Journal of Experimental Child Psychology 234 (2023) 105707

Contents lists available at ScienceDirect

Journal of Experimental Child Psychology

journal homepage: www.elsevier.com/locate/jecp

Shared social groups or shared experiences? The effect of shared knowledge on children's perspective-taking

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ARTICLE INFO

Article history:
Received 13 July 2022
Revised 27 April 2023
Available online 2 June 2023

Keywords:
Social cognitive development
Perspective-taking
Shared knowledge
Theory of mind
Closeness–communication bias
Social groups

ABSTRACT

Although the ability to consider others' visual perspectives to interpret ambiguous communication emerges during childhood, people sometimes fail to attend to their partner's perspective. Two studies investigated whether 4- to 6-year-olds show a "closeness–communication bias" in their consideration of a partner's perspective in a communication task. Participants played a game that required them to take their partner's visual perspective in order to interpret an ambiguous instruction. If children, like adults, perform worse when they overestimate the extent to which their perspective is aligned with that of a partner, then they should make more perspective-taking errors when interacting with a socially close partner compared with a more socially distant partner. In Study 1, social closeness was based on belonging to the same social group. In Study 2, social closeness was based on caregiving, a long-standing social relationship with a close kinship bond. Although social group membership did not affect children's consideration of their partner's perspective, children did make more perspective-taking errors when interacting with a close caregiver compared with a novel experimenter. These findings suggest that close personal relationships may be more likely to lead children to overestimate perspective alignment and hinder children's perspective-taking than shared social group membership, and they highlight important questions about the mechanisms underlying the effects of partner characteristics in perspective-taking tasks.

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https://doi.org/10.1016/j.jecp.2023.105707

Introduction

During communicative interactions, both speakers and listeners often need to consider the other's perspective to communicate clearly. For example, a speaker might tell a friend to look to "their left" rather than simply to look "left" to reduce ambiguity in the possible locations the speaker could be referring to. Speaker perspective-taking allows for clear communication with fewer possible meanings for a listener to interpret. However, because a speaker does not have unlimited time or resources to spend in disambiguating each statement, communication becomes more efficient if the listener can also be relied on to take the speaker's perspective into account. If a colleague asked you to pass them "that pen," you may consider visual referential cues to your colleague's perspective (e.g., where they are looking or pointing) to help you disambiguate which pen your colleague is asking for. For the sake of efficiency, a speaker can often rely on the fact that they share some knowledge or common ground (Clark, 1996; Clark & Brennan, 1991) with a listener, which the listener can use to interpret a pared-down statement (Ackerman et al., 1990; Glucksberg & Krauss, 1967; Hanna et al., 2003; Heller et al., 2012; Keysar et al., 2000; Sperber et al., 2010).

There is ample evidence that adult listeners can use visual perspective-taking—considering what the speaker can and cannot see—to interpret ambiguous communication (Epley et al., 2004; Hanna et al., 2003; Heller et al., 2012; Keysar et al., 2000; Nadig & Sedivy, 2002; Tanenhaus et al., 1995). For example, when given an ambiguous instruction (e.g., "Move the triangle on top of the red one"), adult participants quickly and selectively focused on red items that the speaker could also see (Hanna et al., 2003), reducing the ambiguity of the speaker's instruction. Children can also make use of visual perspective-taking skills when communicating with others. When asked to help an adult retrieve "the toy," 24-month-olds gave the adult a toy that was mutually visible rather than a toy that only toddlers could see (Moll & Tomasello, 2006). Four-year-olds selectively explain pieces of information that are not visible to their partner in order to rectify the partner's false belief (Bass et al., 2019), and 5- and 6-year-olds show evidence of considering a partner's visual perspective from the earliest stages of language processing (Nadig & Sedivy, 2002). These studies suggest that even children can consider another person's visual perspective and use perspective-taking to guide their behavior in a communicative interaction.

Despite the communication benefits and early emergence of perspective-taking skills, both children and adults regularly make perspective-taking errors (Epley et al., 2004; Keysar, 2007; Keysar et al., 2003). A common task used to investigate perspective-taking errors is the director task (Epley et al., 2004; Keysar et al., 2000; Nadig & Sedivy, 2002; Nilsen & Graham, 2009). In one typical setup, the participant plays the role of the listener and sits on one side of a grid containing various objects, with the "director" (played by a confederate or another participant) on the opposite side of this grid (see Fig. 1 below in Study 1 for an example). Some of the objects are visible to both the participant and director, but others are occluded from the director's view and visible only to the participant. The director's role is to ask the participant to move items around in the grid. Importantly, there are often two or more items that fit the same description (e.g., a stuffed toy mouse and a computer mouse), meaning that a director's instruction (e.g., "Next, pick up the mouse") can be ambiguous to the participant. In these cases, the participant needs to take the director's perspective in order to select the mouse that the director was likely referring to (the one that the director could see). Adults and children regularly show initial egocentrism in their response to the ambiguous requests (i.e., by first looking toward the object that the director cannot see, often set up to be the more prototypical referential match), and children occasionally fail to inhibit this initial egocentrism and choose the object that is not in common ground (Epley et al., 2004). Even in adults, egocentrism may result in communication errors or slower responses (Barr, 2008; Epley et al., 2004; Keysar & Barr, 2002; Savitsky et al., 2011), including an incorrect response to an ambiguous request (Savitsky et al., 2011) or taking longer to process an ambiguous request (Barr, 2008).

Which factors modulate perspective-taking accuracy in the director task? One study found that adults and 4- to 12-year-old children showed similar initial egocentrism in their responses to ambiguous requests (i.e., adults and children were equally likely to first look toward the object only they

Fig. 1. Two example grids, shown from the participant's perspective, with target objects highlighted in green and distractor items highlighted in red. Yellow occluders prevent the game partner playing the role of director from seeing certain objects. Examples: scalar perspective-taking trial: left grid—"Move the small candle [to location]"; right grid—"Move the small truck [to location]"; two-object perspective-taking trial: left grid—"Move the dinosaur [to location]"; right grid—"Move the book [to location]"; no perspective-taking trial: left grid—"Move the clock [to location]"; right grid—"Move the shoe [to location]." (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

could see), but adults showed better correction than children (i.e., by later selecting the item that is in common ground) (Epley et al., 2004). Another study suggested that 5- and 6-year-old children selectively fixate on items that are mutually visible from the earliest stages of language processing (Nadig & Sedivy, 2002), showing an efficient consideration of referential cues to their partner's perspective. Although some researchers originally argued that children's perspective-taking errors were due to an inability to consider the other's perspective (e.g., Mossler et al., 1976; Piaget & Inhelder, 1956), the findings that young children (Fan et al., 2015; Nadig & Sedivy, 2002; Nilsen & Graham, 2009) and infants (Liberman et al., 2017; Moll & Tomasello, 2006) show better perspective-taking in some contexts may make it more likely that perspective-taking accuracy fluctuates or is context dependent.

