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Can Self-Persuasion Reduce Hostile Attribution Bias in Young Children?

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Abstract

Two experiments tested an intervention approach to reduce young children's hostile attribution bias and aggression: self-persuasion. Children with high levels of hostile attribution bias recorded a video-message advocating to peers why story characters who caused a negative outcome may have had nonhostile intentions (self-persuasion condition), or they simply described the stories (control condition). Before and after the manipulation, hostile attribution bias was assessed using vignettes of ambiguous provocations. Study 1 ($n = 83$, age 4–8) showed that self-persuasion reduced children's hostile attribution bias. Study 2 ($n = 121$, age 6–9) replicated this finding, and further showed that self-persuasion was equally effective at reducing hostile attribution bias as was persuasion by others (i.e., listening to an experimenter advocating for nonhostile intentions). Effects on aggressive behavior, however, were small and only significant for one out of four effects tested. This research provides the first evidence that self-persuasion may be an effective approach to reduce hostile attribution bias in young children.

Keywords Hostile attribution bias · Self-persuasion · Aggression · Intervention · Experiments

Children's daily social interactions abound with provocations by peers, such as when they are physically hurt, laughed at, or excluded from play. The exact reasons behind these provocations, and especially the issue of whether hostile intent was involved, are often unclear. Responding adequately to such ambiguous provocations is central to children's social adjustment (Dodge et al. 1986). Children who tend to perceive ambiguous provocations in a hostile way (e.g., "she tripped me on purpose") may often respond aggressively, which puts them at risk for psychological maladjustment (Weiss et al. 1992). Indeed, numerous studies have shown that hostile attribution biases are linked to aggressive behavior (for reviews, see: Dodge 2006; De Castro et al. 2002), as early as the preschool years (Runions and Keating 2007; Weiss et al. 1992). Accordingly, many intervention programs aiming to prevent aggressive behavior problems include techniques to reduce children's hostile attribution bias (for a review, see Wilson and Lipsey 2006). Such intervention efforts may best commence in early childhood, when children's hostile attribution bias are still relatively sensitive to change (Crick and

Dodge 1994). The present research tests an intervention approach to reduce hostile attribution bias in young children.

Most interventions that effectively reduce children's hostile attribution bias rely on attribution retraining techniques (e.g., *Coping Power*, Lochman and Wells 2002; *BrainPower*, Hudley and Graham 1993; *Anger Control Training*, Sukhodolsky et al. 2005). Children taking part in these interventions typically are assembled in small groups to discuss ambiguous peer provocations. During these discussions, therapists encourage children to question their hostile attributions and teach them to detect cues signaling that someone acted with benign intent (Hilt 2004). Meta-analytical work has shown that such interventions tend to effectively reduce children's aggressive behavior (i.e., weighed mean difference effect size = 0.26; Wilson and Lipsey 2006). However, it is unknown to what extent the attribution retraining component contributes to these effects (rather than other intervention components such as anger management or social problem solving).

Moreover, little is known of how attribution retraining is best delivered. The goal of attribution retraining is to reduce children's hostile attribution bias by persuading them that peer provocations do not necessarily stem from hostile intentions (e.g., "I don't think she hurt you on purpose. See? She looks sad."). Such persuasion is not straightforward. Research in adults suggests that direct attempts at persuasion occasionally backfire: People may reject (rather than accept) such persuasion when their own beliefs (1) are highly discrepant from the

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persuasive message (i.e., when the message falls outside their "latitude of acceptance;" Atkins et al. 1967), and (2) are strongly held (Eagly and Telaak 1972; Schlenker and Trudeau 1990).

Both these conditions may apply to children with hostile attribution biases. First, the notion that "peers may have benign intentions" will often be discrepant from these children's typical attribution of provocations as stemming from hostile intentions. Second, hostile attribution biases often are strongly held. Children may have initially acquired a hostile attribution bias because others actually *had* hostile intentions and *did* harm them (Dodge 2006; Frankenhuis and De Weerth 2013). Indeed, children holding a hostile attribution bias have often experienced social adversity in the past, such as harsh parenting or peer rejection (Dodge et al. 1995; Perren et al. 2013; Weiss et al. 1992). Ingrained hostile attribution biases are less susceptible to persuasion by others, and thus limit the potential effectiveness of attribution retraining techniques.

Instead of trying to persuade children, therapists may also adopt a more indirect approach to reduce children's hostile attribution bias: self-persuasion (Aronson 1999). Self-persuasion entails asking people to publicly advocate against their own beliefs. The resulting change in beliefs can be explained by cognitive dissonance processes (Festinger 1957): If people publicly espouse viewpoints that are discrepant from their privately held beliefs, they tend to later realign their beliefs with these viewpoints. In adults, self-persuasion has been shown to effectively lead individuals to accept and internalize belief-discrepant messages (Fazio et al. 1977). For instance, one study showed that individuals who strongly opposed the use of marijuana later changed their beliefs if they had recorded a video message advocating the legalization of marijuana (Nel et al. 1969). Similarly, an effective attribution retraining approach may be to ask children themselves to advocate that peer provocateurs may have had nonhostile intentions.

To investigate the potential effectiveness of self-persuasion as an attribution retraining approach, we conducted two between-subjects experiments involving 4–9-year-old children with high levels of hostile attribution bias. In Study 1 ($n = 83$), children recorded a video message (allegedly to be shown to pupils from other schools) advocating why peer provocateurs in a series of ambiguous provocation scenarios may have had nonhostile intentions (self-persuasion condition), or they merely described the scenarios (control condition). Study 2 ($n = 121$) replicated Study 1, and also included a third condition to investigate whether self-persuasion is more effective than persuasion by others (i.e., children listened to an experimenter advocating why the provocateurs may have had nonhostile intentions). In both studies, we used vignettes to assess children's hostile attribution bias before and after the manipulation. Moreover, to investigate to what extent the predicted effect on hostile attribution would generalize to aggressive behavior, we included an in vivo provocation scenario to measure children's aggression in an emotionally involving situation. We also explored

moderation by gender. Boys engage in more direct aggression than girls (Card et al. 2008) and may thus benefit more from the self-persuasion assignment than do girls.

