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Selective Social Belief Revision in Young Children

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ABSTRACT

Recent research has shown that from early in development, children selectively form new beliefs in response to information supplied by others. However, little is known about the development of selective revision of existing beliefs in response to socially conveyed information. Such selective social belief revision has been extensively studied by social psychologists in the context of advice-taking. Here, we adapted the methods of this research tradition for studying selective advice-taking in young children and adults. Participants solved a perceptual judgment task, received advice, and subsequently made final decisions. The informational access (perceptual quality) of participants and advisor were experimentally manipulated. Adults revised their judgments systematically as a function of both their own and the advisor's informational access whereas children based their adjustments only on their own informational access. Two follow-up experiments suggest, however, that this pattern of results in children reflected performance rather than competence limitations: In suitably modified tasks, children did proficiently consider both their own informational situation and that of the advisor in their selective social belief revision.

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Children are born into a complex world. Successfully navigating this world requires the acquisition of extensive knowledge from others. But not all information is of the same quality and worth considering to the same extent. Testimony can be misleading, be it out of ignorance, incompetence or malevolence. And not in all situations should others' testimony be privileged over one's own informational access. Thus, successful strategies of learning from testimony need to be systematic and selective. More specifically, they need to be selective both as a function of the quality of the sources of social information (the better an advisor is informed, the more one should trust her) and of one's own informational quality (the poorer oneself is informed, the more one should trust other advisors). This holds with regard to the formation of new beliefs and judgments, as much as concerning the revision of existing ones.

Much developmental research has investigated the early ontogeny of selective trust and learning. From this research we know that preschoolers do not learn about new matters unselectively, but monitor the competence and reliability of social sources of information: When learning novel words or facts, for example, children from around age four prefer knowledgeable over ignorant, accurate over inaccurate, confident over unconfident

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informants and perceptually well over perceptually poorly situated witnesses (e.g., Brosseau-Liard & Birch, 2011; Harris & Corriveau, 2011; Koenig & Harris, 2005; for a review, see Harris, 2007, 2012). This selective learning comprises context-sensitivity: For example, children of this age generally prefer to learn from adults rather than from peers, but show the reverse pattern when the topic to be learned about is likely to be more familiar to peers (e.g. toys) or when the adult has previously proven to be less reliable than the peer (Jaswal & Neely, 2006).

It is still somewhat unclear what the cognitive foundations and underlying cognitive processes of such early selective trust are. In fact, there may be different forms of selective learning that recruit different strategies. Those strategies are more or less sophisticated and operate context-specifically as a function of task demands and formats, background knowledge and availability of executive function (Hermes, Behne, Bich, Thielert, & Rakoczy, 2018a; Hermes, Behne, & Rakoczy, 2015, 2018b; Sobel & Kushnir, 2013).

Previous research on the development of selective trust, thus, documents remarkable early capacities. The capacities addressed in this line of research, however, are restricted to the acquisition of new information. In a typical test situation, the child is faced with a question she is ignorant about (e.g., "What is this novel object called?", "What is this novel tool good for?", etc.), is then given competing answers by different models, and is finally asked to endorse one of them. This line of research, thus, tells us about the development of selective social belief *formation* (in contrast to selective social belief *revision*). But in many real-life situations, we do not come completely ignorant in search of utterly novel information. Rather, we do come with preexisting beliefs and judgments, and the fundamental challenge is to flexibly and selectively coordinate existing beliefs and new information, to update and revise the former in light of the latter if necessary. We do know from recent research that children from around age 4 are capable of rationally revising previously formed beliefs in light of new data (e.g., Bonawitz, Fischer, & Schulz, 2012; Fernbach, Macris, & Sobel, 2012; Hagá & Olson, 2017; Kimura & Gopnik, 2019). But little is known about the early development of *social* belief revision: do children revise their prior beliefs rationally and selectively in response to others' testimony as a function of the quality and reliability of the testimony?