Because perspective-taking is an effortful cognitive process, some have suggested that adults adjust their levels of perspective-taking based on a perceived need for perspective-taking. Levels of perspective-taking could either be adjusted away from an egocentric default (Keysar & Barr, 2002) or titrated based on a rational weighting of the necessity of perspective-taking for understanding communication against the relative cognitive cost of actually considering someone's perspective (Hawkins et al., 2021). There is some evidence of individual and cultural differences in a tendency to perceive higher need for perspective-taking. For instance, adults from a culture with more emphasis on interdependence showed more perspective-taking in a communication game than adults from a culture with more emphasis on independence (Wu & Keysar, 2007a). In the same vein, infants, children, and adults with social exposure to more than one language showed better communicative perspective-taking than monolinguals (Fan et al., 2015; Liberman et al., 2017; Navarro & Conway, 2021). Regularly taking the perspectives of different social partners (e.g., recognizing that people who speak different languages understand different things) may facilitate a general increase in individuals' perceived need for persepctive-7taking in their interactions.

There are also features of the immediate social context that influence adults' perceived need to take their communicative partner's perspective—for example, whose perspective they need to take. Adults show a tendency to relax their visual perspective-taking, and are more likely to rely on privileged information, when interacting with a socially close communication partner (a friend relative to a stranger; Savitsky et al., 2011) or a partner with whom they share more background knowledge

(Wu & Keysar, 2007b). When adults completed the director task with a close friend, for example, they took longer to make decisions and selected more incorrect items compared with completing the same task with a stranger (Savitsky et al., 2011). Savitsky et al. (2011) posited a "closeness–communication bias" in adults; when communicating with a close partner with whom they have extensive shared knowledge (e.g., a friend, a spouse), adults assume that perspectives are more likely to be aligned and therefore feel less need to engage in perspective-taking. This bias could reduce the cognitive cost of communication without compromising effectiveness as long as perspectives are in fact aligned (Fussell & Krauss, 1989; Greenaway et al., 2015; Todd et al., 2016), but when there is a mismatch in perspectives it can lead to inaccuracies (Savitsky et al., 2011; Simpson & Todd, 2017; Wu & Keysar, 2007b). Wu & Keysar (2007b) provided further evidence that shared knowledge is a likely mechanism by which social closeness leads to relaxing perspective-taking; adults showed less perspective-taking in the director task when they had a partner with whom they had more shared knowledge (knowledge of the names of the novel shapes used in the task) relative to a partner with whom they had less shared knowledge, even though both partners were people they had just met.

Could social closeness and shared knowledge be factors in explaining variability in children's perspective-taking as well? Children can reason about shared knowledge as young as 2 or 3 years. Children use their shared experiences with social partners, such as considering what they have seen or learned about together, in order to make inferences about shared knowledge and communicate accordingly (Köymen et al., 2014, 2016). For example, 3- and 5-year-olds explain the needs of a novel animal to a peer who did not learn about the novel animal, but they do not do so if both they and the peer learned about the animal's needs (Köymen et al., 2016). Children and adults can also infer shared knowledge with individuals who they have never met. Shared social group membership can suggest shared knowledge, such as when two people who are from the same place share knowledge of local landmarks, slang, and/or even cultural songs and dishes (see Isaacs & Clark, 1987; Liberman et al., 2016; Liebal et al., 2013; Shutts et al., 2013; Soley & Aldan, 2020). By 5 years of age, children use social group markers like language and group labels to selectively infer shared cultural knowledge (Goldvicht-Bacon & Diesendruck, 2016; Liberman et al., 2020; Soley, 2019; Soley & Aldan, 2020), and both children and adults adjust their communication with others to account for both shared cultural and personal knowledge (Baer & Friedman, 2018; Goldvicht-Bacon & Diesendruck, 2016; Isaacs & Clark, 1987; Köymen et al., 2016). Given the evidence that children consider social closeness and shared knowledge when communicating with others, we were interested in whether children, like adults, would show a closeness–communication bias in their perspective-taking.

In Study 1, based on the evidence that children infer shared cultural knowledge between social group members, we manipulated social closeness via shared social group membership. We predicted that children would be more likely to assume that their perspectives are aligned with an ingroup member and therefore would be more likely to relax their perspective-taking and make egocentric errors in the director task than when interacting with an outgroup member. In Study 2, we looked for evidence of a closeness–communication bias via personal relationships (analogous to Savitsky et al., 2011, in adults) by comparing children's perspective-taking between a close caregiver with whom they share a kinship bond and a stranger they had just met.

Study 1

In Study 1, each child participated in the director task (Epley et al., 2004; Fan et al., 2015; Keysar, 2007; Nadig & Sedivy, 2002; Savitsky et al., 2011) with two partners. The ingroup partner was described as similar to the children in ways that would suggest shared social group membership (shared location, shared knowledge, and shared accent), and the outgroup partner was described as dissimilar on these same dimensions (different location, different knowledge, and different accent). Previous work has shown that young children treat accent and geography as markers of social group membership (Floccia et al., 2009; Kinzler et al., 2011; Weatherhead et al., 2018a, 2018b), show preferences for (e.g., Dunham et al., 2011; Kinzler et al., 2007) and feel closer to (Sani & Bennett, 2009) their ingroup members, and expect even strangers who belong to the same group to share cultural knowledge (Liberman et al., 2020; Liebal et al., 2013; Soley & Aldan, 2020). We wanted to explicitly highlight that the ingroup partner had some shared cultural knowledge (and the outgroup partner did not), so children were told that the similar partner had knowledge of a TV show well known to children of this age range. If children expect a social group member to share more of their knowledge than an individual from a different social group, they may feel less need to consider the ingroup member's perspective in behaving more egocentrically (making more perspective-taking errors) when interacting with the ingroup member.