The study procedures were approved by the Ethics Committee of the Faculty of Social and Behavioral Sciences of Utrecht University. All materials, raw data and syntax can be found online (Van Dijk et al. 2018).

Study 1

Method

Participants Participants were 83 Dutch children aged 4–8 (58.7% boys; $M_{age} = 6.70$, $SD = 1.36$; 92.2% Caucasian), recruited from kindergarten ($n = 23$) and primary schools (first grade: $n = 25$; second grade: $n = 35$). We selected them from a larger sample of 283 children (62.7% boys; $M_{age} = 6.84$, $SD = 1.29$; 95.2% Caucasian) for having high levels of hostile attribution bias (see *selection of participants*). The schools were located in five municipalities (16,000–206,000 inhabitants) serving middle-class communities (note that income inequality in The Netherlands is low; OECD 2018). A priori power was sufficient (0.80 for $n = 70$) to detect small-to-medium effects ($f = 0.17$) of self-persuasion on pre to post change in children's hostile attribution bias. For all participants, informed consent was obtained from one of the parents (consent rate = 60.6%).

Pre-Assessment We conducted the pre-assessment of children's hostile attribution bias in the context of a larger study on social cognition and peer relationships (Van Dijk 2017). Children were individually interviewed in a quiet room in their school. The interview lasted 35–45 min and was conducted by the first author or one of eight research assistants. We gave children stickers to thank them for their voluntary participation.

Hostile attribution bias (vignettes). We measured hostile attribution bias using four vignettes describing a hypothetical interaction between the child and a same-gender protagonist. The vignettes described ambiguous provocations—that is, the protagonist caused a negative outcome, but it was unclear whether this negative outcome was intended. Story themes were provocations familiar to young children: (1) being hurt, (2) their drawing being ruined (3) being left out of play, and (4) their toy being taken. We drew these themes from vignettes developed to measure hostile attribution bias (Feshbach 1989; Dodge et al. 1985). Experimenters red the stories aloud, each supported by a set of line drawings (i.e., three 8 × 8 cm black-and-white line drawings per vignette; Fig. 1).

We measured attributions using two questions following each vignette. First, the experimenter asked: "Why did the boy/girl [cause the negative outcome]?" If children's first response did not reflect a hostile or benign attribution, the experimenter probed them with a hostile and a benign option

Fig. 1 Sample vignette (boys). "Imagine that you walk into the classroom. Two boys are playing a board game. You ask if you can join the game, but one of the boys says "no"
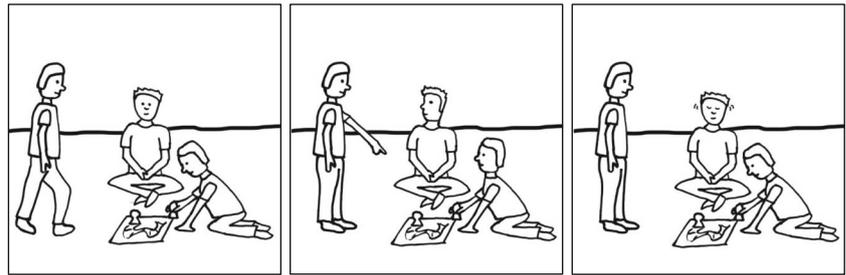
(36.7% of responses; e.g., "did the boy try to reject you, or was it not possible for another player to join in?"). Second, the experimenter asked: "Was the boy/girl trying to be *mean* or *not trying to be mean*?" (we counterbalanced the order of response options across vignettes).

Two trained research assistants coded all responses into the following categories: (a) *hostile* attribution (e.g., "he doesn't like me"); (b) *benign* attribution (e.g., "there were only two pawns in the game"); (c) *ambiguity* attribution (if children indicated that the protagonist's intentions could both be hostile and benign, e.g., "he does not want me to join, or maybe the game is meant for two players"); and (d) *unclear* (if children did not answer or if it was unclear whether children's response reflected hostile or benign intent; e.g., "they wanted to play together"). Inter-coder reliability was good ($\kappa = 0.87$ across vignettes). We resolved coding disagreements (8.1% of responses) by discussion, using children's scores on the hostile-or-benign probe question when available. We calculated hostile attribution bias scores as the average across the eight questions, coding hostile and mean responses as 1 and all other responses as 0 ($\alpha = 0.70$). Meta-analytical work has shown that vignette-based assessments of hostile attribution bias are linked to aggressive behavior ($r = 0.24$; De Castro et al. 2002), supporting concurrent validity.

Aggression (teacher-rated). The day after the pre-assessment, we asked teachers to complete the Instrument for Reactive and Proactive Aggression (IRPA; Polman et al. 2009). They rated the frequency of seven forms of aggressive behavior (i.e., kicking, pushing, hitting, name calling, arguing, gossiping, and doing sneaky things) that their pupil engaged in within the last week, on a 5-point Likert scale (0 = *never*, 1 = *once*, 2 = *several times*, 3 = *every day*, 4 = *several times a day*). We computed aggression scores as the average of the seven items ($\alpha = 0.79$). This measure shows positive associations with other (peer- and teacher-report) aggression measures (Polman et al. 2009) and effectively discriminates between children with disruptive behavior disorders and controls (Schoorl et al. 2016). We also obtained ratings of reactive and proactive motives, but opted not to report these because the results were similar as for the frequency ratings.