Such social revision of beliefs has been extensively studied in social psychology under the rubric of *advice-taking*. In the standard method used in this line of research, the so-called *judge-advisor-system* (JAS; Sniezek & Buckley, 1995), a *judge* is asked to make an initial judgment under some uncertainty, then receives advice from another informant (the advisor), and is then asked to make her final judgment. Typically, these tasks require participants to make and revise fine-grained numerical judgments (for example, to estimate the distance between Berlin and Paris or the size of Brazil). The crucial measure is the degree of advice-taking, that is, how much the judge adjusts her initial judgment in light of the advice (e.g., Harvey & Fischer, 1997; Soll & Larrick, 2009; Yaniv & Kleinberger, 2000). Findings of this research show that adults do heed advice in reasonable even if not perfectly rational fashion. Adults' advice-taking falls short of perfect rationality since, for example, adults weight advice insufficiently and overweigh their own initial judgments, a phenomenon called *egocentric advice discounting* (Yaniv, 2004a; Yaniv & Kleinberger, 2000). But it is functional such that adults revise their judgments in light of advice in a selective and systematical fashion; and thus benefit from these revisions by increasing the accuracy of their judgments (for reviews, see Bonaccio & Dalal, 2006; Rader, Soll, & Larrick, 2017): They weigh advice, for example, according to the advisor's epistemic history (Feng & MacGeorge, 2006), confidence (van Swol, 2009; Yaniv & Kleinberger, 2000), and access to information (e.g., Birnbaum & Stegner, 1979; Sniezek & Buckley, 1995). And they use advice more when they themselves have limited expertise (Yaniv, 2004b) or when the task is difficult (Gino & Moore, 2007).

From a developmental point of view, the fundamental question is how such advice-taking or social belief-revision develops. To date, however, research on advice-taking in developmental contexts is still scarce. In particular, little is known about whether the possibility to adjust judgments in fine-grained continuous ways influences children's willingness to revise beliefs in the first place; whether children's ability to select informants as a function of competence – as documented in the selective trust research – extends to their selection of advisors; and how much the level of children's own knowledge influences their decision-making. Some established lines of research, however, have investigated precursors of, or phenomena comparable to advice-taking.

At first sight, studies on conformity in children may be taken to show social belief revision. In such experiments, modeled on the classical Asch paradigm (Asch, 1956), children are asked to make a judgment about an obvious matter (e.g., which of several sticks is the longest, where it is visually very clear what the correct answer is, e.g., "stick A") facing a majority of other children who give an incorrect answer (e.g., "stick B"). Under these conditions, many children (just like adults) go along with the wrong group judgment ("stick B"). However, it is very clear from control tasks (probing children's judgments in private rather than public situations reveals that they still know perfectly well that in real fact "A" is the correct answer) that children merely pay lip-service to the majority opinion rather than engaging in actual belief revison (Corriveau & Harris, 2010; Haun & Tomasello, 2011). In other words, these studies merely tap normative rather than informational social influence (Deutsch & Gerard, 1955, sometimes also termed "compliance" and "conversion"; Jaswal, Lima, & Small, 2009).

Another line of reseach has shown that children are sensitive to others' judgments when their own perceptual evidence is ambiguous (Jaswal, 2004; Jaswal & Markman, 2007; Li & Yow, 2018). In these studies, children were asked, for example, to judge what was depicted in the picture of a hybrid animal (e.g., an animal that has features of both a cat and a dog but, overall, looks a bit more like a cat). On their own, children based their categorization on visual characteristics (categorization as "cat"), whereas children who heard an adult categorize this animal differently (e.g., "dog") followed this assertion. In terms of belief revision, however, these studies are difficult to interpret since children did not announce an initial judgment.

Yet another line of research has attempted to study belief revision more directly. One series of studies used tasks in which participants were asked to perceptually identify objects. Children had to base their initial judgment on insufficient perceptual access, such as looking at an uninformative excerpt of a picture (O'Neill, Astington, & Flavell, 1992) or tactile information when asked about an object's color (Whitcombe & Robinson, 2000). Results showed that children only changed their own judgment when the contradicting informant was better informed (that is, had information from the more suitable perceptual modality).

While these studies investigated categorical belief revision (such that children could only choose between retaining their prior belief or abandoning it and following the testimony), Rakoczy, Ehrling, Harris, and Schultze (2015) investigated fine-grained belief revision and allowed participants to adapt their initial judgments in graded fashion. In a feeding game, children were asked to judge the optimal amount of food items for unfamiliar animals (e.g., the best number of haystacks to feed a platypus), received advice, and then were allowed to make their final judgment. Participants revised their judgment systematically as a function of the advisor's expertise, prioritizing informants that were introduced as knowledgeable. One possible limitation, however, was that children's initial judgment was an uninformed guess. Thus, it remains somewhat unclear whether children really engage in belief revision (rather than first guessing and then forming a proper belief in response to advice).

