

Many features of children's interactions could have been manipulated in the present experiment, but we aimed to implement scaffolding that was short, reproducible, and could reasonably facilitate children's connections. We chose adult-led scaffolding that guides novel social partners through a brief activity to learn facts about one another because (a) adult-led collaborative activities have successfully structured children's interactions (i.e., in a manner that increased children's perceived connections with peers) in applied interventions (e.g., curricula, songs, or games; Guedes et al., 2023; Hanish et al., 2021; Miller et al., 2017); (b) reciprocal communication and self-disclosure are commonly observed when children form new peer connections in naturalistic contexts (Gottman & Graziano, 1983); and (c) previous studies of adult relationships demonstrate the utility of self-disclosure in helping people connect in experimental contexts (e.g., the Fast Friends Paradigm: Aron et al., 1997; Page-Gould et al., 2008; see also Sprecher et al., 2013 and Vittengl & Holt, 2000). Here, we predicted that adult-led scaffolding of a self-disclosure activity would facilitate connections between novel social partners compared to the *baseline* condition.

In addition to comparing the *baseline* condition to the *experimental* conditions overall, we further manipulated children's autonomy during the scaffolded interaction between two *experimental* conditions. Although adult-led scaffolding appears to often be beneficial—even necessary—in helping children connect (e.g., Guedes et al., 2023; Miller et al., 2017), children also connect better when taking an active role in their own social experiences. For instance, children whose caregivers provide gentle support around peer interactions (e.g., arranging playdates) connect better with peers than children whose caregivers provide more intrusive support (e.g., "helicopter parenting"; Grolnick et al., 2002; Ladd, 1999; Locke et al., 2012), and young children tend to interact more positively with peers in child-directed (vs. adult-directed) activities in educational contexts (Acar et al., 2017; Vitiello et al., 2012).

Here, the *experimental* conditions scaffolded either *active* self-disclosure (in which novel social partners shared information about themselves through prompting from one another) or *passive* self-disclosure (in which novel social partners shared information about themselves through prompting from an adult). Children in both *experimental* conditions learned the same set of personal facts about their novel social partner (e.g., favorite foods), and we expected children in both *experimental* conditions would be more likely to connect than children in the *baseline* condition. However, we reasoned that *active* self-disclosure would allow children to uniquely signal an intention to connect with their novel social partner and boost their confidence in carrying out conversations, thereby leading to more positive interactions and feelings of connectedness than the *passive* condition (see Ladd, 1999). Therefore, active self-disclosure scaffolded by an adult may be particularly effective in situations where children do not connect spontaneously.

Compared to their connections with same-gender peers, children are less likely to spontaneously form connections with other-gender peers. Decades of social development research demonstrate that young children's friendships are highly gender-segregated (Halim et al., 2021; Maccoby, 1990; Rose et al., 2022; Shrum et al., 1988), and many children miss out on the unique benefits of cross-gender friendships (Fabes et al., 2019; Rose et al., 2022). Formal structures that encourage intergroup interactions are thought to be helpful in facilitating meaningful connections across group boundaries (Allport, 1954). To test this possibility directly, in all conditions of the present experiment, we counterbalanced whether children interacted in same-gender or different-gender pairs. This counterbalancing allowed for a comparison of factors that facilitate children's connections in situations where children often do not connect on their own (i.e., different-gender pairs) versus situations where children are more likely to connect without additional support (i.e., same-gender pairs).

Hypotheses

Altogether, the present design allows us to test the impact of adult scaffolding (*baseline* condition vs. *experimental* conditions), children's roles in getting to know one another (*active* condition vs. *passive* condition), pair gender composition (same gender vs. different gender), and interactions of these three factors in facilitating connections (measured both via adult ratings of children's connections and children's own ratings of their connections) between novel social partners. We hypothesized that different-gender pairs would be particularly unlikely to connect at *baseline* (Hypothesis 1); that the *experimental* conditions—both *active* and *passive*—would facilitate children's connections relative to *baseline* (Hypothesis 2); that the *active* condition would be particularly effective, even relative to the *passive* condition (Hypothesis 3); and that the patterns described in Hypotheses 1–3 would be evident in both adults' impressions of children's connections and children's subjective ratings of their own connections (Hypothesis 4).

Peer relationships become particularly important for children's well-being around kindergarten age (Criss et al., 2002; Rubin et al., 2006), yet elementary school children often struggle to form social connections, especially across gender-group boundaries (Halim et al., 2021; Leman et al., 2005; Rose et al., 2022). Moreover, although elementary school-aged children are able to interact with peers independently, children of this age also appear to benefit from adult support to promote positive peer interactions (e.g., Factor et al., 2022; Guedes et al., 2023; Hanish et al., 2021). Thus, the present study focused on 5- to 11-year-old children, who need relationships with peers for healthy development, yet may benefit from adult support to develop peer connections.

Method

Participants

Participants were 288 5- to 11-year-old U.S. children (144 pairs of children; M = 8.01 years; SD = 1.74 years; range = 5.03–11.01 years; female: n = 142, male: n = 145, nonbinary: n = 1; American Indian or Alaska Native: n = 2, Asian or Asian American: n = 24, Black or African American: n = 8, Hispanic or Latino: n = 17, White

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or Caucasian: $n = 200$, Multiracial: $n = 33$, other: $n = 2$, race information not provided: $n = 2$) recruited online through the existing recruitment databases of two universities located in the Midwestern United States. The sample size was determined by a combination of counterbalancing considerations and the result of an a priori power analysis using the modelPower function of R package lmSupport, which suggested that 135 pairs achieved a power of .80 to detect an effect size of $\eta_p^2 = 0.06$ or larger.

Each participant was paired with another participant of the same age in years whom they had never met before. Half of the children were paired with another child of the same gender ("same-gender" pairs, $n = 144$ participants), and the other half of the children were matched with another child of a different gender ("different-gender" pairs, $n = 144$ participants). Pairs were randomly assigned to one of three conditions: *baseline* ($n = 96$ participants), *active* ($n = 96$ participants), or *passive* ($n = 96$ participants).

