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Nonverbal Rhetoric:

Two- to Four-year-old Children Select Relevant Evidence When Trying to Influence Others

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Author Note

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Abstract

This paper investigates the early emergence of reason-giving. Toddlers are sensitive to disagreements, and they can track several kinds of informational access, such as visual perception. We investigated whether young children use these skills (i) when assessing whether providing reasons is needed and (ii) when selecting appropriate behaviors to support their claims. An experimenter disagreed with 2- to 4-year-old children ($N = 71$) about the location of a toy placed in one of four boxes. In a Fully Transparent condition, the boxes were transparent, and the toy was visible to the experimenter and to the participant. In a Window condition, the boxes were partially opaque, and the toy was initially hidden from the experimenter but visible to the participant through a transparent window. In this condition, toddlers could make the toy visible to the experimenter by rotating the baited box.

Participants in the Window condition were more likely to rotate the baited box than those in the Transparent condition. Thus, children were more likely to rotate the box when this action was an efficient way of supporting their claims by revealing new and relevant information to the experimenter. These results demonstrate the presence of precursors of crucial skills required for reason-giving and reveal that from 2 years of age, children do not use fixed persuasion strategies. Instead, they select relevant evidence when attempting to influence others.

Keywords: Argumentation — Disagreement — Reasoning — Naïve epistemology—
Communication – Justification

Nonverbal Rhetoric:

Two- to Four-year-old Children Select Relevant Evidence When Trying to Influence Others

Psychologists have long noted how early some reason-giving and reason-evaluating capacities develop and the crucial cognitive role that these abilities play in development. Piaget claimed that “logical reasoning is an argument which we have with ourselves, and which reproduces internally the features of a real argument” (Piaget, 1928, p. 204; see also Doise & Mugny, 1984; Perret-Clermont, 1980; Vygotsky, 1978). More recent theories have stressed the evolutionary importance of argumentative and, more generally, reason-giving and reason-evaluating skills, suggesting that these skills should develop early (Mercier & Sperber, 2011, 2017; Tomasello, 2014). Yet, to this date, we do not know how early reason-giving develops. To address this developmental issue, we test the existence and nature of reason-giving during toddlerhood, using a non-verbal paradigm.

Recent experiments suggest that preschoolers’ reason-giving already shows two signatures of argumentative skills: (i) a capacity to recognize when producing reasons is needed and (ii) some ability to select appropriate reasons. In one experiment, separate pairs of 3- and 5-year-olds played a game together (Köymen, Rosenbaum, & Tomasello, 2014). In this game, some moves were more conventional than others. Both age groups were more likely to provide reasons when making unconventional than conventional moves. This finding shows that preschoolers are already attuned to whether a situation warrants providing reasons. Another experiment suggested that preschoolers possess some capacity to select reasons that are effective and convince others. In Köymen, Mammen, and Tomasello (2015), 3- and 5-year-olds were able to adjust their reason-giving to the state of knowledge of their interlocutor. Children were provided with a good reason to perform an unconventional action,

either as pairs or individually, and they were then asked to perform the unconventional action, always as pairs. When the children were paired with another child who had heard the reason at the same time as them, they were less likely to use this reason in their interaction, compared to when they were paired with another child who had not heard the reason at the same time as them. Preschoolers could thus use common knowledge between themselves and their interlocutor, or lack thereof, to adjust their reason-giving strategies (see also Mammen, Köymen, & Tomasello, 2018).

In sum, preschoolers are sensitive to whether reasons are needed and they provide their interlocutors with relevant reasons when attempting to influence them. The goal of this article is to investigate whether younger children's nonverbal communicative behaviors already show these two signatures of reason-giving skills. As we argue presently, many of the basic skills required to produce reasons are likely to develop early during infancy and toddlerhood.