Selection of Participants We selected children to take part in the experiment proper if (1) their hostile attribution bias score at pre-assessment was within the highest third for

their grade level (i.e., score > 0.50 for kindergarteners, and score > 0.25 for children from first and second grade), and (2) their task comprehension was rated as sufficient by experimenters (n = 4 children had insufficient comprehension, as indicated by their inability to respond in a meaningful way, even after probing). In total, 83 children took part in the study (n = 5 other children were absent on the day of testing). Selected children scored significantly higher (M = 0.58, SD = 0.18) than unselected children (M = 0.18, SD = 0.18) on hostile attribution bias, p < 0.001, but not on teacher-rated aggression, p = 0.268. We randomly assigned selected children to either the self-persuasion (n = 43) or the control condition (n = 40).

Experimental Manipulation Children participated in the experiment approximately 1 month after the pre-assessment (range = 25–48 days). This session lasted 10–15 min and was conducted by the first author. Children were asked to "publicly" endorse nonhostile attributions in a video message, allegedly to be shown to pupils from other schools. The experimenter made children the advocates of the nonhostile message, telling them that *other children* tend to unjustly attribute hostile intent: "I visit many schools to reduce conflicts amongst pupils. Children often become angry because they think that another child did something mean to them on purpose. However, they cannot be sure that the child tried to be mean; it may have been an accident." Next, the experimenter asked them to record a video message: "Because you are a child, you can explain these things much better than I can. Of course, you cannot join me to visit all these schools every day. So, instead, I would like you to record a video message. Is that OK?" In the control condition, the experimenter told children: "I visit many schools to tell stories to the pupils. The stories describe the things that children do at school" and similarly asked them to help the experimenter, in this case by recording a video message to describe the stories.

In both conditions, children received a picture book that served as the basis of their video message. This picture book contained four stories of ambiguous provocations, depicting: (1) physical harm, (2) not sharing candy, (3) knocking over a block tower, and (4) refusing someone to join a table. Each story involved different characters (all gender-matched and drawn with neutral facial expressions), and consisted of two

colorful A3-size drawings of the setting and the ambiguous provocation (Fig. 2). As the assessment of hostile attribution bias also consisted of ambiguous provocation stories, we minimized resemblance by using unique story themes and drawings in a different format and style.

Before recording each story, children described the scenario, and the experimenter prompted them in case they missed information important for the storyline (17.6% of responses). In the control condition, children recorded their message immediately thereafter: "Please record the story like you just told it." In the self-persuasion condition, the experimenter prompted children to come up with two attributions: "Can we be sure that the boy/girl tried to be mean? What else could have happened?" Most children mentioned benign attributions ($M_{benign} = 1.31$, $SD = 0.42$); hostile attributions were rare ($M_{hostile} = 0.11$, $SD = 0.18$). All children generated benign attributions for at least two stories; most children (69.8%) did so for all stories. If children mentioned no benign attribution (9.3% of responses) or just one (53.5% of responses), the experimenter helped them co-construct their video message by suggesting additional benign attributions (e.g., for the tower story: "the tower was wobbly," "he/she was not paying attention"). Recording of the self-persuasion video messages went well: Most children (74.4%) did so without needing an instruction reminder.

Manipulation check. The first author and a trained research assistant who was blind for condition scored all videos for the number of benign attributions ($r = 0.90$), and resolved coding disagreements by discussion. We computed benign attribution scores as the average across the four stories ($\alpha = 0.89$).

Post-Assessment Directly following the experimental manipulation, children went to another room in their school to complete the post-assessment of hostile attribution bias (assessed in vivo and using vignettes) and aggression (assessed in vivo and by teachers). This session lasted 10–15 min and was conducted by a research assistant who was blind for condition.

Hostile attribution bias (vignettes). First, we measured children's hostile attribution bias using four vignettes that described similar ambiguous provocations as the pre-assessment vignettes (e.g., we described the situation of "being left out of play" for a computer game instead of a board game). We coded attributions conform the pre-assessment ($\kappa = 0.73$ across vignettes) and averaged them across the four vignettes to create a single hostile attribution bias score ($\alpha = 0.76$).

Hostile attribution bias and aggression (in vivo). We set up an in vivo provocation scenario to measure children's post-manipulation hostile intent attributions and aggression in an emotionally involving situation: Their toy was taken away by an alleged peer. Children had chosen this toy to receive as a gift before recording their video message. They had stored it in a name-labeled box, to be opened after completing the vignette task. Upon finding out their toy was gone, the experimenter neutrally stated: "That's strange… It's probably taken by the boy/girl who was here just now." The experimenter encouraged children to pick another toy, not responding to any questions children asked about the alleged peer.

First, we measured children's aggression using a sticker task (Slagt et al. 2017). The experimenter told children that the alleged peer would later receive some stickers, and told them that they could select the stickers their peer would receive. The experimenter handed them a box packed with stickers, saying: "Some stickers are torn, but you may as well pick those. Please select ten stickers and put them in this envelope," and then left the room. We computed aggression scores as the proportion of torn stickers that children allocated to their alleged peer. This measure has demonstrated moderate-to-strong stability over a 2-week interval (Spearman correlations between $0.35 < \rho < 0.79$) and is positively associated with relevant variables in samples of preschoolers, such as negative affect ($0.13 < \rho < 0.25$) and antisocial intentions ($0.24 < \rho < 0.25$) (Slagt et al. 2017). We found no correlation between this measure and teacher-rated aggression (Table 1), possibly reflecting a state/trait difference between the measures (Anderson and Bushman 1997).