In sum, from existing research we know about the development of selective trust such that children selectively acquire new beliefs on the basis of social information; we know about children's early susceptibility to normative social influence; and we have some direct evidence for selective nonsocial belief-revision and some indirect evidence for precursors of selective social belief revision. But, so far, we do not know about young children's advice-taking as a function of epistemic status. The rationale of the present study, thus, was to investigate the early development of selective social belief revision more directly, systematically and stringently. In particular, we analyzed children's (and adults') selective advice-taking as a function of both their own informational status (will they selectively revise beliefs more when they themselves are in poor epistemic condition?) and the informational status of informants (will they selectively revise beliefs more in response to informants who are in better epistemic conditions?). To this end, we implemented a basic JAS set-up in ways as simple, ecologically valid, and child-friendly as possible: the task was to make quantitative perceptually grounded judgments concerning the location of a mark on a continuous analogue scale. The quality both of the subject's own perceptual access as well as that of the advisor were systematically varied by having them look through clear or blurred windows. In addition to this objective manipulation of the quality of perceptual access, we also investigated whether this difference in knowledge levels was reflected in children's subjective ratings of perceptibility and confidence. We focused on children at age 4–5 years for various reasons: First, robust skills for selective belief formation have been documented from age 4 on (Harris, 2012). Second, basic skills for (nonsocial) belief revision have also been shown from around this age (e.g. Fernbach et al., 2012; Kimura & Gopnik, 2019).

In Study 1, we tested participants in three contrasts: (1) Judge and advisor had poor visibility (*poor-poor*). (2) The judge had high and the advisor poor visibility (*high-poor*), and (3) the judge had poor and the advisor high visibility (*poor-high*).[1] Past research has shown that children prefer knowledgeable informants when learning new information (Koenig & Harris, 2005) and are willing to revise guesses following an informant that had better visual access (Whitcombe & Robinson, 2000). We anticipated, therefore, that children, just like adults, would take more advice from an advisor with better perceptual access, and that children would take more advice when they themselves are in poor perceptual situations compared to when they were in favorable perceptual situations. The results of Study 1, however, showed that children (unlike adults) only adjusted their degree of advice-taking as

[1]In the present design, it was conceptually impossible to implement a high-high condition because the advice consisted of *random* judgments to ensure comparable differences between initial estimates and advice in all conditions. While such differences are highly plausible whenever at least one of the two judgments is based on poor visibility, the same is not true when both judge and advisor have good visibility of the stimulus. Here, participants would expect the advisor to make a similar judgment. Frequent violations of this expectation could result in participants losing trust in the advisors' perceptual reliability or the credibility of the experimental set-up.

a function of their own informational quality. Studies 2 and 3, therefore, investigated potential explanations for this lack of advice differentiation by simplifying the original task in two ways: first, we presented children with the decision task in form of a classical selective trust paradigm (Study 2) and second, we modified the revision process to categorical rather than continuous belief revision (Study 3). Taken together, Studies 2 and 3 suggest that under suitable circumstances children are able to adjust their social belief revision selectively as a function of both their own and the advisor's informational access.

Study 1

Method

Participants

Thirty-five adults (age range: 18–44 years, M = 24.7 years, 26 women) and forty 4-to 5-year-olds (age range: 48–71 months, M = 59.5 months, 23 girls) were included in the final sample who participated in all tasks. Participants were native German speakers and came from mixed socioeconomic backgrounds. Adults had answered our study announcement on public notice-boards. Children were recruited from a database of families who had previously given consent to their participation. Six additional children were tested but excluded from the final sample because of experimenter error (n = 1), because of comprehension problems (n = 2) or because they were uncooperative (n = 3).[2] Participants were tested individually in the lab. Assuming an inter-correlation of measures within participants of r =.70 and given a tolerated type I error level of 5%, our sample provides a test power of .80 for small effects of f = 0.11 within participants (main effect and within-between interaction) and for large effects of f = .30 between participants.

Material

We presented participants with a perceptual judgment task in which they were to estimate the location of a mark on a stick they could see through a window. Wooden sticks of 20 cm length were used. Sticks were placed in a triangular box (edge length of triangle: 57 cm, height: 44 cm). The box consisted of a clear window side, a blurred window side (window measurements: 27.5 cm × 23.5 cm), and an opaque backside (see Figure 1; for additional details, see the Appendix). At the clear window, the black mark was easily visible while at the blurred window it was hardly visible. Participants and advisors made their judgments by indicating on an analogue scale (a two-dimensional picture of the stick of the same length, laid out horizontally on the table) where the mark was. The advisor pretended to give advice based on her perceptual access (where advice in fact consisted in randomized judgments, so that the de facto accuracy of advices did not co-vary with condition, ranging from one to 200 mm, produced by a random generator and secretly marked on the advisor's scale prior to the test sessions).