Method

Participants

A total of 48 children participated in the current study; 24 4-year-olds (*M* = 54.65 months, *SD* = 3.32, range = 48.23–59.37; 12 boys) and 24 6-year-olds (*M* = 76.85 months, *SD* = 3.43, range = 72.5–82.83; 12 boys). We chose to test 24 4- and 6-year-old children based on the sample size used in previous studies using a similar task (e.g., Fan et al., 2015). Prior studies suggest that children can understand the structure and rules of the director task in this age range but may show variability in their perspective-taking skills (Nadig & Sedivy, 2002; Nilsen & Graham, 2009). We planned to look for age differences between 4- and 6-year-olds as well as the primary evidence for a closeness–communication bias (more egocentrism with the ingroup partner). All participants were typically developing children from a mid-sized city in New Zealand recruited from a database of families interested in participating in research on child development. Data were collected between May 2017 and July 2018. An additional 3 children were excluded for failing the memory check (*n* = 1), experimenter error (*n* = 1), or choosing to stop participating in the study (*n* = 1). The majority of the children who participated were exposed to a second language but were not bilingual (*n* = 45), and very few were monolingual (*n* = 3).[1]

Stimuli and design

Participants were asked to move items commonly known to young children (e.g., a cup, a toothbrush, a ball) around a 4 × 4 shelf (16 slots; see Fig. 1). Four slots in the shelf were occluded from one side. The items were in a fixed position in the shelf across children, but the order of the item sets and the items that each partner asked for were counterbalanced, such that each target item was sometimes requested by the similar partner and sometimes requested by the dissimilar partner. We also counterbalanced the order of the partners, such that half the participants played with the similar partner first and half played with the dissimilar partner first.

Procedure

Children participated in the director task in our lab testing room, with our procedure closely modeled on the procedure of Fan et al. (2015) (study script available on the Open Science Framework [OSF] at https://osf.io/kru39/), who also examined perspective-taking in 4- to 6-year-old children. Children were brought into the testing room and sat on a child-sized chair in front of the shelf. The experimenter explained the purpose of the game, namely that the goal was to move the objects inside the shelf to their correct places. Children then participated in a practice trial where they gave the experimenter instructions on how to move things around the shelf first. Children were taken to the far side of the shelf and asked to put on dark sunglasses. The game partners (adult confederates) later wore these sunglasses during the task to avoid any eye gaze cueing, so this step ensured that all children knew that the shelf could be seen through the sunglasses. In the first two practice trials, children were instructed to ask the experimenter to move a singular object with no competitor to a different location inside the shelf (above, under, or next to another object; e.g., "Put the paint above the butterfly"). On the third and fourth practice trials, children were instructed to ask for an object of which they could see one, but the experimenter could see two. The experimenter always selected the object that was not visible to the children to demonstrate an egocentric error. The experimenter asked whether the object was correct and, when children objected, guided them to repeat the instruction. The experi-

[1] Because most children had the same language background, we were unable to investigate the impact of language background on perspective-taking.

menter would then move the correct object. The experimenter told children explicitly that some of the squares were blocked, meaning that from one side of the shelf some of the items were hidden.

Children were then told that they were going to be introduced to their two game partners. The two partners were brought into the testing room individually and sat on child-sized chairs opposite the children. When they were not playing the game, the partners were instructed to read a book that had been placed on their chair. During the consent procedure, the experimenter asked caregivers to record where their children lived and to select a TV show or book that they regularly watched or read and enjoyed from a provided list of popular books and shows in New Zealand. During the study, the experimenter asked each partner where they were from and whether they knew about the TV show or book that the children enjoyed. The ingroup partner spoke in a New Zealand accent, said that she lived in the same city as the children, knew about the same TV show or book as the children, and provided a fact about that show or book for validation. The outgroup partner spoke in a foreign accent (southern U.S. accent), said that she lived in a different country (Disentis), said that she did not know about the TV show or book that the children knew but did know about another TV show or book (Obwah), and provided a novel fact about that TV show or book. The experimenter asked children to point to the partner who lived in each place as a memory check. If children failed this memory check, the introduction process and memory check were repeated until children answered correctly. One child failed this memory check three times and so was excluded from the sample.

Children completed two grids (6 trials) with each partner; each of the two grids contained 1 non-perspective-taking trial with no competitor object, 1 perspective-taking trial with a duplicate object occluded from their partner's view, and 1 scalar perspective-taking trial where the partner requested the "small" object and the smallest object was occluded from their partner's view (see Fig. 1). Partners kept neutral expressions throughout the study and looked to the center of the shelf. If children had questions during the test trials, the experimenter answered them in order to avoid the partners providing additional information. In between trials, children were provided with a distraction activity to occupy them while the experimenter turned the shelf away and switched the objects. After these 6 trials, the children were told that they were now going to play the game with the second partner. Upon completion of the 12 test trials, the experimenter placed a picture of each partner above their head on the wall and asked the partners to leave the room. Children were asked to point to the picture of the ingroup and outgroup partners to ensure that they remembered which partner was which (e.g., "Can you point to the girl who lives far away in Disentis and knows about Obwah?"), and 1 child was excluded from the analyses for failing to pass this final memory check. Children were then asked to indicate how similar each partner was to themselves (coded on a 4-point Likert scale: 1 = *really not like me,* 2 = *a little not like me,* 3 = *a little like me,* 4 = *really like me*) and also which partner they would like to play the game with again as a measure of preference.

Coding

Two cameras recorded the participants during the study; one was positioned on top of the shelf to record children's eye gaze, and the second was positioned behind the participants facing the shelf to record their responses to the instructions. Our coding scheme closely replicated that of Fan et al. (2015). For each trial, an offline coder was asked to record whether the participants first looked to the target object or the distractor object (i.e., the left side of the grid or the right side of the grid given that target and competitor objects were always on opposite sides), and the object that children moved within the shelf. If children touched or moved one object, we coded the first object that children moved to another location. The coder also indicated whether children chose the ingroup partner or outgroup partner in response to the questions at the end of the study along with children's Likert scale ratings for each partner. The coders were blind to condition and so were unaware when the children were playing with a similar or dissimilar partner. A reliability coder coded 25% of trials. Reliability was perfect (100%) for decisions on which item was moved as well as for responses to the questions at the end of the study. Reliability was also very high for gaze coding (98.59% of trials had agreement). We used coding from a third coder who was blind to condition for any trials with coding disagreements.