Aside from the 144 pairs of children included in analyses, 31 additional pairs were tested but excluded from analyses due to parent or sibling interference with the study procedures ($n = 27$); experimental error ($n = 1$); at least one member of the pair dropping out of the study during the *free talk* phase ($n = 1$); or at least one member of the pair participating in the study previously ($n = 2$). Additional information about participant demographics and exclusion criteria are provided in Supplemental Material.

Procedure

Participants' parents provided written informed consent, and the protocol was approved by the institutional review boards of both universities. Participation took place entirely on the Zoom video conferencing platform, and each session involved two participants (Child A and Child B) and two experimenters (Experimenter A and Experimenter B). Experimenter scripts and study materials are provided on Open Science Framework (OSF; https://osf.io/2zgy6; King et al., 2024). The experiment unfolded across five distinct phases.

Setup Phase

All participants completed the *setup* phase first, regardless of condition assignment. Participants were admitted to the call from the waiting room one by one and assigned to separate breakout rooms with separate experimenters (i.e., Child A and Experimenter A were assigned to one breakout room, while Child B and Experimenter B were assigned to another breakout room) to configure their Zoom settings. This one-by-one assignment strategy ensured that participants did not see one another, even momentarily, prior to the *scaffolded encounter* phase (for participants in the *active* or *passive* conditions) or *free talk* phase (for participants in the *baseline* condition).

Learning and Scaffolded Encounter Phases

Baseline Condition. Participants in the *baseline* condition did not complete a *learning* phase or a *scaffolded encounter* phase.

Passive Condition. During the *learning* phase, participants in the *passive* condition learned that they would be working with the other participant in their pair (their "partner") to fill in the blanks of a story, but that to do so, they needed to answer a series of predetermined questions, which would be asked by Experimenter A. Then, participants learned about each predetermined question in turn by watching their experimenter (A or B) ask the questions to a cartoon monster, who responded with an answer; this provided a preview of the content of the questions, a demonstration of how questions would be asked by the experimenter, and a demonstration of how the questions could be answered. Predetermined questions were (a) What is your name? (b) What is your favorite food? (c) What do you like to play inside? (d) What do you like to play outside? (e) What is your favorite book or movie? and (f) What is your least favorite food?

During the *scaffolded encounter* phase, the participants left their separate breakout rooms to meet, for the first time, in the main room with the experimenters. During this phase, Experimenter B turned their camera off and muted themself, while Experimenter A asked the participants each of the questions they learned about during the preceding *learning* phase, one by one (e.g., "What are your favorite foods?"). After both participants finished answering a question, Experimenter A read the corresponding section of the story, using the participants' answers (e.g., "ice cream" and "pizza") to fill in the appropriate blanks. In the event that participants were silent or not following directions, the experimenter waited 10 s before repeating the prompt. In the *passive* condition, the *scaffolded encounter* phase took approximately 10 min to complete in total.

Active Condition. Participants in the *active* condition completed a similar *learning* phase to that in the *passive* condition with two key differences. First, instead of learning that they would later need to answer the predetermined questions asked by Experimenter A, participants learned that they and their partners would *ask each other* the predetermined questions. Second, rather than watching the experimenter ask the monster the questions, the experimenter told the participant what questions to ask the monster and participants asked the monster the questions themselves.

During the *scaffolded encounter* phase, the participants left their separate breakout rooms to meet, for the first time, in the main room with the experimenters. During this phase, Experimenter B turned their camera off and muted themself, while Experimenter A prompted the participants to ask one another the questions they learned about in the *learning* phase, one by one (e.g., "It's favorite foods time!"). After both participants finished asking and answering a question, Experimenter A read the corresponding section of the story, using the participants' answers to fill in the appropriate blanks. In the *active* condition, the *learning* and *scaffolded encounter* phases took approximately 11 min to complete in total (due to participants asking questions themselves, rather than the experimenter asking the questions).

Free Talk Phase

After completing the *setup* phase (for participants in the *baseline* condition) or the *scaffolded encounter* phase (for participants in *active* or *passive* conditions), participants moved on to the *free talk* phase. Participants were told that they could talk about whatever they wanted for a couple of minutes and that the experimenter would inform them when the time was up. Both experimenters turned their cameras off and muted their microphones. After 2 min passed, Experimenter A turned on their camera, unmuted themself, and told the participants that their time was up and they were done talking to each other for the day.

Child-Reported Measures Phase

After the *free talk* phase, participants returned to their separate breakout rooms with the same experimenter who had guided them through the *setup* phase (and *learning* phase, if applicable) earlier in the experiment. In the breakout room, the experimenter shared their screen to guide children through the *child-reported* measures. All participants completed all measures. The Future Interest measure was always presented first, and the remaining measures were presented in one of two orders (Liking, Fun, Closeness, Approach Intentions, Sharing; or the reverse). Children within a pair completed measures in the same order.

Measures

Free Talk Measures

Coder-Rated Connection. Two adult coders unaware of condition assignment rated video recordings of participants' *free talk* interactions on five 5-point Likert scale items intended to capture social connection. Lay adults are skilled at estimating people's connections based on behavioral observations in live or virtual contexts (e.g., by observing differences in length of pauses between utterances; Templeton et al., 2022; affect: Kurtz & Algoe, 2017; or gaze: Breil & Böckler, 2021), thus we reasoned that adult coders would incorporate their observations of many affiliative cues when forming their impressions. Video recordings were not available for eight pairs because one of the parents in each of those pairs did not give permission to video-record their child. Thus, results on coder-rated connection are based on the 136 pairs for whom video recordings were available.

Coders rated children's *free talk* interactions on five dimensions of connection (Global Success; Connection; Comfort; Fun; Talkativeness) using 5-point Likert scales with higher values indicating greater perceived connection. Coder agreement was moderate or better for all measures (weighted κs: Connection = .55; Comfort = .58; Fun = .60; Global = .76; Talkativeness = .83), and ratings across measures were highly correlated (Pearson *r*s > 0.83). Thus, both coders' ratings of the five measures were averaged to form a single composite score per participant pair.