Recognizing When Reasons Are Needed

Reason-giving is most appropriate when we believe our audience has ignored, doubted, or disagreed with what we have communicated—or when we deem our audience likely to do so. Two-year-olds already have some sensitivity to such disagreements. For instance, they contradict adults who name objects incorrectly, often by using negation (Choi, 1988; Hummer, Wimmer, & Antes, 1993; Kim, 1985; Koenig & Echols, 2003; Pea, 1982; Rakoczy & Tomasello, 2009). Toddlers and preschoolers also react to third-party assessments of propositions. They prefer to learn what an informant communicates if other people assent rather than dissent to this testimony (Fusaro & Harris, 2008, 2013). Moreover, toddlers and children recognize that someone might explicitly deny the truth of a proposition. This ability is most evident in their capacity to process truth-functional negations (Austin, Theakston, Lieven, & Tomasello, 2014; Feiman, Mody, Sanborn, & Carey, 2017; Hill, Collier-Baker, &

Suddendorf, 2012; Mascaro & Morin, 2015; Mascaro & Sperber, 2009). In short, a sensitivity to explicit disagreements about the truth of a proposition is already present during toddlerhood. As a result, explicit markers of disagreement such as denials are likely to help children identify when they need reasons to influence others, at least from toddlerhood onwards.

Selecting Efficient Means of Influencing Others

To influence others, one needs more than recognize when they disagree (or might disagree) with one's claims. One also needs to select behaviors that can efficiently influence others, an ability that is likely to improve with the development of representations of mental states (also known as mentalizing or theory of mind, Premack & Woodruff, 1978). Indeed, children's use of others' mental states when making influence attempts improves throughout childhood when practicing deception (Lee, 2013; Mascaro & Morin, 2014; Mascaro, Morin & Sperber, 2017), and honest persuasion (Bartsch & London, 2000; Bartsch, Wade & Estes, 2011). Furthermore, the amount and quality of children's attempts in persuasion tasks correlates positively with their capacity to predict explicitly agents' behavior on the basis of their beliefs (Bartsch, London & Campbell, 2007, Study 2; Kołodziejczyk & Bosacki, 2015; Lonigro et al., 2017; Peterson, Slaughter & Wellman, 2018; Slaughter, Peterson & Moore, 2013). In short, available evidence suggests that the development of mentalizing abilities constrains the set of persuasion strategies that are available to children. This hypothesis makes a straightforward developmental prediction: the first reason-giving behaviors to emerge are likely to be those that involve precociously developing mentalizing abilities.

One of the most primary forms of mentalizing abilities is the capacity to recognize informational access (e.g., Martin & Santos, 2016; Wellman, 1992). Infants and toddlers have some sensitivity to the factors that influence what others know. They recognize that communicative actions can inform others (Liszkowski, Carpenter, Striano, & Tomasello,

2006; Martin, Onishi, & Vouloumanos, 2012; Song, Onishi, Baillargeon, & Fisher, 2008; Vouloumanos, Martin, & Onishi, 2014; Vouloumanos, Onishi, & Pogue, 2012; for a review see Harris & Lane, 2014). Furthermore, infants and toddlers recognize that visual perception provides people with information about their surroundings (Luo & Baillargeon, 2007; Luo & Johnson, 2009; Moll & Tomasello, 2006; Sodian, Thoermer, & Metz, 2007; Southgate, Senju, & Csibra, 2007). For example, in Moll and Tomasello (2006), toddlers interacted with an adult searching for an object. One candidate object was hidden from the adult but was visible to the participant. The other candidate object was visible to both the participant and the adult. When asked to help the adult find the object that she was looking for, 24-month-old toddlers gave the adult the object that was hidden from her. These results suggest that toddlers have some sensitivity to what other people can see. Thus, infants and toddlers recognize some of the conditions enabling informational access in others. This capacity is likely to support the development of the first reason-giving behaviors geared towards influencing others.

In sum, two basic capacities required to produce reasons efficiently are present well before children can recruit them in a verbal context: a sensitivity to disagreements and expectations about the kind of experiences (such as seeing) that can inform others. Here, we investigate whether these two abilities support the early development of reason-giving in toddlers. No experimental study has specifically tested reason-giving at such an early age, in part because previous studies relied on measures of verbal production. To be suitable for very young children, our study relies on nonverbal behaviors, a possible precursor of verbal reason-giving.

Operationalization Principle

In our study, we tested whether toddlers adjust their nonverbal behaviors to the context when trying to provide support for their claims. We manipulated whether the exact same action (rotating a box) was relevant by varying the presence of explicit disagreement.

An experimenter asked toddlers to indicate the location of a box containing a toy. Participants could provide their answer by pointing at the correct box or by grasping it. They could also manipulate the position of the boxes by rotating them. In the Window condition, a box containing a toy was opaque except for a transparent window that faced the participants but was hidden from the experimenter. Thus, participants could provide additional evidence about the toy's location by rotating the box and making the toy visible to the experimenter through the transparent window. The Fully Transparent condition used the same procedure as the Window condition except that the box containing the toy (as well as the other boxes) was entirely transparent, making the toy visible to the experimenter at all times. Thus, in this condition, participants could not provide additional evidence about the location of the toy by rotating the box.