Results

Data for Study 1 are available on the OSF (https://osf.io/kru39/). As in Fan et al. (2015), we began by scoring each first look and reach trial for each child with a 1 for a correct response (selecting or looking to the target object) and a 0 for an incorrect response (selecting or looking to the distractor object). We then conducted a binomial logistic regression to analyze the effect of partner (2: ingroup vs. outgroup) and trial type (2: perspective-taking trials collapsing across scalar and two-object trials vs. non-perspective-taking trials) on children's object choices. Although we initially planned to run parametric analyses, we chose to run a binomial logistic regression to account for the binomial nature of children's responses (correct object or incorrect object) across repeated trials, and the model included partner and trial type as fixed predictors of children's responses and participant as a random predictor to control for the interdependence on repeated trials (parametric analyses are reported in the online supplementary material). The model showed no significant interaction between partner and trial type (Wald χ^2 = 1.38, *df* = 1, *p* = .24), and there was no significant main effect of partner on children's correct reaches (Wald χ^2 = 0.01, *df* = 1, *p* = .93). As expected, the model showed a significant main effect of trial type (Wald χ^2 = 185.27, *df* = 1, *p* < .001; see Fig. 2), where performance was higher on the non-perspective-taking trials (*M* = .99, *SD* = .05) than on the perspective-taking trials (*M* = .52, *SD* = .30; see Fig. 2). Model fit was not significantly improved by adding participant gender (Wald χ^2 = 0.30, *df* = 1, *p* = .585), counterbalancing order (Wald χ^2 = 4.32, *df* = 5, *p* = .505), or age group (Wald χ^2 = 1.47, *df* = 1, *p* = .226). The null effect of age is consistent with the previous research (Fan et al., 2015) on which we modeled our procedure, which found an effect of language exposure but not of age in children's perspective-taking scores.

As a second analysis of children's perspective-taking, we investigated whether the number of children who got 100% of the perspective-taking trials correct varied based on the social identity of the partner. Mirroring the findings from the logistic regression, there was no effect of partner; children were equally likely to select 100% of the correct objects when they were playing with the ingroup partner (*n* = 4) or the outgroup partner (*n* = 5) (χ^2 = 0.123, *p* = .726).

Next, we analyzed participants' first look during perspective-taking trials in order to assess any partner effects on children's *initial* egocentrism. This model showed no effect of partner similarity on children's first looks toward the target object (Wald χ^2 = 0.91, *df* = 1, *p* = .338). Model fit was significantly improved when age group was added as a covariate (Wald χ^2 = 4.11, *df* = 1, *p* = .043), suggesting an overall increase in correct first looks on perspective-taking trials with an increase in age, but there was no interaction between age and partner similarity (Wald χ^2 = 0.36, *df* = 1, *p* = .550). There were no effects of participant sex (Wald χ^2 = .01, *df* = 1, *p* = .915) or counterbalancing order (Wald χ^2 = 6.54, *df* = 1, *p* = .477) on correct first looks.

We calculated recovery (making an incorrect first look but moving the target object) and incorrect switching (making a correct first look but moving the distractor object) on each trial for each child. We included this measure because children sometimes show initial egocentrism but subsequent corrections of their egocentric first reactions (Epley et al., 2004; Nilsen & Graham, 2009). There was no effect of partner on rates of recovery (incorrect first look but correct reach; Wald χ^2 = 0.21, *df* = 1, *p* = .65) or incorrect switching (correct first look but incorrect reach; Wald χ^2 = 0.55, *df* = 1, *p* = .457).

We examined the latency of children's object choices to look for any partner effects on how long children took to make a decision. We calculated trial latency from the onset of the target noun to the children touching the object they later moved within the shelf. There was no significant effect of partner similarity on latency in the perspective-taking trials, *t*(69.24) = 0.92, *p* = .36, *d* = .14, but there was a significant effect of partner similarity on the non-perspective-taking trials, *t*(60.972) = 2.69, *p* = .009, *d* = .42, where children took longer to make decisions when playing with the outgroup partner (*M* = 5085 ms, *SD* = 2805) than with the ingroup partner (*M* = 3917 ms, *SD* = 1094). It is possible that this effect was simply due to children taking longer to interpret instructions in a different accent (e.g., Bent, 2014; Munro & Derwing, 1995; Nathan et al., 1998).

To examine whether children saw the ingroup partner as more like themselves than the outgroup partner, we compared children's similarity ratings for each partner as well as their preferred partner to play a game with again. Children rated the similar partner (*M* = 3.0, *SD* = 0.86) as significantly more like themselves than the dissimilar partner (*M* = 2.5, *SD* = 0.94), *t*(46) = 2.29, *p* = .027, *d* = .33, and

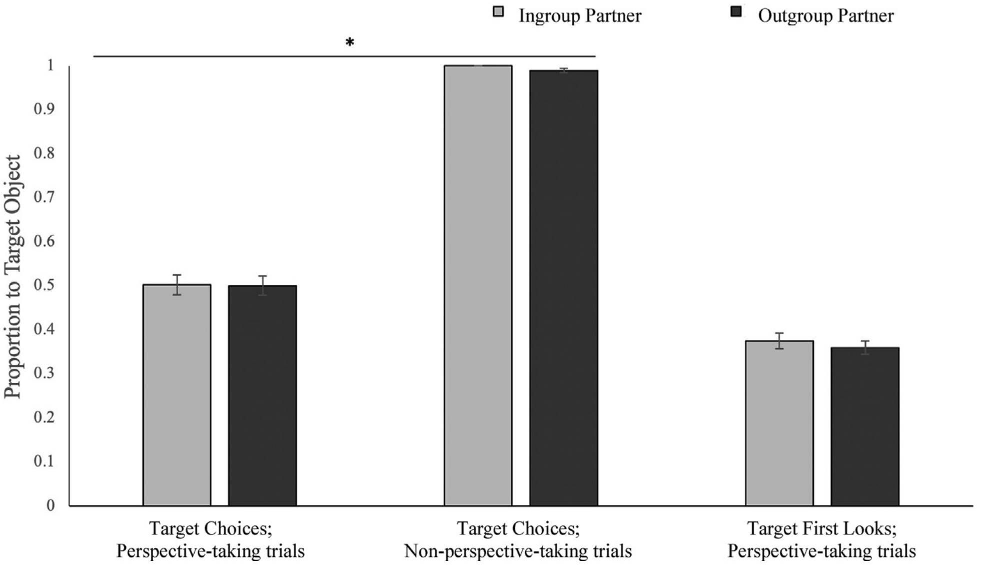

Fig. 2. Children's target object choices (cumulative perspective-taking trials and non-perspective-taking trials) and first looks (cumulative perspective-taking trials). Asterisk (*) denotes a significant main effect of trial type in Study 1. Error bars represent standard error of the mean.

significantly more children indicated that they would choose to play with the similar partner again (n = 37) than the dissimilar partner (n = 10) (p < .001, two-tailed binomial). These results suggest that our similarity manipulation was effective, although it did not influence children's perspective-taking.