Fig. 2 Picture book (girls). Ambiguous provocation story (presented on separate pages)

Table 1 Zero-Order Correlations of Study 1 Variables (Above Diagonal, $n = 83$) and Study 2 Variables (Below Diagonal, $n = 121$)

		1	2	3	4	5	6
1	Hostile attribution bias – Pre	–	0.32**	0.57***	0.20	0.31**	−0.11
2	Aggression (teacher-rated) – Pre [a]		–	0.21	0.68***	0.08	0.15
3	Hostile attribution bias – Post	0.28**		–	0.18	0.28**	0.06
4	Aggression (teacher-rated) – Post [a]				–	0.10	0.07
5	Hostile attribution bias (in vivo) [b]	0.07		0.22*		–	−0.03
6	Aggression (in vivo: torn stickers) [b]	0.06		−0.03		0.22*	–
7	Aggression (in vivo: evaluations) [c]	0.04		0.10		0.17	0.16

[a] Assessed only in Study 1 ($n = 79$); [b] Correlations based on $n = 81$ in Study 1 and $n = 118$ in Study 2. [c] Assessed only in Study 2 ($n = 114$). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Next, we measured children's attributions to their alleged peer using two questions. First, upon returning to the room, the experimenter asked: "I wonder why the boy/girl took your toy. What do you think?" We coded these responses conform the vignette assessments ($\kappa = 0.95$). Second, the experimenter asked: "Do you think he/she was being *unkind* or *not*?" We scored *hostile* and *unkind* responses as 1 and averaged them to create a single in vivo hostile attribution bias score. This score was significantly correlated with vignette-assessed hostile attribution bias, both at pre- and post-assessment (Table 1). Meta-analytical work suggests that assessments of children's hostile attribution bias using staged provocations yield strong correlations with aggressive behavior ($r = 0.55$, De Castro et al. 2002), supporting concurrent validity.

Last, we ensured that the provocation scenario was resolved: The experimenter with whom children had recorded the video message entered the room, explaining that she had mistakenly taken the toy. Children got back their toy and could select new stickers for their alleged peer.

Aggression (teacher-rated). One week after the manipulation, we invited teachers to complete the IRPA, concerning children's aggressive behavior in the last week ($\alpha = 0.80$). Teachers received a gift-card to thank them for their participation.

Results

Preliminary Analyses Table 1 presents zero-order correlations and Table 2 presents descriptive statistics for the Study 1 variables.

Data preparation. We used pairwise deletion to handle missing values (1.2%). We retained outliers ($z > 3.29$) in the analyses (results were virtually identical when excluding them). Most variables had a positively skewed distribution. Hence, in addition to parametric analyses, we report bias-corrected accelerated (BCa) bootstrap 95% confidence intervals (5000 samples).

Equivalence of experimental conditions. At pre-assessment, children in the self-persuasion and control condition did not significantly differ from each other with regard to grade, gender, age, vignette-measured hostile attribution bias, and teacher-rated aggression (all ps > 0.05), indicating successful randomization.

Manipulation check. The manipulation was effective (Table 2). Children in the self-persuasion condition made more benign attributions in their video message than children in the control condition, $F(1, 81) = 337.16$, $p < 0.001$, $\eta_p^2 = 0.81$.

Gender and age differences. Age was significantly correlated with vignette-assessed hostile attribution bias at pre-assessment ($r = -0.51$, $p < 0.001$) and post-assessment ($r = -0.38$, $p < 0.001$), teacher-rated aggression at pre-assessment ($r = -0.34$, $p = 0.002$), and in vivo hostile attribution bias ($r = -0.24$, r 0.031). We found no moderation by age for the primary analyses.

Boys allocated more torn stickers to the peer who had allegedly taken their toy than girls, $p < 0.001$, $\eta_p^2 = 0.12$. We observed no other gender differences, $0.078 < p$s < 0.861. We explored moderation by gender for all analyses, and report significant effects below.

Primary Analyses *Hostile attribution bias (vignettes).* We predicted that children in the self-persuasion condition would show stronger reductions in hostile attribution bias from pre- to post-assessment than would children in the control condition. A 2 (Time) × 2 (Condition) ANOVA supported this prediction, yielding a significant interaction effect (depicted in Fig. 3, left panel), $F(1, 80) = 19.80$, $p < 0.001$, $\eta_p^2 = 0.20$. Thus, the intervention effectively reduced children's hostile attribution bias in response to vignettes of ambiguous peer provocations.

Hostile attribution bias (in vivo). We predicted that children in the self-persuasion condition (vs. children in the control condition) would be less likely to attribute hostile intent to the peer who allegedly took their toy. However, an ordinal regression analysis did not support this prediction, $b(SE) = 0.31(0.46)$, $p = 0.507$, Nagelkerke $R^2 = 0.01$.

Aggression (in vivo: torn stickers). We predicted that children in the self-persuasion condition would allocate less torn stickers to the alleged peer provocateur than would children in the control condition. This prediction was not supported. Although a one-way ANOVA yielded a significant effect of

Table 2 Range, Means (M), Standard Deviations (SD), and Bootstrap 95% Confidence Intervals (95% CI) of the Study 1 Variables for Children in the Self-Persuasion and Control Condition Separately, and—for In Vivo Aggression—for Boys and Girls Separately

			Self-persuasion			Control			Self-persuasion	Control
		Range	n	M	SD	n	M	SD	95% CI	95% CI
Benign attributions in video message		0.00–2.25	43	1.62	0.36	40	0.25	0.32	[1.51; 1.72]	[0.16; 0.34]*
Hostile attribution bias (vignettes)	– Pre	0.38–1.00	43	0.58	0.18	40	0.58	0.18	[0.53; 0.63]	[0.53; 0.64]
	– Post	0.00–1.00	43	0.28	0.29	40	0.49	0.25	[0.19; 0.36]	[0.41; 0.56]*
Hostile attribution bias (in vivo)		0.00–1.00	43	0.20	0.33	38	0.22	0.30	[0.11; 0.30]	[0.14; 0.32]
Aggression (in vivo: torn stickers)		0.00–1.00	43	0.17	0.22	38	0.32	0.32	[0.10; 0.24]	[0.23; 0.42]†
	– Boys	0.00–1.00	27	0.22	0.24	23	0.43	0.30	[0.14; 0.31]	[0.32; 0.55]*
	– Girls	0.00–1.00	16	0.08	0.16	15	0.15	0.28	[0.02; 0.17]	[0.04; 0.30]
Aggression (teacher-rated)	– Pre	1.00–3.71	43	1.27	0.45	40	1.38	0.58	[1.16; 1.41]	[1.23; 1.55]
	– Post	1.00–3.14	40	1.15	0.26	39	1.26	0.47	[1.08; 1.24]	[1.14; 1.41]