During the baseline phase, there was no reason for participants to assume that the experimenter disagreed with them. In contrast, during the test phase, the experimenter contradicted participants after they indicated where the toy was located. We hypothesized that during the test, the participants would rotate the box more often when this action was relevant to provide evidence for their claims (i.e., in the Window condition). We chose to test 2- to 4-year-old participants because we wanted to (i) assess reason-giving at the earliest age possible, and (ii) evaluate possible developmental effects. Since we used denials in our experimental setup, we did not test one-year-olds because, at that age, the understanding of truth-functional negation is not well consolidated (Austin, Theakston, Lieven, & Tomasello, 2014; Feiman, Mody, Sanborn, & Carey, 2017).

Material and Methods

Participants

Two groups of 2- to 4-year-old children participated in this study (Window condition: $n = 35$, $M_{age} = 35.51$ months, $range = 23-48$ months, 14 girls; Fully Transparent condition: n

= 36, $M_{age} = 35.67$ months days, $range = 23-48$ months, 13 girls). Eleven additional participants were excluded ($M_{age} = 31.27$ months, $range = 24-41$ months, 6 girls) because they did not understand the task, i.e., they did not answer correctly by pointing towards or reaching for the location of the dog in at least three out of the four baseline trials (5), because of technical failure (1), or because participants stopped interacting with the experimenter after being contradicted (5). Our rejection rate (11 participants out of 82, 13%) is similar to what has been observed in comparable studies (e.g., Moll & Tomasello, 2006; Moll & Meltzoff, 2011). However, it should be noted that our sample is representative only of the children kept in the analysis, and who were thus able to understand the task and willing to engage with the experimenter. The mean age of the excluded participants tended to be lower than the mean age of the participants in the Window condition ($t(44) = 1.78, p = .082, d = .69$) and in the Fully Transparent condition ($t(45) = 1.78, p = .071, d = .72$), presumably because the youngest of our participants were more likely to fail to understand the task.

The research reported in this manuscript followed the guidelines of the Declaration of Helsinki and it was approved by the local board of daycare centers involved in the project. The written informed consent of participants' caregivers was obtained prior to the study. Institutional Review Board approval from ethics committees for bio-medical research was not sought because this project involves purely behavioral measures, and it falls out of the scope of biomedical research (Title: Non-verbal rhetoric; Institution: Institute for Cognitive Sciences Marc Jeannerod). The participants were recruited from urban daycare centers and preschools in a large French city. They came from mixed socioeconomic backgrounds and were native French speakers. They were randomly assigned to one of the two conditions (Window vs. Fully Transparent).

We aimed at achieving the largest possible sample size, given our recruitment opportunities. Therefore, we tested all the available participants within our target age range in

the schools that we contacted. To assess the resulting power to detect a difference between participants' number of rotations of the box across conditions (Window vs. Fully Transparent) with a Fisher Exact test, we ran a compromise power analysis with G-Power 3.1 (Faul, Erdfelder, Lang & Buchner, 2007). This analysis postulated a low probability to rotate the box during the trials of the Fully Transparent condition ($p_r = .1$) and a large effect size ($\alpha = .05$, $\beta = .05$, $OR = 6.71$, Chen, Cohen & Chen, 2010). It revealed that our sample size yielded an implied power equal to .90.

Materials and Procedure

Participants were tested in a quiet room at their daycare center. The main experimenter was aware of the study's hypotheses (as in many comparable experiments, e.g., Moll & Tomasello, 2006; Warneken & Tomasello, 2006). A daycare employee familiar to the participants, and unaware of the hypotheses of the study, acted as a confederate and was present in the room during the entire experiment. Both the experimenter and the confederate were instructed not to influence participants' choices during the warm-up and test phases. One camera (temporal resolution = 25 frames/second) recorded participants' behaviors. Toddlers sat in front of the experimenter, across a child-size table. The experimenter said: "*Look, I brought little boxes to play with.*" She placed four plastic boxes (8 x 6 x 5 cm) on a rectangular cardboard tray (60 x 27 cm) positioned in the center of the table, approximately 30 cm from the participants. Three of the boxes were empty; a small plastic dog was glued inside the fourth box. Boxes were closed with lids, and they were locked so that participants could not open them easily.