Discussion

In Study 1, manipulating social group membership via similarity did not affect children's perspective-taking. There was no effect of partner group membership on any of our measures of perspective-taking, including object choices, latency, initial first looks toward an object, and recovery or incorrect switching. Age did not have an effect on perspective-taking for the ingroup versus outgroup partner; in neither age group did children show a difference in perspective-taking between partners (note that we are comparing rates of perspective-taking between partners rather than overall perspective-taking ability; e.g., Epley et al., 2004; Mossler et al., 1976; Nilsen & Graham, 2009). Importantly, the null effect of partner group membership on perspective-taking does not appear to be because our ingroup manipulation was not effective. Children rated the ingroup partner as more similar to themselves than the outgroup partner and were more likely to say that they would prefer to play with the ingroup partner again (in line with research demonstrating an ingroup preference in children; Aboud, 2003; Bigler et al., 1997; Mahajan & Wynn, 2012). The null finding is also likely not because the task was not tapping into perspective-taking. Children made fewer correct reaches on perspective-taking trials than on non-perspective-taking trials, and rates of choosing the correct object in perspective-taking trials are consistent with previous findings using the director task (e.g., Fan et al., 2015). We also think it is likely that children inferred a larger bank of shared cultural knowledge with the ingroup partner. Children in this age range (4–6 years) have been shown to predict shared cultural knowledge between group members based on accent and shared geographic background (e.g., Goldvicht-Bacon & Diesendruck, 2016; Liebal et al., 2013; Soley & Aldan, 2020; Weatherhead et al., 2018a), and in our study the ingroup partner even explicitly expressed some shared knowledge with children (knowledge of a TV show or book). Therefore, the null results in the director task between partners suggest that shared social group membership—although this should suggest differences in shared knowledge to children—does not influence children's perspective-taking in this task.

One possible reason for this is that shared social group membership with a stranger, even if it suggests more shared knowledge than with the outgroup, does not produce a strong enough feeling of aligned perspectives to lead to relaxed perspective-taking for the ingroup partner. In adults, a direct manipulation of shared knowledge between strangers led to a decrease in perspective-taking for the director with more shared knowledge (Wu & Keysar, 2007b), and adults also show decreased perspective-taking for close friends compared with strangers where there is a stark difference in shared knowledge (Savitsky et al., 2011). Our manipulation required an extra inferential step of inferring more shared knowledge with an ingroup member based on group membership, even though both directors were strangers to the children. Perhaps children, like adults, would relax their perspective-taking for a communication partner who is close to them and with whom they have direct evidence of extensive shared knowledge and perspective alignment.

Therefore, we modeled our Study 2 manipulation on Savitsky et al. (2011), examining whether children's perspective-taking would be influenced by their personal closeness to their communication partner. The difference in shared knowledge between a close friend or family member compared with a stranger is evident and does not need to be inferred. Adults tend to assume that their perspectives are aligned with close friends and partners, leading to reduced perspective-taking of close others and a tendency to overestimate the effectiveness of communication (Clark, 1996; Savitsky et al., 2011, Van Der Wege et al., 2021). Because 4- to 6-year-old children's closest social connections are their families, we decided to compare children's perspective-taking between an experimenter they had just met and the caregiver who brought them into the lab for the study, reasoning that children's close kin versus a stranger should produce the maximum reliable difference in closeness and shared knowledge.

Study 2

We preregistered all method details and our analysis plan on the OSF for Study 2 (https://osf.io/kru39/). Our manipulation was inspired by Savitsky et al. (2011), and we based our sample and method on Fan et al. (2015). We conducted a power analysis and estimated the effect size using the effect size observed between groups in Fan et al. (2015) (d = .83, α = .05). This analysis showed that 24 children per condition (caregiver or experimenter) would yield 80% power to detect an effect. We ran this power analysis before deciding that conducting a binomial logistic regression was more appropriate for our data, and so the power analysis was conducted using the parametric stats, as reported in Fan et al. (2015). Children completed the director task either with a caregiver or with an experimenter they had just met. If children, like adults, perceive that their perspectives are more likely to be aligned with those of a close family member with whom they have shared a lifetime of knowledge and experiences, they should show decreased perspective-taking for a caregiver than for the stranger.

Method

Participants

A total of 48 4- to 6-year-old children participated in the current study (M = 65.35 months, SD = 11.33, range = 48.87–82.43; 22 boys). Because we observed no effects of age group in Study 1, we followed Fan et al. (2015) in collecting data from 4- to 6-year-olds in Study 2, aiming to balance the sample across these 3 years and look at age as a continuous variable in our analysis. Participants were typically developing children from a mid-sized city in New Zealand recruited from a database of families interested in participating in research on child development, and none of the children had participated in Study 1. Data were collected between February 2019 and July 2020. An additional 3 children were excluded for not following instructions (n = 1), procedural error (n = 1), or parental interference (n = 1). As in Study 1, the majority of participants had some exposure to a second language but were not bilingual (n = 37), and a handful of children were monolingual (n = 6) or bilingual

(n = 5). The numbers of children with each language background were similar across the two conditions (caregiver vs. research assistant).

Procedure

Participants in this study completed the same director task as in Study 1 in our lab testing room, either with their caregiver or with an unfamiliar director, a research assistant from the lab. We did not give caregivers or research assistants any training prior to the study appointment, but they were provided with a sheet of instructions to read over right before the study (available on the OSF at https://osf.io/kru39/), and the written instructions were placed on the director's side of the shelf for them to consult during the study. Because caregivers only completed the task with their own children, research assistants—who were not given any information about the motivation for the study—were also regularly swapped across participants to ensure as much as possible that research assistants were not more practiced at the director role than caregivers. The research assistant always had a New Zealand accent, and no other information about the research assistant was provided. Children met the research assistant for the first time when entering the testing room. To keep the information children were given consistent between partners, caregivers and research assistant directors were instructed not to answer any questions from the children and the children were told that if they had questions they should ask the experimenter running the study.