Word Counts. Videos of *free talk* phase interactions were transcribed by adult coders unaware of condition assignment, and word counts were generated using Linguistic Inquiry and Word Count-22 software (Boyd et al., 2022).

Child-Reported Measures

We selected six *child-reported* measures based on tasks used across the developmental science literature to probe children's connection to, and interest in friendship with, peers (including peers from different social groups; e.g., Laursen & Hartup, 2002; Pauker et al., 2022; Shutts et al., 2016; Xiao et al., 2022). Given the novelty of the present experimental approach and conceptual complexity of human connection, it is unclear precisely how children's thinking about peer connections may vary based on experiencing this brief experimental manipulation. For example, the manipulation could impact some aspects of connection (e.g., interest in spending time together) more than others (e.g., feelings of warmth). Thus, we included a range of measures that could reasonably capture different manifestations of children's feelings of connection with a peer. Specifically, the selected measures probe children's subjective feelings of connection (liking, fun, and closeness) and interest in maintaining or expanding upon their novel peer relationship (future interest, approach intentions, and sharing). All measures required participants to verbally indicate their response. All experimenter materials and dependent measures are available on OSF (https://osf.io/2zgy6; King et al., 2024).

Closeness. Participants rated how close they felt to their partner on a vertical 5-point scale depicting two cartoon people with decreasing amounts of space between them (*not close at all* = 0; *kind of not close* = 1; *in-between* = 2; *kind of close* = 3; *really close* = 4).

Fun. Participants rated how much fun they had with their partner on a horizontal 5-point scale that depicted increasingly large circles to represent bigger amounts of fun (*not fun at all* = 0, *kind of not fun* = 1, *just okay* = 2, *kind of fun* = 3, *really fun* = 4).

Liking. Participants rated how much they liked their partner on a 5-point scale that depicted smiley faces with increasingly positive expressions to represent greater liking (*do not like at all* = 0; *kind of do not like* = 1; *just okay* = 2; *kind of like* = 3; *really like* = 4).

Future Interest. Participants indicated their interest in a future interaction with their partner on a 3-point scale: prefer to talk to a novel unidentified child who was ostensibly in the *waiting room* (0), prefer to talk to their *same partner again* (2), or *not sure* (1).

Approach Intentions. Participants were asked whether they would like to approach their partner outside of the study session (*no* = 0; *not sure* = 1; *yes* = 2) in five scenarios: saying hello to the partner at school, letting the partner play with their friends together at lunchtime or recess, sharing their colored pencils with their partner, inviting their partner to play at their house after school, and sharing a secret with their partner. Scores for the five scenarios were summed to create an overall score, which could range from 0 (*all "no" answers*) to 10 (*all "yes" answers*).

Sharing. Participants indicated how many "funny pictures" they would like to share with their partner. Participants saw one example picture of a dog driving a car alongside four "hidden" pictures, represented by black rectangles. Participants indicated how many of the four hidden pictures they wished to share with their partner.

Unlike the adult-rated *free talk* measures, child-reported measures were not consistently correlated (Pearson *r*s ranged from .10 to .66). Thus, child-reported measures were analyzed individually. Because pairs were the unit of analysis (see Analysis Strategy below), the two participants' scores were averaged within each pair to generate one score per pair for each child-reported measure.

Transparency and Openness

Hypotheses, methods, and analyses were preregistered prior to the start of data collection. Study preregistration, materials, data, and analysis scripts are available on OSF at https://osf.io/2zgy6 (King et al., 2024).

Results

Analysis Strategy

All analyses were conducted in R. Hypotheses were tested by estimating regression models for each dependent variable (i.e.,

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coder-rated connection, number of words spoken during the *free talk* phase, and the six child-reported measures). Models treated pairs as the unit of analysis, examining (a) effects of pair gender composition in the *baseline* condition; (b) whether the two *experimental* conditions (collapsed) differed from the *baseline* condition; (c) whether the two *experimental* conditions (*active* and *passive*) differed from one another; and (d) interactions between condition effects and pair gender composition. Further details about the analysis strategy are provided in Supplemental Material. See Figures 1–3 for plots of coder-rated connection (Figure 1), word counts (Figure 2), and child-reported dependent measures (Figure 3) by condition and pair gender composition.

Baseline Condition

We first examined children's connections at *baseline*. As hypothesized, same-gender pairs were more likely to connect than different-gender pairs in the *baseline* condition (H1). This pattern was evident in both coder ratings and children's own ratings of connection (H4). In particular, same-gender pairs in the *baseline* condition were rated by adult coders as appearing more connected during the *free talk* phase, $b = .59$, $F(1, 132) = 4.03$, $p = .047$, $\eta_p^2 = .03$, 95% CI [0.01, 1.18], and children in those pairs reported having more fun during the session, $b = .48$, $F(1, 140) = 5.16$, $p = .025$, $\eta_p^2 = .001$, 95% CI [0.06, 0.90], and expressed higher interest in future contact with their partner, $b = .44$, $F(1, 140) = 6.93$, $p = .009$, $\eta_p^2 = .02$, 95% CI [0.11, 0.77], than different-gender pairs in the *baseline* condition. No other simple effects of gender in the *baseline* condition were significant ($ps > .071$).