Missing scores resulted from experimenter error ($n = 2$; in vivo measures) or from teachers failing to complete the questionnaire ($n = 4$; aggression-post).
* indicates that 95% CIs do not overlap, † indicates marginal overlap

condition, $F(1, 79) = 6.38$, $p = 0.014$, $\eta_p^2 = 0.08$, the aggression variable was highly skewed, and so we should rely on the nonparametric bootstrap 95% confidence intervals. The intervals of the two conditions slightly overlapped, indicating that the effect of condition was not significant (Fig. 3, right).

This result was qualified by gender, however. Using a 2 (condition) × 2 (gender) ANOVA, we found a significant interaction effect, $F(2, 77) = 7.41$, $p = 0.001$, $\eta_p^2 = 0.16$, indicating that the effect of the self-persuasion manipulation on in vivo aggression was significant for boys but not for girls (i.e., the bootstrap 95% confidence intervals for boys do not overlap, whereas they do for girls; see Table 2). Boys in the self-persuasion condition allocated almost half as many torn stickers to the alleged provocateur (22% of stickers) as did boys in the control condition (43% of stickers).

Aggression (teacher-rated). Fourth, we predicted that children in the self-persuasion condition would show stronger reductions in teacher-rated aggression from pre- to post-assessment than would children in the control condition. However, a 2 (time) × 2 (condition) ANOVA did not support this prediction: The interaction was not significant, $F(1, 77) = 1.18$, $p = 0.281$, $\eta_p^2 = 0.02$, and we found no gender moderation.

Discussion

Study 1 provides evidence that self-persuasion may be used effectively to reduce children's hostile attribution bias. Moreover, we found that, among boys, this approach may reduce aggressive behavior as assessed using a behavioral measure following an in vivo provocation. We observed no aggression reduction as reported by teachers over the course of the week following the self-persuasion manipulation.

Study 2

Study 2 builds on Study 1 in several ways: First, we wanted to replicate the Study 1 findings for hostile attribution bias. Second, given that in Study 1 we found little aggression among girls on the in vivo task, in Study 2, we investigated the effects of self-persuasion on a second in vivo task designed to assess relational aggression. Gender differences in the prevalence of relational aggression tend to be relatively small or non-existent (Card et al. 2008). Third, following persuasion theory, we tested whether self-persuasion leads to larger reductions in children's

Fig. 3 Effects of the self-persuasion manipulation on children's vignette-measured hostile attribution bias (left) and their allocation of torn stickers to an alleged peer provocateur (right). Error bars represent 95% bootstrap confidence intervals
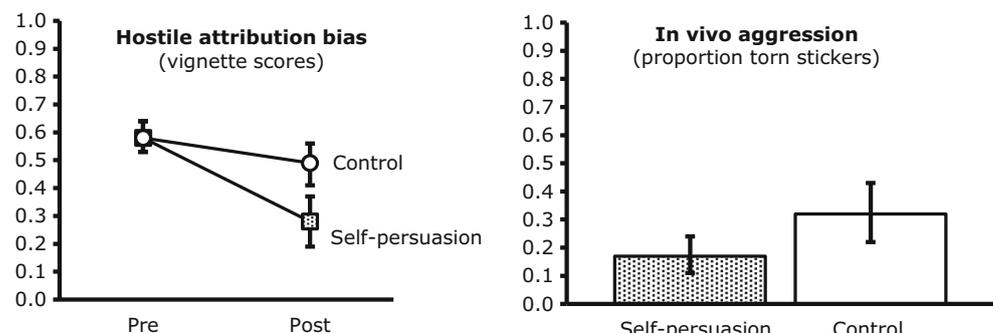
hostile attribution bias than persuasion by others. Direct attempts at persuasion may lead children to reject the intervention's message, whereas self-persuasion should promote ownership of the message (Aronson 1999; Atkins et al. 1967). By directly comparing these approaches, we aimed to better understand how attribution retraining can be effectively delivered.

Method

Participants Participants were 121 Dutch children aged 6–9 (49.6% boys; $M_{age} = 7.71$, $SD = 0.92$; 95.0% Caucasian), recruited from primary schools (first grade: $n = 47$; second grade: $n = 43$; third grade: $n = 31$). We selected them from a larger sample of 569 children (53.4% boys; $M_{age} = 7.83$, $SD = 0.92$; 90.2% Caucasian) for having high levels of hostile attribution bias (see *selection of participants*). The schools were located in nine municipalities (12,000–345,000 inhabitants) serving middle-class communities. A priori power was excellent (> 0.99 for $n = 120$) to replicate the hostile attribution bias effect obtained in Study 1 ($f = 0.50$), and sufficient (0.80 for $n = 120$) to replicate the main effect for condition on in vivo aggression scores ($f = 0.29$, as obtained for boys). Informed consent was obtained from one of the parents of all individual participants included in the study (consent rate = 54.9%).

Pre-Assessment The pre-assessment lasted 10 min and was conducted by the first author or one of eight research assistants. We interviewed children individually in a quiet room in their school and gave them stickers to thank them for their participation.