In addition to using a different manipulation, we made other minor procedural changes aimed at reducing the overall difficulty of the task from the aspects unrelated to perspective-taking. First, we used a between-participants design in Study 2 to be more consistent with prior studies that have found effects of social experience and partner type on perspective-taking in the director task (Fan et al., 2015; Savitsky et al., 2011). The switch was also motivated by practical purposes; we wanted to keep partners blind to the key manipulation, the contrast between caregivers and research assistant directors. A between-participants design allowed us to keep the specific purpose of the study from the director until the end of the session and allowed caregivers of children in the research assistant condition to watch their children participate via a live feed to our reception room TV (a requirement of our lab ethics approval). To reduce additional task demands, we only used two-object perspective-taking trials (removing the scalar perspective-taking trials used in Study 1) and we asked children to simply remove the items from the grid and place them into a basket (whereas in Study 1 children also needed to remember where in the grid to place the requested objects). Lastly, at the end of the director task, children were asked how easy or hard they thought the game was. Because children played the game with only one social partner (between participants), we did not ask them which partner they were most similar to or who they wanted to play with again.

Our hypothesis was that children would perform worse on the director task when interacting with their caregiver due to relaxing their perspective-taking. However, lowered performance could also be due to other reasons, including that caregivers are less practiced experimenters than research assistants (and the task was relatively complicated). To address this confound, we also included the day–night task (Gerstadt et al., 1994), typically used to measure executive functioning. Children completed this task with the same partner as in the director task (either their caregiver or the research assistant). If lowered performance on the perspective-taking task was due to caregivers having less experimental training in how to run a study, then we would expect performance to also be lower on the day–night task for children who were participating with their caregiver. Alternatively, if any differences were specific to perspective-taking, then we would not see a difference in the day–night task. Interestingly, Fan et al. (2015) found no association between an executive functioning task and perspective-taking in the director task, so we felt confident in using the day–night task as a non-perspective-taking measure of performance. A total of 36 participants completed the day–night task with their partner given that we began running this task after the first 10 children had participated. Because we did not add this task at the onset of the study, it was not preregistered and therefore the results should be taken as relatively more exploratory. In addition, 2 children chose to stop their participation in the study after completing the perspective-taking trials but before beginning the day–night task.

During the day–night task, the caregiver or research assistant showed children cards with a picture of either a sun or moon on them. Children were instructed to say "day" when shown the picture of the moon and to say "night" when shown the picture of the sun, meaning that children needed to inhibit a typical response to respond correctly (i.e., "day" for the sun picture and "night" for the moon picture). The main experimenter completed 4 practice trials with the children to ensure that they understood the instructions and then asked the caregiver or research assistant to run the remaining trials with the 16 test cards.

Coding

Coders used the same coding protocol as in Study 1 (Fan et al., 2015), and a reliability coder coded 25% of trials. Reliability was perfect (100%) for decisions on which item was moved, responses to the questions at the end of the study, and responses in the day–night task. Reliability was also very high for gaze coding (98.95% of trials had agreement). We used coding from a third coder who was blind to condition for any trials with coding disagreements.

Results

In our preregistration, we noted that we would use parametric testing (analysis of variance [ANOVA] and *t* tests) and analyze the data as proportions. However, upon consideration of the non-parametric data and design, we decided that using a binomial logistic regression would be more appropriate to control for the repeated trials with a binomial outcome. In addition, Shapiro–Wilk tests revealed that the distributions for proportion of correct reaches, first looks, and recovery and incorrect switching across partner differed from a normal distribution (all *p*s $< .002$), and thus we report the most appropriate statistical tests here and report the parametric analyses in supplementary material (available on the OSF at https://osf.io/kru39/). As in Study 1, children were given a reach score and a first look score for each trial. A correct response (selecting or looking first to the target object) was scored as 1, and an incorrect response (selecting or looking first to the distractor object) was scored as 0. Children also received a recovery score (first looking at the distractor but then selecting the target) and an incorrect switching score (first looking at the target but then selecting the distractor) on each trial.

We conducted a binomial logistic regression to analyze the effect of partner (2: caregiver vs. research assistant) and trial type (2: perspective-taking vs. non-perspective-taking) on children's correct reaches (i.e., selecting the mutually visible target object). This model yielded a significant interaction between partner and trial type (Wald $\chi^2 = 5.39$, $df = 1$, $p = .02$; see Fig. 3). In non-perspective-taking trials, children's correct object choices did not differ when playing with the caregiver ($M = .98$, $SD = .07$) or research assistant ($M = .94$, $SD = .11$) (Wald $\chi^2 = 2.14$, $df = 1$, $p = .14$). In perspective-taking trials, there was a nonsignificant trend, such that children made fewer correct reaches when playing with their caregiver ($M = .69$, $SD = .31$) than when playing with the research assistant ($M = .81$, $SD = .28$) (Wald $\chi^2 = 2.77$, $df = 1$, $p = .096$). The model fit was not significantly improved by adding participant gender (Wald $\chi^2 = .72$, $df = 1$, $p = .394$), counterbalancing order (Wald $\chi^2 = 3.15$, $df = 7$, $p = .87$), or age in months as a continuous covariate (Wald $\chi^2 = .02$, $df = 1$, $p = .90$).

In another preregistered analysis following from Fan et al. (2015), we examined whether children's likelihood of responding correctly on 100% of perspective-taking trials varied based on the communicative partner. Children were more likely to respond correctly on 100% of their perspective-taking trials when playing with the research assistant ($n = 15$) than when playing with their caregiver ($n = 8$) ($\chi^2 = 4.09$, $p = .043$), again suggesting reduced perspective-taking with the close partner. The difference between partners was not affected by how easy or hard children found the game to be; children rated the game as equally "easy" when playing with their caregiver ($M = 3.44$, $SD = 0.92$) or the research assistant ($M = 3.63$, $SD = 0.59$), $t(39.24) = -0.84$, $p = .408$, $d = -.24$.

We then examined children's latency to select an object within the shelf. Although there was no difference in latency between partners on the non-perspective-taking trials, $t(159.84) = -0.16$, $p = .87$, $d = -.02$, on the perspective-taking trials children were marginally slower at making choices when playing with their caregiver ($M = 5498$ ms, $SD = 3329$) than when playing with the research assistant ($M = 4195$ ms, $SD = 1224$), $t(116.04) = 1.98$, $p = .050$, $d = .29$. As in Study 1, we also examined how children's first looks compared with their final choices. There was no effect of partner on children's correct first looks (Wald $\chi^2 = 0.12$, $df = 1$, $p = .727$), suggesting that initial rates of egocentrism