Window condition

In the Window condition, each box was entirely covered with opaque white paint except for a transparent window (4.5 x 4.5 cm) located on its front side. The experimenter placed the boxes so that their opaque sides faced her, while the transparent windows faced

the toddlers. Participants could thus see the boxes' contents—including where the dog was—at all times. In contrast, it was not possible to see the boxes' content from the experimenter's point of view.

The experimenter explained: "*Let's play a game. You have to find the little dog.*" Then, during each baseline trial, the experimenter asked the prompt question: "*Where is the dog?*" We evaluated the participants' answer to the prompt question in the same way in the baseline, pre-test, and test trials. In all cases, participants could answer by pointing at or reaching for one of the boxes. For each trial, we considered that the participants selected accurately the box containing the toy when they pointed at it, reached for it, or rotated it. When a child's answer to the prompt question was ambiguous or directed towards more than one box, the experimenter repeated the prompt question until the participant selected only one box. When participants pointed at or reached for an empty box in response to the prompt question, we assumed that they failed to select the correct box. During the baseline trials, when the participant failed to indicate the correct box, the experimenter pointed at the baited box and said: "*Here is the dog.*"

In the baseline, pre-test and test trials, we also recorded whether participants answered the prompt question by rotating the box containing the dog so that its front part (i.e., the transparent window in the Window condition) faced the experimenter. To do so, participants had to rotate the box clockwise or counterclockwise around its vertical axis more than 90° from its position at the beginning of the experiment (thus making it possible for the experimenter to see the dog from her viewpoint in the Window condition). In a given trial, a participant was allowed to both point at and rotate the same box (no participant pointed at one box and rotated another one).

Once the child had pointed at or reached for the box containing the plastic dog, the experimenter proceeded to the next baseline trial. At the end of each baseline trial, the

experimenter swapped the location of the box containing the dog with the location of one of the empty boxes. There were four baseline trials. For each trial, the experimenter placed the box containing the dog in a different position, using the following fixed order: right end of the row (trial 1), third position from the right end of the row (trial 2), left end of the row (trial 3), and second position from the right end of the row (trial 4). The left and right sides referenced here are from the participants' viewpoint. The experimenter displaced the box containing the dog prior to each of the baseline trials.

After the fourth baseline trial, the experimenter placed the box containing the dog in the second position from the right end of the row of boxes. She reached for her cell-phone in her pocket while saying "*Oh, I have a phone call, I'll be right back*" before leaving the room. While she was away, the confederate took the box containing the dog and placed it at the left end of the row. After staying outside the room for approximately 15 seconds, the experimenter came back in the testing room. She sat across the table in front of the participant and said: "*Let's continue the game!*" and the pre-test trial began. The experimenter asked: "*Where is the dog?*" Once participants had pointed at, reached for, or rotated one of the boxes, the experiment proceeded to the test phase. The first test trial started by the experimenter contradicting participants by saying: "*No... the dog is not there...*" before asking the prompt question again: "*Where is the dog?*" Once the participant had pointed at, reached for, or rotated one of the boxes, the experimenter proceeded to the next test trial. Unlike in the baseline trials, during the pre-test and test trials, the experimenter did not indicate the real location of the dog to the participants when they failed to select the correct box. Each test trial started by the experimenter using a specific sentence to contradict the participants. During the second trial she contradicted participants by saying: "*Is the dog there? No, I don't think so,*" and during the third trial, she contradicted participants by saying: "*Well, it's not there!*" During the test trials, the boxes were not manipulated by the

experimenter or by the confederate. The experimenter simply contradicted the participant three times (once per test trial), each time after the child answered the question about the location of the dog (“*Where is the dog?*”). We used only three test trials because we were worried that our participants might stop responding after being contradicted too many times. The fact that the boxes could be rotated was never demonstrated to the participants.

Fully Transparent condition

The Fully Transparent condition followed the same procedure as the Window condition, except that the plastic boxes used in the game were entirely transparent. Thus, both the participant and the experimenter could see the location of the plastic dog at all times.