Hostile attribution bias (vignettes). We measured hostile attribution bias using the same vignette-procedure as in Study 1. All responses were coded by both the research assistant who conducted the assessment and the first author. Inter-coder reliability was good for all coders ($0.80 < \kappa < 0.96$). We scored *hostile* responses as 1, and averaged them across vignettes to create a single hostile attribution bias score ($\alpha = 0.66$).

Selection of Participants The large size of the screened sample allowed us to raise our Study 1 inclusion criterion. In Study 2, we selected children to participate in the experiment proper if they (1) scored ≥ 0.50 on hostile attribution bias, and (2) had sufficient task comprehension ($n = 5$ children did not). We excluded two children with an autism spectrum disorder diagnosis, and 15 other children were absent on the day of testing. In total, 121 children took part in the study. We randomly assigned them to the self-persuasion condition ($n = 41$), the other-persuasion condition ($n = 41$), or the control condition ($n = 39$).

Experimental Manipulation Approximately 1 month after the pre-assessment (range = 14–52 days) children took part in the experimental manipulation, which lasted 10–15 min and was conducted by the first author or one of four research assistants.

We used the same instructions for the self-persuasion and control condition as in Study 1. Instructions in the other-persuasion condition were identical to the self-persuasion condition, except that children were told "It is much better if pupils see how I explain these things to another pupil" and were asked to watch the experimenter record the video message. Thus, children in the self- and other-persuasion conditions heard the same intervention message, but only children in the self-persuasion condition advocated for this message themselves.

The other-persuasion manipulation took place as children prepared their video message. The experimenter encouraged children to describe the story, and then asked: "Why did the boy/girl [cause the provocation]?" If children mentioned hostile attributions (75.6% of children did so at least once), the experimenter restructured their hostile belief, saying: "Do you think so? Maybe he/she did not do it on purpose at all!" Next, irrespective of children's own attributions, the experimenter provided two benign attributions (e.g., for the tower story: "Maybe he just tried to help, or maybe the tower was just too high"). As in Study 1, children in the self-persuasion condition generated their own benign attributions ($M_{benign} = 1.32$, $SD = 0.40$; $M_{hostile} = 0.00$), and children in the control condition described the storyline.

Last, the video message was recorded. Children advocated why the story character may have had nonhostile intentions (self-persuasion condition), listened to the experimenter advocating this message (other-persuasion condition), or described the stories (control condition).

Manipulation check. A research assistant who was blind for condition coded the number of benign attributions in children's video messages. To assess inter-coder reliability, the first author also coded a subset of 20% of the videos ($r = 0.92$). We computed benign attribution scores as the average across the four stories ($\alpha = 0.92$).

Post-Assessment Directly following the experimental manipulation, children went to another room in their school. The post-assessment lasted 20–25 min and was conducted by one of four trained research assistants who were blind for condition.

Hostile attribution bias (vignettes). First, we measured children's hostile attribution bias using the same post-assessment vignettes as in Study 1. Inter-coder reliability was good for all coders ($0.81 < \kappa < 0.87$) and internal consistency reliability was sufficient ($\alpha = 0.64$).

Aggression (in vivo: torn stickers). We used the same in vivo provocation scenario as in Study 1, again assessing aggression as the proportion of torn stickers that children allocated to the peer who allegedly had taken their toy away.

Aggression (in vivo: negative evaluations). Next, children were provided with the opportunity to relationally aggress towards the alleged peer by providing a negative evaluation of the peer to a third party (i.e., participants' friend). This measure was modeled after the Negative Evaluation Task,

which is linked with hostile cognitions in adults (β = 0.35; DeWall et al. 2009), and was adapted for use with young children. The experimenter explained that the alleged peer and their friend would be cooperating on a task, and would therefore need some information about each other. The experimenter asked children to indicate for both their friend and the alleged peer how "nice" and "stupid" they were. First, children rated their friend on a note containing two 4-point scales, for each scale circling one of four squares of increasing size. Next, they rated the alleged peer, and then put the note in an envelope to be passed on to their friend before meeting the alleged peer. Children's "stupid" ratings for the alleged peer indexed aggression (scale 0–3).

Hostile attribution bias (in vivo). We measured children's attributions of the alleged peer's behavior using the same two questions as in Study 1. In Study 2, if children did not reply to the open-ended attribution question, we asked a follow-up question: "Did the boy/girl not know that the toy was yours, or did he/she just take it?" Inter-coder reliability was sufficient for all coders (0.64 < κ < 1.00). We scored *hostile* and *unkind* responses as 1 and averaged them to create a single hostile attribution bias score. This score was significantly positively associated with vignette-assessed hostile attribution bias at post-assessment (Table 1).

Last, the provocation scenario was resolved: One experimenter explained that she had mistakenly taken the toy, whereupon the other experimenter tossed the evaluation notes and gave children the opportunity to select new stickers for their alleged peer. Children also completed a 4-item questionnaire about their self-perceived competence for making intent attributions, but this scale was dropped from the analyses because it was unreliable (α = 0.40).

Results

Preliminary Analyses Table 1 presents zero-order correlations and Table 3 presents descriptive statistics for the Study 2 variables.

Data preparation. We used pairwise deletion to handle missing values (1.8%). There were no outliers ($z > 3.29$). Most variables had a positively skewed distribution. Hence, in addition to parametric analyses, we report bias-corrected accelerated (BCa) bootstrap 95% confidence intervals (5000 samples).

Equivalence of experimental conditions. At pre-assessment, children in the self-persuasion, other-persuasion, and control conditions did not significantly differ with regard to gender, age, and hostile attribution bias (all $ps > 0.05$), indicating that randomization was successful.