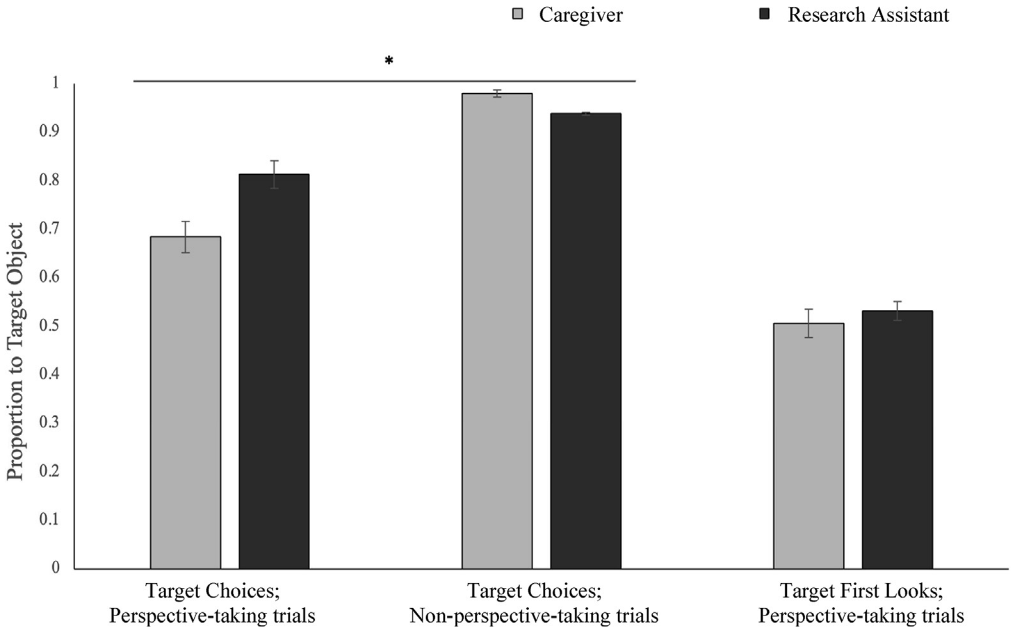

Fig. 3. Children's correct object choices (perspective-taking and non-perspective-taking) and first looks (perspective-taking) during Study 2. Asterisk (*) denotes a significant interaction between partner and trial type. Error bars represent standard error of the mean.

were similar regardless of partner. Partner had no effect on rates of recovery (incorrect first look but correct object choice; Wald $\chi^2 = 1.29$, $df = 1$, $p = .257$) or on incorrect switching (correct first look but incorrect object choice; Wald $\chi^2 = 0.04$, $df = 1$, $p = .851$).

Finally, we compared performance on the day–night task for the caregiver and the research assistant. We calculated the proportion of correct responses for each child out of the 16 cards presented given that 3 children were not shown all 16 cards by their partner (because the participants chose to stop partway through or two cards were stuck together and the partner did not notice). A two-sample *t* test showed no significant effect of partner on children's correct responses during the day–night task, $t(33.64) = 0.013$, $p = .896$, $d = .04$, demonstrating similar accuracy when playing with a caregiver (proportion correct: $M = .77$, $SD = .21$) or a research assistant ($M = .76$, $SD = .23$) and suggesting that the difference in performance between caregiver and research assistant in the director task was a difference in perspective-taking. Because studies have sometimes found associations between perspective-taking and executive function tasks in adults (e.g., Brown-Schmidt, 2009) and sometimes in children (Nilsen & Graham, 2009; cf. Fan et al., 2015), we also looked at the correlation between children's scores on the perspective-taking trials of the director task and the day–night task. There was no correlation between the two tasks, $r(34) = .11$, $p = .520$, consistent with Fan et al.'s (2015) finding of no correlation between an executive function card sort task and children's perspective-taking in the director task.

Discussion

The findings from Study 2 provide some evidence that children's perspective-taking differs when communicating with a close caregiver compared with a stranger they had just met. In perspective-taking trials, children made marginally fewer correct reaches when playing with the caregiver compared with the research assistant. Similarly, significantly fewer children made correct reaches on 100% of their perspective-taking trials when playing with their caregiver than when playing with the research assistant. Finally, children took marginally longer to make decisions in perspective-taking trials when playing with their caregiver than when playing with the research assistant. As in Study 1, there was no effect of partner on children's correct first looks toward an object. Partner identity did not influence children's performance in non-perspective-taking trials or in the inhibitory control day–night task, suggesting that the effect appears to be specific to perspective-taking rather than task performance generally.

Interestingly, we observed no correlation between the day–night task and perspective-taking in the director task, suggesting that the director task we used may tap into more social perspective-taking rather than more general inhibition or cognitive control. This finding replicates the pattern seen in Fan et al. (2015), where children's executive functioning was not related to perspective-taking in the director task. Although there has long been discussion of perspective-taking as inhibition (of one's own perspective), it is possible that different perspective-taking tasks require different skills; some are more about visual cues and mental rotation, whereas other might be more social or related to mental states. For example, age-related increases in egocentric errors are more prominent in tasks that require self mental rotation than in tasks that require object mental rotation, suggesting that executive functioning abilities can affect different perspective tasks differently (see Inagaki et al., 2002, for further discussion). Future research using a battery of perspective-taking tasks is required in order to understand which perspective-taking tasks are more cognitive in nature (and related to executive functioning) versus more social in nature. It is possible that partner identity is more likely to affect perspective-taking on social perspective-taking tasks than on cognitive perspective-taking tasks. Overall, the results of Study 2 suggest that closeness with a communication partner can lead children to feel a decreased need to engage in perspective-taking.

General discussion

Two studies investigated whether social closeness to a communication partner influences children's visual perspective-taking, hypothesizing that children would relax their perspective-taking for a close partner with more shared knowledge. When we manipulated social closeness via social group membership in Study 1, children reported feeling more similar to the ingroup partner than the outgroup partner but showed no difference in perspective-taking between partners. Study 2 used a starker manipulation of personal closeness, contrasting children's perspective-taking between a stranger the children had just met and a caregiver with a close kinship bond. In this case, there were small but consistent effects of our manipulation specific to perspective-taking and in the predicted direction. That is, children showed reduced perspective-taking for their caregiver, with whom they share their lifetime of knowledge and personal experiences, compared with the novel research assistant. Children in this study performed similarly in non-perspective-taking trials and in an executive function task with both partners. Although the study should be replicated, the consistent direction of the effect and its specificity to the perspective-taking trials provide evidence that children's perspective-taking is reduced when communicating with a close partner.