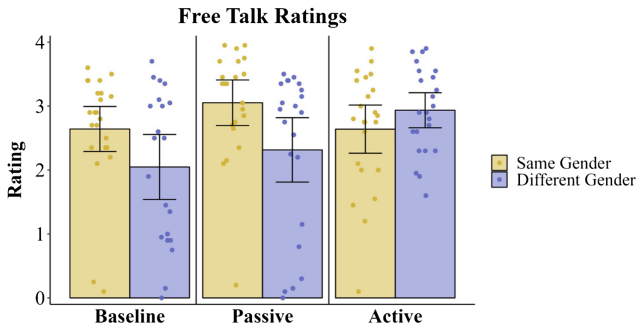
Baseline Versus Experimental Conditions

Next, we tested the hypothesis that children whose interactions were scaffolded (*experimental* conditions) would become more connected than children who interacted without support (*baseline* condition). As expected, pairs of participants in the *experimental* conditions appeared and felt more connected than pairs in the *baseline* condition (Hypothesis 2), and again this pattern was evident in both adults' observations and children's ratings (Hypothesis 4). The number of words spoken by children during the *free talk* phase was larger in the *experimental* conditions (*passive* condition $M = 148$, $SD = 77.00$; *active* condition $M = 153$, $SD = 63.90$) than the *baseline* condition ($M = 113$, $SD = 66.00$), $b = 13.05$, $F(1, 130) = 10.03$, $p = .002$, $\eta_p^2 = .07$, 95% CI [4.90, 21.20]. Coder-rated connection was also higher for pairs in *experimental* conditions than pairs in the *baseline* condition, $b = .13$, $F(1, 130) = 4.84$, $p = .030$, $\eta_p^2 = .03$, 95% CI [0.01, 0.25], and children in *experimental* conditions liked their novel social partner more, $b = .09$, $F(1, 138) = 5.72$, $p = .018$, $\eta_p^2 = .04$, 95% CI [0.02, 0.16], had more fun during the session, $b = .19$, $F(1, 138) = 20.21$, $p < .001$, $\eta_p^2 = .13$, 95% CI [0.11, 0.28], and felt closer to their novel social partner, $b = .16$, $F(1, 138) = 10.19$, $p = .002$, $\eta_p^2 = .07$, 95% CI [0.06, 0.26], than children in the *baseline* condition. Other child-reported measures did not reveal scaffolding main effects ($ps > .242$).

The impact of scaffolding on children's connections also varied based on pairs' gender composition in children's ratings of how much fun they had during the session, $b = -0.21$, $F(1, 138) = 6.10$, $p = .015$, $\eta_p^2 = .04$, 95% CI [−0.38, −0.04], and their interest in future contact with their partner, $b = -0.14$, $F(1, 138) = 4.36$, $p = .039$, $\eta_p^2 = .03$, 95% CI [−0.28, −0.01]. Although we observed gender in-group favoritism in children's ratings of how much fun they had during the session and their interest in future contact with their partner (see above), this gender in-group favoritism was not observed in the active condition, fun: $b = -0.35$, $F(1, 140) = 2.64$, $p = .107$, $\eta_p^2 = .001$, 95% CI [−0.79, 0.08]; future contact: $b = .04$, $F(1, 140) = .06$, $p = .805$, $\eta_p^2 = .02$, 95% CI [−0.29, 0.37], or the *passive* condition, fun: $b = .04$, $F(1, 140) = .03$, $p = .856$, $\eta_p^2 = .001$, 95% CI [−0.41, 0.49]; future contact: $b = -0.02$, $F(1, 140) = .02$, $p = .901$, $\eta_p^2 = .02$, 95% CI [−0.35, 0.31]. Other dependent measures did not reveal interactions between scaffolding and pair gender composition ($ps > .234$). Overall, these results suggest that children in *experimental* conditions were more likely to connect than children in the *baseline* condition—especially across gender group boundaries (Hypothesis 2).

Figure 1

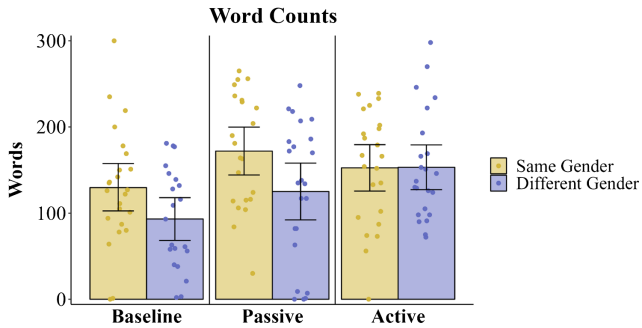
Coder-Rated Connection Results by Condition and Pair Gender Composition



Note. Larger values indicate a greater perceived connection. Bars depict means, error bars depict 95% confidence intervals, and small dots depict individual ratings ($N = 136$). See the online article for the color version of this figure.

Figure 2

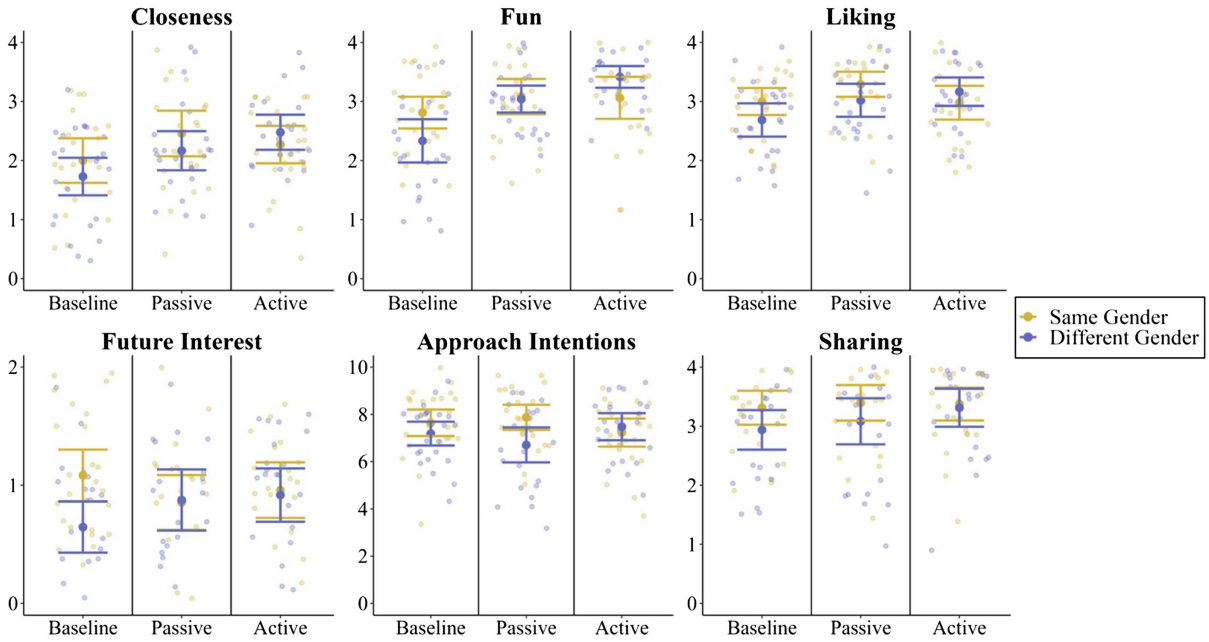
Words Spoken During the Free Talk Phase by Condition and Pair Gender Composition