Coding and analysis

For each baseline and test trial, we coded whether participants accurately indicated the location of the dog, by pointing at or by reaching for the baited box. For each participant, we computed the proportion of trials in which they correctly indicated the location of the dog, resulting in three accuracy ratios ranging from 0 to 1, one for the baseline trials, one for the pre-test trial, and one for the test trials. We also assessed whether participants rotated the box containing the dog during each baseline trial, during the pre-test trial, and during each test trial. Each participant scored 1 for each trial in which they rotated the box, for a total number of potential rotations per participant ranging from 0 to 4 during baseline trials and from 0 to 3 during test trials.

Data were coded offline. A primary coder aware of the study’s hypotheses coded the entire dataset, and a secondary coder unaware of the study’s hypotheses double-coded a subset of the data ($n = 26$). The inter-rater agreement was high for all our measures (average $\rho = .93$, $range = .87-.99$; agreement on whether the participants rotated the box or not during the test trials: $\kappa = 1$). We analyzed the data from the primary coder. All the statistical analyses reported in this paper are two-tailed. We performed Mann-Whitney U tests by

compiling the null distribution of the rank-sum statistic using exact permutations (Bergmann, Ludbrook, & Spooren, 2000). Statistics were computed using R 3.4.3 (R Core Team, 2018), with the following packages: *lme4* (v1.1-19; Bates, Maechler, Bolker, & Walker, 2015), *exactRankTests* (v0.8-29; Hothorn & Hornik, 2017), and *coin* (v1.2-2, Hothorn, Hornik, van de Wiel & Zeileis, 2008).

Results

Preliminary analyzes revealed that Condition (Window vs. Fully Transparent) had no effect on participants' accuracy ratios during baseline trials ($M = .96$, $Mdn = 1$, $SD = .09$ vs. $M = .96$, $Mdn = 1$, $SD = .09$, $U = 1275$, $Z = .27$, $r = .03$, $p = 1$, Mann-Whitney U test), pre-test trials ($M = 1$, $Mdn = 1$, $SD = 0$ vs. $M = 1$, $Mdn = 1$, $SD = 0$) or during test trials ($M = .89$, $Mdn = 1$, $SD = .18$ vs. $M = .88$, $Mdn = 1$, $SD = .17$, $U = 1283$, $Z = .33$, $r = .04$, $p = .76$, Mann-Whitney U test). In short, there was no detectable difference in participants' ability to locate the dog across conditions. The participants' accuracy ratios were significantly lower during the test trials than during the baseline trials in both conditions (Window condition: $W_+ = 105$, $W_- = 15$, $Z = -2.56$, $r = .31$, $p = .01$; Fully Transparent condition: $W_+ = 118$, $W_- = 18$, $Z = -2.59$, $r = .31$, $p = .01$, Wilcoxon tests for matched pairs). Similarly, the participants' accuracy ratios were significantly lower during the test trials than during the pre-test trial in both conditions (Window condition: $W_+ = 0$, $W_- = 55$, $Z = -2.80$, $r = .33$, $p = .005$; Fully Transparent condition: $W_+ = 0$, $W_- = 78$, $Z = -3.06$, $r = .36$, $p = .002$, Wilcoxon tests for matched pairs). These results reveal that the participants reacted to being contradicted by the experimenter, and that they were sensitive to the negative evaluation of their answer by the experimenter.

Furthermore, Condition (Window vs. Fully Transparent) had no effect on participants' tendencies to rotate the boxes during the baseline trials (4 participants out of 35 rotated the box at least once in the Window condition, 1 participant out of 36 did so in the Fully

Transparent condition, $OR = 4.52$, $p = .199$, Fisher Exact test) or during the pre-test trial (3 participants out of 35 rotated the box at least once in the Window condition, and 3 participants out of 36 did so in the Fully Transparent condition, $OR = .69$, $p = 1$, Fisher Exact test). Thus, when participants were not contradicted by the experimenter, their tendency to rotate the boxes was comparable—and very low—across conditions. Detailed descriptive statistics for the number of rotations per condition during the baseline, pre-test and test phase are reported in Table 1.

Descriptive Statistics for the Number of Rotations

		Window condition			Fully Transparent condition		
		Baseline	Pre-test	Test	Baseline	Pre-test	Test
Number of rotations	Mean	0.14	0.09	0.8	0.03	0.08	0.19
	Mdn	0	0	0	0	0	0
	SD	0.43	0.28	1.02	0.17	0.28	0.52
	SE	0.07	0.05	0.17	0.03	0.05	0.09
	Min	0	0	0	0	0	0
	Max	2	1	3	1	1	2
	Skew	2.95	2.83	0.87	5.51	2.89	2.52

Table 1. Descriptive Statistics for the Number of Rotations During the Baseline, Pre-test and Test Trials per Condition (Window vs. Fully Transparent).