Why would children show reductions in perspective-taking for a caregiver relative to a stranger (Study 2) but not for a social ingroup member relative to an outgroup member (Study 1)? We have hypothesized that the mechanism of the closeness–communication bias is the increased shared knowledge between close partners, leading children to perceive high perspective alignment and thus feel less need to consider that their partner's perspective might differ from their own (Savitsky et al., 2011). The finding that a manipulation of high versus low shared knowledge influences perspective-taking in adults, even in the absence of any prior closeness with either communication partner, provides support for this idea (Wu & Keysar, 2007b). Children share a greater quantity of knowledge with their caregiver than with a stranger as well as more types of knowledge. As with any ingroup member, children share cultural knowledge with their caregiver by virtue of being part of the same cultural community, but they also share extensive episodic knowledge by virtue of a lifetime (for the children) of shared experiences. Sharing extensive episodic knowledge and experiences with a close partner as children do with a caregiver and as adults do with close friends (Savitsky et al., 2011), may be more likely to lead to an assumption that perspectives are aligned and to a subsequent reduction of visual perspective-taking in this task (relative to the inferred shared cultural knowledge with a novel ingroup member). However, to carefully isolate the role of shared knowledge in children's perspective-taking, future work with children should directly manipulate shared knowledge (both amount and type) between partners while holding closeness constant. For example, researchers could experimentally manipulate the amount of shared episodic knowledge that children have with an experimenter by assigning children to participate in the task with an experimenter they have already interacted with versus someone completely new.

Although we posit that the differences in shared knowledge underlie the closeness–communication bias we saw in Study 2 but not in Study 1, there are other differences between the manipulations used in the two studies that suggest alternative explanations. The difference in feelings of closeness between a caregiver and a stranger is likely to be much greater than that between a novel ingroup member and outgroup member, and other meaningful differences between kin members (i.e., caregivers) and non-kin members are consistently observed in other research. Children from an early age show unique expectations and behaviors with respect to kin versus non-kin (Spokes & Spelke, 2016; Thomas et al., 2022a, 2022b), and there is evidence from nonhuman primates showing that kin is a privileged category (Murray et al., 2016; Planer, 2021; Emery Thompson, 2019). Kinship and personal feelings of closeness could lead to reduced perspective-taking in accordance with anchoring-and-adjustment models of communication. There is evidence that adults represent those closest to them as highly similar to, and even overlapping with, the self (Aron et al., 1991; Decety & Sommerville, 2003), making it more difficult to adjust away from an egocentric default to take the perspective of close others (Keysar, 2007; Keysar & Barr, 2002; Nickerson, 1999). This effect might be strongest in children with their closest kin; however, it could also apply to close non-kin (e.g., a close friend or playmate).

The asymmetric nature of the relationship between a caregiver and child is another factor that could have driven the partner difference in Study 2. According to efficiency-driven accounts of communication in adults, individuals are more likely to consider a partner's perspective when the expected benefits outweigh the cognitive costs, and they should perceive less need for perspective-taking when their social partner is likely to take on a greater share of the communicative labor (Hawkins et al., 2021). If similar efficiency motivations underlie children's communication (as in Frank & Goodman, 2014; Morisseau et al., 2013), children might, based on past experience, expect their caregiver to take on more of the communicative burden (and this expectation should apply less to a social ingroup member who they had just met, as in Study 1). Even during infancy, caregivers tend to tailor communication to their children and structure interactions in ways that can optimize children's learning (Yurovsky, 2018). If children tend to expect their caregivers to take a leading role in structuring communication, this may have caused them to relax their perspective-taking in the director task. This idea is also supported by research showing that children sometimes place more weight on a caregiver's knowledge than knowledge provided by a domain expert (Boseovski & Thurman, 2014) and that they take a less active role in making moral decisions when communicating with a parent than when communicating with a peer (Mammen et al., 2019). To disentangle the influence of personal closeness from the parent–child communicative dynamic, future work could compare children's perspective-taking for a close friend classmate compared with a non-friend classmate; this would also somewhat equate for shared knowledge by using partners from the same school community and classroom environment.

Ultimately, it will be important for research to tease apart shared knowledge, closeness and kinship, and the caregiving relationship in order to hone in on the specific factors and mechanisms that lead to a closeness–communication bias in children's perspective-taking. We cannot compare absolute rates of perspective-taking across our two studies due to the methodological differences aimed at reducing task demands. So, further replication is needed to directly compare perspective-taking differences stemming from shared experiences, closeness, and shared social groups. Regardless of which factor matters most, however, it is interesting that children may have a reduced tendency to consider their caregiver's perspective but not the perspective of an ingroup member. Reduced perspective-taking for a caregiver could have implications for social interactions, for example, perhaps making it more difficult for children to understand their caregiver's behavior when the caregiver acts in ways that are not aligned with children's desires. Like adults when interacting with close others, children might also tend to overestimate their communicative effectiveness with their caregiver, resulting in frustrating communicative breakdown (Savitsky et al., 2011; Van Der Wege et al., 2021). Children may eventually show the closeness–communication bias with partners other than caregivers as well, such as social ingroup members. It is possible that children may need experience in interacting with multiple social partners before they begin to tailor their perspective-taking based on partner characteristics beyond close personal relationships or kinship. Testing school-aged children in the 7- to 9-year age range would be informative as to the development of adjusting perspective-taking to specific interaction partners.

Another important point to consider when investigating the influence of partner characteristics on perspective-taking is that effects seem to vary in the literature depending on how perspective-taking is measured. The finding of no correlation between the director task and the executive function task in our Study 2, as well as in Fan et al. (2015), suggests that this task is tapping into a social form of communicative perspective-taking rather than extraneous cognitive tasks such as general response inhibition. This could explain why our Study 2 effect is consistent with the partner effects on perspective-taking seen in adult work using the same task (Savitsky et al., 2011; Wu & Keysar, 2007b). However, we might not expect to see the same effects of a close partner in other types of perspective-taking tasks; for example, in adults some (non-communication-based) perspective-taking tasks show a converse effect of improved perspective-taking for socially close or similar social targets (e.g., individuals who are the same age: Ferguson et al., 2018; individuals who are in the same social group: Ye et al., 2021). Considering how factors like closeness and similarity to a social partner can influence different types of perspective-taking—and theory of mind more generally (see McLoughlin & Over, 2017, for an interesting example of this in children)—is an important research avenue.

Data availability

Our data is available on the Open Science Framework (https://osf.io/kru39/).

Acknowledgment

This research was supported by a Marsden Fast-Start grant (VUW1607) from the Royal Society of New Zealand to A.M.

Appendix A. Supplementary material

Supplementary material to this article can be found online at https://doi.org/10.1016/j.jecp.2023.105707.

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