Note. Bars depict means, error bars depict 95% confidence intervals, and small dots depict pair word counts ($N = 136$). See the online article for the color version of this figure.

Figure 3

Child-Reported Dependent Measures Results by Condition and Pair Gender Composition



Note. Large dots depict means, error bars depict 95% confidence intervals, and small dots depict pair responses ($N = 144$). See the online article for the color version of this figure.

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Active Versus Passive Experimental Conditions

Finally, we tested our hypothesis that children would be more likely to connect in the *active* condition than the *passive* condition. There were no main effects of *active* versus *passive* conditions on pair connection (*p*s > .235). However, outcomes for children in the *active* versus *passive* conditions varied based on pair gender composition both for coder-rated connection during the *free talk* phase, $b = -0.52$, $F(1, 130) = 6.40$, $p = .013$, $\eta_p^2 = .05$, 95% CI [−0.92, −0.11], and children's ratings of their approach intentions, $b = -0.71$, $F(1, 138) = 5.56$, $p = .020$, $\eta_p^2 = .04$, 95% CI [−1.30, −0.11]. In particular, children in the *passive* condition demonstrated in-group favoritism in their connections: Same-gender pairs appeared more connected, $b = .74$, $F(1, 132) = 6.12$, $p = .015$, $\eta_p^2 = .03$, 95% CI [0.15, 1.33], and expressed greater approach intentions, $b = 1.17$, $F(1, 140) = 7.57$, $p = .007$, $\eta_p^2 = .02$, 95% CI [0.33, 2.00], than different-gender pairs. However, no such in-group favoritism was observed in the *active* condition: There were no differences between same-gender and different-gender pairs' coder-rated connection, $b = -0.30$, $F(1, 132) = 1.05$, $p = .306$, $\eta_p^2 = .03$, 95% CI [−0.87, 0.27], or children's ratings of their approach intentions, $b = -0.25$, $F(1, 140) = .35$, $p = .557$, $\eta_p^2 = .02$, 95% CI [−1.09, 0.59]. There were no other significant interactions between pair gender composition and *active* versus *passive* conditions (*p*s > .082). Overall, these results suggest that the *active* condition was particularly effective at facilitating connections across gender groups (Hypothesis 3).

In sum, the results provide empirical support that (Hypothesis 1) at *baseline*, children interacting in different-gender pairs are less likely to connect than children interacting in same-gender pairs; (Hypothesis 2) scaffolding from an adult can indeed facilitate connections between children meeting for the first time, particularly children of different genders; (Hypothesis 3) between the two scaffolding methods tested here, *active* self-disclosure was particularly effective for eliminating gender in-group favoritism in pairs' connections; and (Hypothesis 4) the hypothesized effects are reflected in both adult observations and children's self-reports. Moreover, the effects—although modest—were observed across several measures designed to capture unacquainted children's budding social connections, providing converging evidence of factors that foster this process.

Supplemental Analyses

In addition to the above analyses that test the preregistered hypotheses, we also conducted exploratory analyses examining the effects of pair age, race/ethnicity composition, and personality. Older children connected better than younger children overall, coder-rated connection: $b = .17$, $F(1, 132) = 3.99$, $p = .047$, $\eta_p^2 = .08$, 95% CI [0.002, 0.33], but age effects did not vary by condition (*p*s > .09). We observed no differences in connection between pairs who shared the same racial/ethnic identity and pairs whose racial/ethnic identities differed (based on parent-reported demographics; all *p*s > .09). However, many people's racial and ethnic identities do not align with others' initial *perceptions* of their identities (e.g., Khanna & Johnson, 2010); thus, we also conducted exploratory analyses based on adult coders' *perceptions* of children's identities. Here, we found that pairs of children perceived as belonging to the same racial/ethnic group ($n = 91$ pairs) connected better than pairs of children perceived as belonging to different racial/ethnic groups, $n = 45$ pairs; coder-rated connection: $b = .77$, $F(1, 132) = 6.46$, $p = .012$, $\eta_p^2 = .02$, 95% CI [0.17, 1.37], but this difference was less

pronounced in *experimental* conditions than in the *baseline* condition, $b = -0.26$, $F(1, 127) = 4.32$, $p = .040$, $\eta_p^2 = .02$, 95% CI [−0.51, −0.01]. Finally, analyses of personality suggest that pairs containing at least one extraverted child ($n = 107$ pairs) connected better than pairs consisting of two introverted children ($n = 34$ pairs; $ps < .048$), but this gap narrowed in *experimental* conditions relative to *baseline* ($ps < .03$). See Supplemental Material for complete descriptions of these analyses, as well as additional Supplemental Analyses examining pairs' conversation content, correlations between measures, boy–boy versus girl–girl same-gender pairs, and effects of prior familiarity with the primary experimenter. Overall, these Supplemental Analyses suggest that the conditions tested here were similarly effective across the included age range (5–11 years) and that the *experimental* conditions may hold promise for closing race- and personality-based gaps in children's connections. We return to these possibilities in the Discussion section.