Next, we analyzed whether participants rotated the box containing the dog during each trial of the test phase as a binary outcome, using Generalized Linear Mixed Models (GLMMs) with a binomial distribution (logit link). To explicitly take into account repeated measures, all mixed models included participant identity as a random intercept. We fit two different kinds of models: (1) the null models, featuring participant identity as the only

predictor, and (2) the hypothesis-driven models, which included Condition as an additional factor. We used likelihood ratio tests (LRTs) to compare the overall fit of each of these models. For the estimates of the model parameters, the 95% confidence intervals were assessed by computing a likelihood profile and finding the appropriate cutoffs based on the LRT.

We built the hypothesis-driven model by including participant identity as a random intercept, and the fixed effect of Condition (Window versus Fully Transparent). The hypothesis-driven model provided a better fit than the null model (LRT: $\chi^2(1) = 8.93$, $p = .003$), thus revealing that during the test, the participants rotated the target box more often in the Window condition than in the fully Transparent condition ($\beta = 1.24$, $SE = .44$, $95\%CI: .46-2.30$, $Z = 2.80$). Non-parametric analyses confirmed this result (the number of participants that rotated the box at least once was 16 out of 35 in the Window condition vs. 5 out of 36 in the Fully Transparent condition, $OR = 5.22$, $p = .004$, Fisher Exact test, see Figure 1).

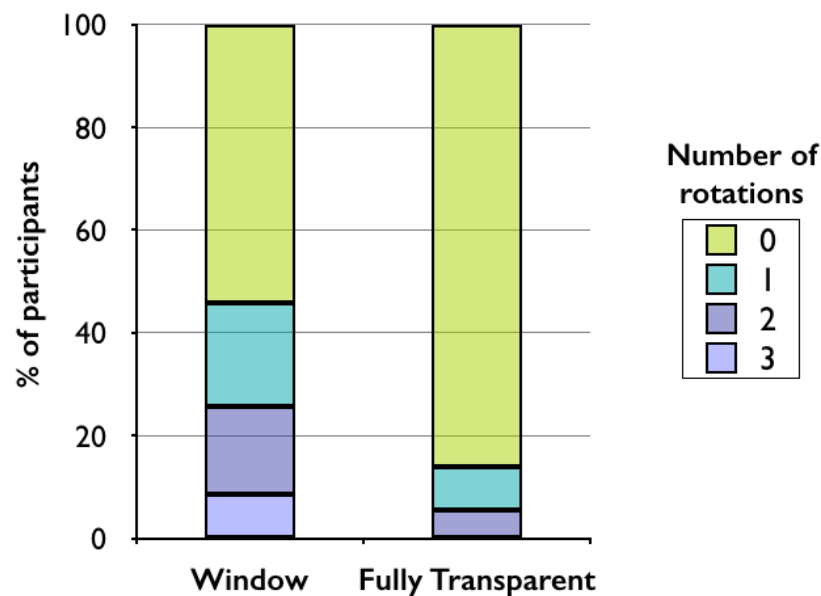


Figure 1. Number of rotations of the box containing the dog during the test phase per percentage of participants and per condition (Window vs. Fully Transparent).

To assess whether our main result held for the youngest participants in our sample, we analyzed separately the performance of 2-year-olds (Window condition: $n = 19$, $M_{age} = 29.42$ months, $range = 23-36$ months; Fully Transparent condition: $n = 17$, $M_{age} = 28.76$ months, $range = 23-35$ months) and of 3-year-olds (Window condition: $n = 16$, $M_{age} = 42.75$ months, $range = 37-48$ months; Fully Transparent condition: $n = 19$, $M_{age} = 41.84$ months, $range = 37-48$ months). For each of these age groups, we built a null model including only participant identity as a random intercept and a hypothesis driven model, which included Condition (Window vs. Fully Transparent) as an additional factor. The hypothesis-driven model provided a better fit than the null model by a LRT both for 2-year-olds ($\chi^2(1) = 7.49$, $p = .006$) and for 3-year-olds ($\chi^2(1) = 4.14$, $p = .042$), thus confirming that the participants of each age group rotated the box more in the Window condition than in the Fully Transparent condition (2-year-olds : $\beta = 1.31$, $SE = .60$, $95\%CI = .36-2.81$, $Z = 2.19$; 3-year-olds: $\beta = 1.47$, $SE = .75$, $95\%CI = .10-3.42$, $Z = 1.96$). Non-parametric analyses tended to confirm these results, although they reached only marginal significance for 3-year-olds (for 2-year-olds, the number of participants that rotated the box at least once was 8 out of 19 in the Window condition vs. 1 out of 17 in the Fully Transparent condition, $OR = 11.64$, $p = .019$; for 3-year-olds, the number of participants that rotated the box at least once was 8 out of 16 in the Window condition vs. 4 out of 19 in the Fully Transparent condition, $OR = 3.75$, $p = .09$, Fisher exact tests).