Manipulation check. The manipulation was effective (Table 3). Children in the self-persuasion condition made more benign attributions in their video message than children in the control and other-persuasion conditions, $F(2, 118) = 271.46$, $p < 0.001$, $\eta_p^2 = 0.82$.

Table 3 Range, Means (M), Standard Deviations (SD), and Bootstrap 95% Confidence Intervals (95% CI) of the Study 2 Variables for Children in the Self-Persuasion, Other-Persuasion, and Control Condition Separately

	Range	Self-persuasion			Other-persuasion			Control			Self-pers.	Other-pers.	Control
		n	M	SD	n	M	SD	n	M	SD	95% CI	95% CI	95% CI
Benign attributions in video	0.00–3.25	41	1.47	0.50	41	0.00	0.00	39	0.15	0.19	[1.32; 1.63]		[0.10; 0.21]*
Hostile attribution bias (vignettes)													
– Pre	0.50–1.00	41	0.60	0.15	41	0.62	0.14	39	0.58	0.11	[0.56; 0.66]	[0.58; 0.66]	[0.55; 0.62]
– Post	0.00–1.00	41	0.25	0.22	41	0.26	0.21	39	0.52	0.21	[0.19; 0.32]	[0.20; 0.33]	[0.45; 0.59]*
Hostile attribution bias (in vivo)	0.00–1.00	40	0.33	0.45	40	0.29	0.39	38	0.47	0.45	[0.19; 0.46]	[0.17; 0.41]	[0.33; 0.62]
Aggression (in vivo: torn stickers)	0.00–1.00	40	0.27	0.19	40	0.27	0.26	38	0.32	0.24	[0.22; 0.33]	[0.19; 0.35]	[0.24; 0.40]
Aggression (in vivo: evaluations)	0.00–3.00	37	1.08	0.80	39	0.97	0.78	38	1.37	0.94	[0.83; 1.33]	[0.74; 1.23]	[1.08; 1.67]

Missing scores resulted from experimenter error ($n = 3$; in vivo measures) and from children not understanding the task ($n = 4$; relational aggression). * indicates that 95% CIs of the self- and other-persuasion versus control condition do not overlap

Gender and age differences. We observed no gender differences for any of the study variables. Age was significantly correlated with in vivo hostile attribution bias ($r = 0.22$, $p = 0.016$) but not with other variables. We found no moderation effects of gender or age in the primary analyses.

Primary Analyses

Hostile attribution bias (vignettes). We used planned contrasts to test whether children in the self-persuasion condition showed stronger reductions in hostile attribution bias from pre- to post-assessment compared to (1) the control condition, and (2) the other-persuasion condition. The 2 (Time) × 2 (Condition) ANOVA yielded the predicted interaction effect, $F(2, 118) = 27.10$, $p < 0.001$, $\eta_p^2 = 0.32$. Planned contrasts revealed that children in the self-persuasion condition showed stronger reductions in hostile attribution bias than did children in the control condition ($p < 0.001$), thus replicating the main finding of Study 1. However, they did not show stronger reductions in hostile attribution bias than did children in the other-persuasion condition ($p = 0.771$). Self- and other-persuasion were equally effective at reducing children's hostile attribution bias (Fig. 4, left).

Hostile attribution bias (in vivo). We used an ordinal regression analysis to test whether children in the self-persuasion condition were less likely to attribute hostile intent to the peer who allegedly took their toy than children in each of the other conditions. Using dummy variables to code these contrasts, we found no significant differences in children's in vivo hostile attributions between the self-persuasion and control condition, $b(SE) = 0.71(0.44)$, $p = 0.106$, or the other-persuasion condition, $b(SE) = 0.08(0.45)$, $p = 0.861$.

Aggression (in vivo: torn stickers & negative evaluations). We used planned contrasts to test potential differences between conditions in in vivo aggression, assessed both as allocating torn stickers and as spreading negative evaluations. We found no such differences. For allocating torn stickers, the ANOVA showed no condition effect, $F(2, 115) = 0.55$, $p = 0.578$, $\eta_p^2 < 0.01$. Planned contrasts showed no significant differences between the self-persuasion condition versus the control condition ($p = 0.391$), or the other-persuasion condition ($p = 0.923$). Similarly, for spreading negative evaluations,

the ANOVA showed no effect of condition, $F(2, 111) = 2.25$, $p = 0.111$, $\eta_p^2 = 0.04$. Planned contrasts showed no significant differences between the self-persuasion and the control ($p = 0.194$), or other-persuasion ($p = 0.582$) conditions (Fig. 4, right).

Discussion

Study 2 provides evidence that self-persuasion and other-persuasion are equally effective at reducing children's hostile attribution bias. We observed no effects of self- or other-persuasion on children's in vivo aggression.

General Discussion

Reducing children's hostile attribution bias is challenging. Although attribution retraining techniques can effectively reduce children's aggressive behavior (Wilson and Lipsey 2006), little is known of how such retraining is best delivered. Persuasion theory suggests that children may readily reject the message that "other people may have nonhostile intentions," to the extent that they perceive this message as too discrepant from their own beliefs (Atkins et al. 1967). To investigate this issue, two experiments tested the potential effectiveness of self-persuasion to reduce children's hostile attribution bias; an approach that, as research in adults has shown, can be used effectively to persuade people of belief-discrepant messages (Fazio et al. 1977).

Both experiments (Study 1 and 2) showed that self-persuasion effectively reduced hostile attribution bias as assessed using a well-established vignette-based procedure. Study 2 further showed that self-persuasion was equally effective at reducing hostile attribution bias as was persuasion by others. Whereas we expected self-persuasion to be more effective than persuasion by others, this finding suggests that the two attribution retraining approaches may be valuable complementary strategies. Our experiments show that self-persuasion leads to short-term reductions in young children's hostile attribution bias. Future research is needed to investigate the potential long-term effects and explore the relevance of self-persuasion for clinical practice.