We also conducted exploratory analyses focused on the amount of time participants spent completing the *learning* and *scaffolded encounter* phases (i.e., the phases that contained condition manipulations) in the *active* condition (in which child participants ask the questions themselves) versus the *passive* condition (in which the adult experimenter asks the questions). We recorded the duration (in seconds) of both phases from study session video recordings (*learning:* $M = 226$, $SD = 35$; *scaffolded encounter:* $M = 387$, $SD = 98$), which revealed that *active* condition pairs took approximately 23 s longer to complete the *learning* phase, and approximately 42 s longer to complete the *scaffolded encounter* phase, than *passive* condition pairs, *learning:* $b = 23.47$, $F(1, 176) = 22.37$, $p < .001$, $\eta_p^2 = .11$, 95% CI [13.68, 33.27]; *scaffolded encounter:* $b = 42.05$, $F(1, 87) = 4.18$, $p = .044$, $\eta_p^2 = .05$, 95% CI [1.19, 82.92]. Critically, the interactions between *experimental* condition (*active* vs. *passive*) and pair gender composition, described above in the primary results, replicated when controlling for time children spent together during the *scaffolded encounter* phase, coder-rated connection: $b = -1.17$, $F(1, 84) = 8.28$, $p = .005$, $\eta_p^2 = .09$, 95% CI [−1.97, −0.36]; approach intentions: $b = -1.37$, $F(1, 84) = 4.27$, $p = .042$, $\eta_p^2 = .05$, 95% CI [−2.68, −0.05], suggesting that these interactions do not stem solely from *scaffolded encounter* duration differences.

Discussion

Despite widespread interest in helping children form peer relationships—from caregivers, clinicians, policy-makers, educators, and scientists—there is currently limited empirical evidence regarding factors that directly affect children's ability to form new peer connections in real time. Previous studies provide helpful insights into correlates of positive peer relationships, but controlled experiments testing possible mechanisms of connection are sparse. The present research proposes a new experimental approach to fill these long-standing gaps.

Here, children's peer connections varied in response to a brief virtual manipulation of children's initial encounters. In alignment with classic findings showing that children favor same-gender peers on average, children of the same gender connected more than children of different genders in the *baseline* condition. However, scaffolding initial encounters between children of different genders, especially in a manner that prompted children to take an active role in sustaining the interaction, eliminated children's gender in-group favoritism. These results provide empirical support for the utility of scaffolding children's active self-disclosure in facilitating interactions among children who may otherwise miss out on crucial opportunities to connect.

The findings of the present experiment contribute to theories of children's peer connections, providing causal evidence for scaffolding, self-disclosure, and active self-disclosure in particular as mechanisms that facilitate children's ability to form new peer connections. Prior work has documented common features of childhood friendships, such as sharing similarities and spending time together (see Newcomb & Bagwell, 1995 and Rubin et al., 2006 for review). Other research has observed the process of spontaneous friendship formation in naturalistic settings, finding that young children who "hit it off" engage in reciprocal communication and self-disclosure during their initial encounters (e.g., Gottman & Graziano, 1983). Building upon this prior work, the present study is among the first to experimentally test factors believed to facilitate children's ability to form new social connections.

The present study suggests that scaffolding from adults can help children connect. But, critically, in a situation where children typically do not connect—here, across gender group boundaries—children in the present experiment were more likely to connect when they took an active (vs. passive) role during their initial encounter with a novel peer. Indeed, the *passive* condition of the present experiment, in which an experimenter directed children to take turns disclosing personal facts (e.g., favorite foods), was less effective at helping children connect with different-gender peers compared to a matched *active* condition in which children took turns disclosing personal facts without direct prompting from the experimenter. In both *experimental* conditions, children were aware that the questions were predetermined. Why, then, did the *active* condition elicit stronger connections, particularly among children interacting across gender group boundaries?

Potential Mechanisms

It is possible that compared to the *passive* condition, the *active* condition improved children's confidence in interacting with outgroup peers. Although both *experimental* conditions provided children with an experience of reciprocal self-disclosure, the *active* condition uniquely prompted children to take personal responsibility for moving the interaction forward. This scaffolded experience may have improved children's self-efficacy in their ability to communicate with other-gender peers (Bandura, 1982; Wheeler & Ladd, 1982). Indeed, in domains ranging from academics to athletics, people who conquer a challenging activity with support (i.e., scaffolding) subsequently feel more confident engaging in the activity independently and are more likely to succeed at the activity in the future as well (see Bandura, 1982 and Schwarzer, 1992 for review). Future research could test this potential mechanism in the context of the present experiment by measuring children's feelings of self-efficacy about interactions with peers in general, and peers of different genders in particular, following the condition manipulation (e.g., via the Children's Self-Efficacy for Peer Interaction Scale; Wheeler & Ladd, 1982).

As an alternative potential pathway, children in the *active* condition may have received particularly strong evidence against the assumption that other-gender peers do not want to get to know them. Children are more likely to anticipate being excluded by other-gender peers than same-gender peers, and children who expect these

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asymmetric outcomes are less likely to form friendships with peers outside of their gender ingroup; further, these asymmetric expectations and friendship patterns are particularly evident among children who feel more uncertain about how to communicate with other-gender peers (e.g., "knowing how to talk to" other-gender peers; Zosuls et al., 2014). To facilitate children's connections with other-gender peers, it may be necessary to disrupt this assumption-avoidance cycle (see Shelton & Richeson, 2005 for a discussion of a similar possibility in the race/ethnicity domain). If children tend to assume that other-gender peers are uninterested in getting to know them at baseline, then the *active* condition may have uniquely disrupted these assumptions by providing an interaction with an other-gender peer during which both children appeared motivated to get to know one another (i.e., by asking and answering questions). Future research could test this possible mechanism by measuring children's beliefs about outgroup peers' interest in getting to know them following the intervention. If children in the *active* condition were found to be more optimistic about outgroup peers' interest, this would suggest that disrupting children's assumptions about gender outgroup peers' interest in connecting may have been a contributor to the condition effects observed here.