We also performed additional analyses to assess the effect of a set of exploratory variables on the participants' tendency to rotate the box during the test phase, focusing on the effects of Gender, Number of rotations during the baseline phase, Number of rotations during

the pre-test phase, Age, and interaction between Age and Condition (see the Supplemental Materials). These analyses revealed only an effect of the number of rotations during the baseline phase, indicating that the participants that rotated the target box more during the baseline phase also rotated it more during the test phase. These analyses also confirmed that the effect of Condition (Window vs. Fully Transparent) remained significant, even after taking into account the effect of all the exploratory variables.

Discussion

Between the ages of 2 and 3—or even earlier—toddlers have been shown to pay attention to disagreements (Hummer, Wimmer, & Antes, 1993; Koenig & Echols, 2003), and they are sensitive to the kind of input—such as visual perception—that can provide others with information (Moll & Tomasello, 2006; Nurmsoo & Bloom, 2008). The present experiment assessed whether young children could select relevant evidence when attempting to influence others.

Two- to four-year-old children were presented with an experimenter who asked them about the location of a toy placed in one of four boxes, and the experimenter disagreed with the information initially provided by the participant. In the Fully Transparent condition, all boxes were transparent, so the children did not have any obvious means of providing further support for their claims to the experimenter. In contrast, in the Window condition only the children, and not the experimenter, could see in which box the toy had been placed. Thus, the children had the opportunity to turn the box toward the experimenter as an additional means of supporting her claim about the toy's location.

Children rarely turned the baited box when the boxes were fully transparent. In contrast, children were more likely to turn the box in the proper context: when the experimenter was disagreeing with them and when turning the box could reveal new and relevant information to the experimenter (i.e., in the Window condition). These results are

compatible with two—non-mutually exclusive—interpretations of what children attempted to achieve by rotating the baited box in the Window condition. A first possibility is that the participants aimed to change the experimenter's belief about the location of the target toy by showing it to her. Second, it is possible that by rotating the baited box the children attempted to justify their answer by demonstrating to the experimenter that they had visual access to the location of the toy. Both of these interpretations suggest the presence of precursors of skills crucial for reason-giving: deciding when to use reason-giving and choosing reasons appropriately as a function of the context.

Overall, our participants rotated the box at a low baseline rate. This pattern can be explained by the fact that (i) in the Fully Transparent condition, rotating the box was irrelevant, and (ii) the participants had to discover on their own that the boxes could be rotated. Remarkably, approximately half of the participants rotated the box at least once (16 participants out of 35, 46%) in the Windows condition, in which this action was relevant to support their claims about the toy's location. Thus, a substantial subset of the participants established on their own the relevance of rotating the box as a way of supporting their claims.

Our study was not designed to assess capacities for reason-giving at the individual level. Therefore, it is difficult to make strong claims about the participants that did not rotate the box in the Windows condition. They might have either (i) not realized that they could rotate the box, or (ii) might have not dared to do it, or (iii) might have not realized that rotating the box was an appropriate way of providing support for their claims. Our data do not allow us to disentangle between these possibilities. Crucially, the fact that the participants' tendency to rotate the box was significantly higher in the Window condition than in the Fully Transparent condition indicates that by 2 years of age, at least a large subset of children is capable of selecting relevant evidence when providing support for a conclusion.

Three important features may explain why young children succeeded in engaging in reason-giving in our study. First, our participants had to provide support for something true. By contrast, the capacity to persuade someone of something false appears to develop later, during the preschool years (e.g., Lee, 2013; Mascaro, Morin & Sperber, 2017; Russell et al., 1991). Thus, the fact that in our experiment, children were asked to give reasons in a benevolent and truthful manner might have contributed to their success.