Fig. 4 Effects of the self-persuasion and other-persuasion manipulation on children's vignette-measured hostile attribution bias (left) and their negative evaluations of the alleged peer provocateur (right). Error bars represent 95% bootstrap confidence intervals
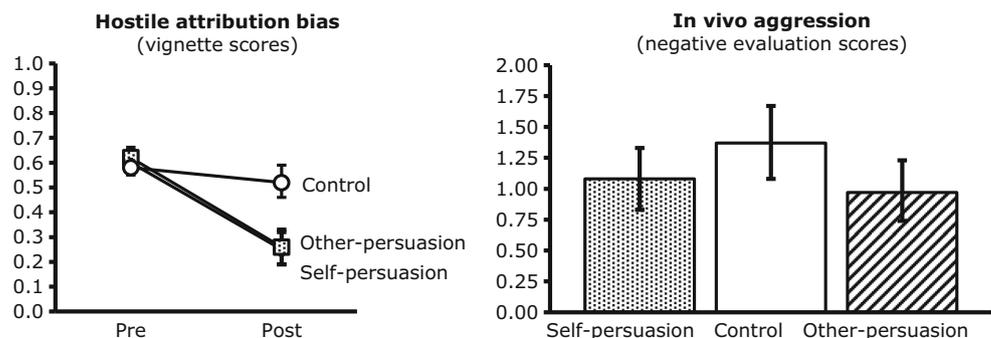
We also investigated whether self-persuasion would decrease children's aggression. The effects were limited. Study 1 showed that boys, but not girls, who recorded a self-persuasion message (vs. a control message) were less likely to engage in in vivo aggression (i.e., allocating torn stickers to a peer who allegedly took their toy). However, Study 2 did not replicate this effect, nor showed effects on a second in vivo aggression task designed to tap relational aggression (i.e., spreading negative evaluations of the alleged peer). These findings suggest that single instances of self- or other-persuasion may affect children's cognition more than their behavior—which is further supported by the fact that we found no effects of self-persuasion on teacher-reported aggression (measured in Study 1). It is possible that more extensive persuasion techniques are required to effectuate change in children's ingrained behavior patterns, perhaps particularly in emotionally engaging situations such as losing a valued gift. Future research will need to test this possibility, and address the cognition-behavior discrepancy in our findings.

The self-persuasion effect on hostile attribution bias as assessed using the vignette-based paradigm did not generalize to children's hostile attributions for the in vivo, real-time peer provocation scenario. Generalization to ecologically valid settings is important, but it is also challenging. Our in vivo attribution measure was correlated with vignette-assessed hostile attribution bias, supporting its convergent validity. That said, we assessed children's attributions using two questions only, to avoid the possibility that children would infer that the provocation was staged. The downside of this approach is that it reduced assessment sensitivity, which might have accounted for this null-result.

Our results inform self-persuasion theory, which predicts that self-persuasion will often be more powerful at changing beliefs than persuasion by others (Aronson 1999). Although self-persuasion was generally effective at changing children's hostile attributions, we found no evidence that it was *more* effective than persuasion by others. One possible explanation is that the limitations of persuasion by others are less potent in young children compared to adults. Research in adults has shown that individuals are more likely to reject a persuasive message if they accept a limited range of views as compatible with their own (Eagly and Telaak 1972). However, children may generally have wider latitudes of acceptance. As they have just started to construct their knowledge of the social world, they may accept a wider range of information to be true (Crick and Dodge 1994). Accordingly, the relative effectiveness of self-persuasion compared to persuasion by others might increase as children grow older—a possibility that should be tested in future work.

Although we found that self-persuasion and other-persuasion were equally effective in reducing children's hostile attribution bias, self-persuasion may have additional attractive features. First, self-persuasion affords agency to children: Children are helping others, rather than receiving help from others. As such, self-persuasion is likely to benefit children's sense of autonomy or competence, which may contribute to their well-being (Ryan and Deci 2000). This may be especially relevant when treating hostile attribution biases in aggressive children, who tend to resist influence by authority figures and prefer taking on agentic roles (Ellis et al. 2016; Salmivalli et al. 2005). Second, self-persuasion encourages children to reflect on the possibility of making benign attributions, rather than "correcting" their hostile attributions. As such, self-persuasion has the advantage that it does not trigger children's existing hostile knowledge base (Anderson and Bushman 2002), nor affirm their identity of being someone who misperceives social situations (Swann Jr. 2011).

Our research has both strengths and limitations. Strengths include our replication of the self-persuasion effect on hostile attribution bias across studies, and the inclusion of an active control condition, which enabled us to rule out potential alternative explanations of the study findings (such as that children's hostile attribution bias decreased due to repeated exposure to social provocation scenarios). A limitation is that the self-persuasion manipulation and hostile attribution bias assessments inevitably covered the same content (i.e., ambiguous social provocations). Although we used unique story themes and different visual presentation formats, we cannot rule out the possibility that demand effects at post-assessment have contributed to the observed effects. Further, although we sampled children with high scores on hostile attribution bias, we did not target children with clinically elevated levels of aggressive behavior. In theory, these children might benefit more from self-persuasion, because they tend to have stronger hostile attribution biases (Dodge 2006), and may thus be particularly resistant to persuasion by others (Schlenker and Trudeau 1990). This idea may be tested in future research.

In sum, our experiments suggest that self-persuasion may be an effective approach to reduce hostile attribution bias in young children. We hope this research will encourage efforts to further investigate self-persuasion techniques as a promising intervention approach, and help young children to persuade themselves away from their biased hostile attributions.

Compliance with Ethical Standards All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

Informed Consent was obtained from one of the parents of all individual participants included in the study.

Conflict of Interest The authors declare that they have no conflict of interest.

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