Broader Implications

The present experiment lays the groundwork for accelerated theoretical advancement in the developmental science of peer connection. Here, we establish a paradigm with which theories of children's peer relations may be rigorously tested in a controlled setting—specifically, arranging young children's interactions with novel social partners on a virtual platform. This paradigm eliminates much of the noise inherent to applied studies (e.g., prior familiarity of participants; confounds due to lack of experimental control), thus allowing for more highly controlled tests of factors influencing children's connections than previously possible. Using this methodology, future studies can test the causal relevance of other factors featured in prominent theories of early peer relationships, such as cooperation (e.g., Warneken et al., 2006), perceived similarity (e.g., Laursen, 2017), perspective-taking (e.g., LeMare & Rubin, 1987), and synchrony (e.g., Rabinowitch & Knafo-Noam, 2015). These factors may be critical for promoting peer connections among children in their natural environments, yet isolating these factors may be impossible outside of controlled laboratory experiments.

Scaffolding children's initial encounters helped pairs of children form an immediate peer connection with only minimal time and effort, illuminating new prospects for interventions to improve children's social lives. Existing interventions that help children connect across gender group boundaries are often implemented over the course of several days or weeks, such as through long-term changes to children's classroom environments (e.g., the "Buddy Up" program; Hanish et al., 2021). Thus, it is encouraging that the present intervention facilitated children's connections with a manipulation that lasted only 10 min. Additionally, the consistent patterns of effects observed across different dependent measures suggest that these manipulation effects are reliable, albeit modest in size.

The short-term impacts of the present experiment are encouraging; however, we cannot know for sure what the long-term effects of this brief manipulation may be, constraining the generalizability of the present findings to children's initial formation of peer connections. It is possible that with additional opportunities to connect, children who experienced different conditions of the present experiment would not vary in their ability to maintain their budding connections. Indeed, research in naturalistic contexts suggests that the process of becoming friends differs from the process of maintaining existing friendships (e.g., Gottman & Graziano, 1983; Rubin et al., 2006). However, the skills children practiced in the present intervention—reciprocal communication, self-disclosure—emerge naturally in both contexts (Gottman & Graziano, 1983; Ladd et al., 1996; see Rubin et al., 2011 for review). Thus, if this short intervention improved children's confidence about engaging in peer interactions, then these short-term gains in children's connections may persist over time.

Forming even a single peer connection provides continued opportunities for children to practice their social interaction skills and, in turn, puts children in a better position to forge additional peer connections in the future—in other words, children's peer connections may snowball over time (see Walton & Wilson, 2018 for discussion of self-reinforcing interventions). First impressions are also "sticky" (e.g., Tetlock, 1983), further supporting the possibility that an initial connection formed via the present paradigm could grow with repeated contact. If it is indeed possible that a simple manipulation such as the one tested here could launch children into a self-reinforcing cycle of connection with peers, this could transform the way educators, clinicians, and scientists approach childhood peer relations interventions. In a time where children struggle to connect perhaps more than ever in recent history (e.g., Aral & Kadan, 2023; Wong et al., 2020), simple yet effective interventions will be essential to reach the growing number of children in need of support.

Limitations

The *baseline* condition of the present experiment differed from the *experimental* conditions in several ways, many of which cannot be disentangled in any single experiment. For example, the *experimental* conditions included adult scaffolding of a self-disclosure activity that involves cooperation, provides opportunities for synchrony, and allows *experimental* condition partners to spend more time together than the *baseline* condition partners. Thus, we cannot point to a single factor that caused the different results among pairs in the *baseline* versus the *experimental* conditions. Although we could have designed a *baseline* condition to better control for *some* of these differences—controlling for all of these differences at once would be impossible in a single condition—we decided that a *baseline* condition demonstrating what children do in the complete absence of support would be more useful. Such a condition mirrors many of children's everyday opportunities to connect; often, children occupy the same physical or virtual space, but are provided with no instructions or support to encourage connection. Nevertheless, differences between the *baseline* condition and *experimental* conditions leave open questions about which features of the *experimental* conditions were most instrumental in facilitating children's connections relative to *baseline*.

Importantly, the *active* and *passive* conditions were nearly identical and varied only in the framing of children's roles in the activity, thereby providing a particularly strong test of how children's *roles* in an adult-scaffolded activity impact their ability to connect. But, despite matching *experimental* conditions for content children received about one another, children in the *active* condition

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took longer to exchange this content than children in the *passive* condition. These differences are somewhat unsurprising: Compared to predetermined questions posed by a trained experimenter (as in the *passive* condition), children likely include more disfluencies and pauses when asking the same predetermined questions themselves (as in the *active* condition). Interactions between pairs' *experimental* condition assignment and gender composition replicated while controlling for time children spent together, suggesting that time alone does not explain differences in children's connections across gender group boundaries in the *active* versus *passive* conditions. However, future work could address the impact of time more directly, such as by including an additional version of the *active* condition that matches the *passive* condition in *duration* rather than *content* (since matching for both is not possible within a single condition).

The findings of the present experiment strongly supported the hypotheses, but not all of the child-reported measures reflected the predicted results. Most notably, hypotheses were never supported by the Sharing measure. It is possible that sharing something with a peer reflects prosocial intentions rather than feelings of connection per se and therefore does not vary based on the factors we examined in the present experiment. Furthermore, although other child-reported measures supported the hypotheses overall, the patterns of results varied somewhat between different measures (i.e., main effects of *experimental* condition and gender composition, and interactions between conditions and pair gender composition, were not observed for every measure). It is unclear what this variation represents. For instance, it is possible that the different measures capture different aspects of children's connections. Indeed, we included several measures for precisely this reason—that some aspects of children's connections may be more or less influenced by the experimental manipulation. It will be important for future research to investigate which types of child-reported measures are responsive to particular connection-promoting manipulations in order to better understand which aspects of children's connections are impacted by particular connection-promoting factors.