Second, in our study non-verbal reason-giving consisted in making an object visible to support claims about its location. The informative value of direct visual perception is recognized early by infants and toddlers (e.g. Luo & Johnson, 2009; Moll & Tomasello, 2006; Nurmsoo & Bloom, 2008). By the age of 2, children also have sufficient perspective taking abilities to place an object behind a screen in order to hide it from someone (Flavell, Shipstead, & Croft, 1978; McGuigan & Doherty, 2002; Mascaro et al., 2017). Importantly, previous studies of perspective taking have not assessed children's ability to recruit the evidential weight of perception to provide support for a claim. Our study reveals that 2- and 3-year-olds can use their perspective taking abilities to manipulate an object to make it visible for an adult and that they do so when it is relevant to support claims about the object's location.

A third reason why many children succeeded in our task is that we made the disagreement between the experimenter and the participant explicit, building upon young children's capacity to contradict an adult who is mistaken (Hummer, Wimmer & Antes, 1993; Koenig & Echols, 2003). Crucially, when contradicting someone, a child may simply deny this individual's claims without trying to select the most relevant evidence that may change the individual's mind. By contrast, in our study, the difference between children's behavior in the Window and in the Transparent conditions shows that children modulate their response to convey relevant information in order to provide support for their claims.

Previous studies suggest that children can evaluate others' reasons from 2 years of age (Castelain, Bernard, & Mercier, 2018; Castelain, Bernard, Van der Henst, & Mercier, 2016; Corriveau & Kurkul, 2014; Koenig, 2012; Mercier, Bernard, & Clément, 2014; Mercier, Sudo, Castelain, Bernard, & Matsui, in press). Yet, to evaluate reasons and to produce them to influence others are quite different tasks. Indeed, to evaluate a reason, one needs to assess the epistemic weight of a piece of information that is already presented to us as a reason. By contrast, to produce reasons efficiently, one needs to find a decent reason in a vast space of possible reasons (Mercier, 2012). Moreover, finding a good reason means assessing the persuasive value of a piece of evidence from someone else's viewpoint. This process requires to take into account some of the audience's mental states (knowledge, beliefs) that may differ from one's own. Our study indicates that children adjust their reason-giving attempts to the informational access of their audience from 2 years of age onward.

Observational data suggest that children start providing what appears to be reasons nearly as soon as they can produce sentences, between the ages of 18 and 24 months (Kuczynski & Kochanska, 1990; Kuczynski, Kochanska, Radke-Yarrow, & Girnius-Brown, 1987; Perlman & Ross, 2005). By the age of three, children have been said to "generate and think about positive and negative reasons for pursuing different courses of action or for holding specific sets of beliefs" (Stein & Bernas, 1999, p. 97; see also Dunn & Munn, 1987). Our data shed light on the cognitive underpinnings of these early reason-giving behaviors.

One route to discover what kind of behavior is likely to influence others is simply to rely on previous successes. Across repeated instances of social interactions, very young children may discover that by engaging in certain behaviors (e.g., smiling, crying, shouting, insisting, negating a contradictor's viewpoint), they are more likely to get others to comply with their requests or to agree with them (e.g., Weiss & Sachs, 1991; Bartsch, Wright & Estes, 2010). This kind of fixed strategy is efficient up to a point. A more powerful way of

influencing others is to rely on abstract principles that guide the selection of reasons, which one may call *naïve rhetoric*. A naïve rhetoric is likely to include some sensitivity to the role of evidence in persuading others. We find that the roots of naïve rhetoric are already present during toddlerhood. In our study, participants did not rely on a fixed, pre-established strategy. If they had, they would have rotated the boxes equally across conditions (Window vs. Fully Transparent). Instead, toddlers were sensitive to the type of evidence that their interlocutor already had access to. When trying to provide support for their claims, toddlers were more likely to provide the experimenter with a piece of information when it was novel rather than when it was already known.

Observing flexible reason-giving in toddlers introduces a host of novel questions about the development of argumentative skills, and naïve rhetoric. Do toddlers assume that some sources of evidence (e.g., direct perception) are stronger than others (e.g., testimony)? Do they make assumptions about the way evidence from different sources are combined? Do they evaluate the well-formedness of their own reasons? Our results indicate that addressing these questions and therefore testing the preverbal roots of human reason-giving during infancy and toddlerhood is feasible.

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