The *experimental* conditions of the present study helped elementary school-aged children connect, and these effects did not vary based on participant age. However, methods that help children connect with peers during elementary school may not be appropriate for children of other ages. Whereas elementary school children may build friendships through exchanging questions or sharing secrets (see Rubin et al., 2011), infants' and toddlers' friendships are play-based (Howes, 2009). Thus, prompting toddlers to engage in reciprocal self-disclosure may not foster their connections; instead, toddlers may benefit from opportunities to play alongside peers. Later in development, adolescents face unique challenges in peer contexts (e.g., the separation of peers into "clique" groups) and are more likely to rebel against authority than younger children (Bukowski et al., 1998; Smetana, 2005), which may limit their receptivity to an adult-scaffolded peer interaction. Thus, although the present *experimental* conditions helped elementary school-aged children forge new peer connections, additional research will be necessary to identify factors that help peers connect earlier or later in development.

Children in all conditions of the present experiment interacted on a virtual meeting platform under the supervision of adult experimenters. Although elementary school children are often supervised by adults (Morrongiello et al., 2011), children are also more likely to follow rules while being observed (vs. alone; Kochanska et al., 2001). Thus, children in the present experiment may have altered their behavior in the presence of an experimenter, such as by more closely adhering to norms of politeness compared to an unsupervised interaction. Virtual environments have become increasingly prevalent meeting spaces for young children over the past decade (e.g., Aral & Kadan, 2023; Kearney et al., 2022; Kousky, 2016), providing unique opportunities for children to connect with peers they may otherwise miss out on opportunities to connect with. However, virtual environments also have unique limitations: Children meeting virtually may experience connectivity disruptions or limited visibility of nonverbal signals (e.g., hand gestures), which could hinder communication (Paradisi et al., 2021). Children interacted virtually with adult supervision in all conditions of the present experiment; thus, it is unlikely that between-condition differences in children's connections were affected by these features of the design. Additional research will be necessary to determine whether the effects observed here replicate when children interact in person or without adult supervision.

Future Directions

The present manipulation is extraordinarily brief and simple compared to typical applied interventions. Nevertheless, there are open questions about the extent to which particular features of the present manipulations were essential to their success in helping children form peer connections. Children in the *experimental* conditions of the present study were led through an activity by an adult, and the literature supporting this decision implemented support from teachers (e.g., Hanish et al., 2021) or parents (e.g., Ladd, 1999) to facilitate children's peer connections. However, is the presence of an adult necessary in order for children to connect? It is possible that adult scaffolding is particularly effective because it provides an "institutionalized" opportunity for children to interact across group boundaries—that is, an intergroup interaction that is formally supported (Allport, 1954). Alternatively, it is possible that a peer could provide similar support. Indeed, influential peers have been found to be effective agents of social change. In one field-based intervention, providing seed groups of adolescents with antibullying training led to a reduction of student conflict over time in intervention schools compared to control schools, and influential peers were particularly effective at changing school norms (Paluck et al., 2016). It is possible that a socially competent peer could have been equally or more effective than an adult at scaffolding children's interactions in the present experiment, and future laboratory-based work in this domain should test the effectiveness of peer versus adult scaffolding directly to determine situations that benefit more from adult versus peer support.

The questions children answered during the self-disclosure activity also warrant further consideration. In selecting questions for the self-disclosure activity, we prioritized questions that would be age-appropriate; would be possible for children from a range of backgrounds to answer; and would include domains of interest to children. With these goals in mind, children answered questions about their favorite and least favorite foods, favorite indoor and outdoor activities, and favorite books or movies. It is possible that some of these questions were more impactful than others; for instance, foods are highly culturally and socially relevant (e.g., Liberman et al., 2016), and thus questions about food may have played a larger role in facilitating children's connections than other questions. Further, compared to adult studies using self-disclosure activities to promote the formation

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of social relationships (e.g., Aron et al., 1997; Page-Gould et al., 2008), the questions children answered here were less emotionally intimate. For example, the Fast Friends Paradigm includes questions such as "Do you have a secret hunch about how you will die?" and "When did you last cry in front of another person?" (Aron et al., 1997). We did not include especially emotionally charged questions in the present study, and in future work, it would be possible to vary the emotional intimacy of questions in the present paradigm. For instance, asking "What is your favorite food?" may be less emotionally charged than asking "What is your favorite food that your parent cooks for you?" Future work could directly compare children's connections following more versus less emotionally charged questions in order to determine whether children, like adults, become closer when sharing more deeply personal information compared to more superficial personal information.

The *active* condition of the present experiment was particularly effective at closing gender-based gaps in elementary school-aged children's connections, raising the exciting possibility that this method could help peers connect in other challenging contexts as well. For instance, children are more likely to connect with peers who share their racial/ethnic background (Aboud et al., 2003; Shrum et al., 1988), and shy children form fewer peer connections than outgoing children (Kalutskaya et al., 2015; Kokkinos et al., 2016). Supplemental Results suggest that the present method may indeed hold promise for helping children connect with peers in these contexts: Shy children and children perceived as belonging to different racial/ethnic groups were less likely to connect at baseline, but the *experimental* conditions narrowed these gaps. These results must be interpreted with caution, as the present study was only designed to compare children's connections with same- versus different-gender peers—for instance, White children were overrepresented compared to children from other racial/ethnic backgrounds. It will be essential for future tests of this paradigm to include participants from even more diverse backgrounds, which will provide a stronger test of factors that aid children's connections in intergroup contexts more broadly.

Conclusion

The social, academic, and clinical impacts of early peer connections can ripple throughout childhood and even into adulthood, suggesting that children's first peer relationships set the stage for the rest of their lives (e.g., Rubin et al., 2006; Wentzel, 2009). Recent history has seen unprecedented challenges in children's ability to form these crucial connections with peers (e.g., due to the pandemic; Aral & Kadan, 2023)—challenges which are only expected to proliferate in coming decades (e.g., due to climate change disasters, such as fires and floods; Kearney et al., 2022; Kousky, 2016). Using controlled experimental approaches to clarify mechanisms of early peer connection will not only improve the science of peer relations, but will also be essential for supporting the well-being of the youngest members of our society for the foreseeable future.

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