For visibility ratings (how well they could see the mark), we presented children with an eye scale (8 cm × 29 cm) consisting of five eyes ranging from small to big. For confidence ratings (how confident participants were of their judgments), we presented them with a 5-point scale consisting of 5 thumbs ranging from one pointing straight down to one

[2]Three of these children did participate in the initial visibility and certainty ratings for which their data were entered into the corresponding analyses (see below).

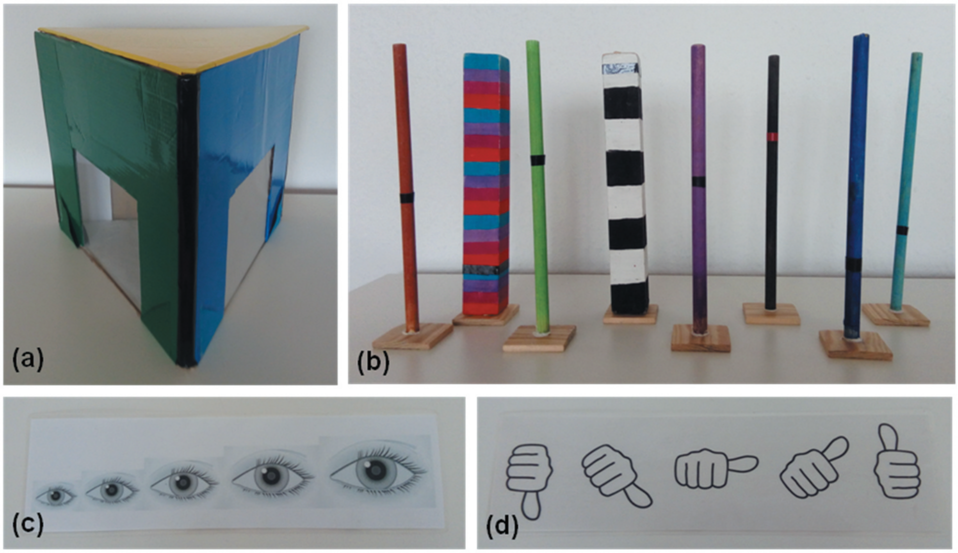

Figure 1. Materials used in study 1: Three-sided box with clear and blurred window (a). Colored sticks (b), rating scales for visibility (c) and confidence (d).

pointing straight up (see Figure 1). Adults rated visibility and confidence using a standard 5-point Likert scale.

Design and procedure

The task was presented in JAS format with participants taking the role of judges. Advisors were adult confederates of the experimenter who presented the judges with the randomized advice. We used a 2 (age groups) × 3 (conditions) mixed factor design, with age groups (children and adults) as between factor and conditions (*poor-poor, high-poor*, and *poor-high*) as within-subjects factor. All participants received two trials of each condition (i.e. a total of 6 trials). The trials per condition were administered in blocks, with order of condition-blocks counterbalanced across participants.[3]

Every session started with an introduction phase prior to the test phase. In the introduction phase, participants were familiarized with the set-up and material to make sure that they understood the design of the three-sided box, the concept of transferring a black mark on a colored stick onto a picture of the stick, and the subjective rating of visibility and confidence.

Introduction phase. The introduction phase consisted of two steps. In the first step, children were introduced to a 5-point confidence rating scale and a 5-point visibility scale (for details, see the Appendix). For our adult participants (who used a Likert scale), a similar familiarization with the confidence and visibility ratings was not necessary. In the second step, participants judged one stick at the clear and one stick at the blurred window (order of windows counter-balanced). Visibility and confidence ratings were requested for each judgment. This step served as manipulation check to ensure the validity of our set-up.

[3]For details about additional measures administered in the test session of Study 1 that were not in the main focus and are thus not reported in the main text, see the Supplementary Online Material.

Test phase. In the six tests trials (2 per condition), judge and advisor were each placed at one side of the box. In the *poor-poor* condition, they both sat at the same side whereas in the *poor-high* and *high-poor* conditions they sat at opposite sides (for an illustration of the high-poor condition, see Figure 2). Then one stick was put in the box (in such a way that participants could not observe the placement), and both judge and advisor were asked to make their initial judgments. Then, the judge was seated at the opaque side of the box, received the scale of the advisor in addition to the scale with her own initial judgment, and was asked to make her final judgment on a third scale. The judge rated visibility and confidence both for her initial and for her final judgment.

The dependent measures were as follows:

- *Visibility rating*: Children accessed their own visibility for their initial and final judgment on a 5-point scale (5 eyes of different size). Adults were asked to rate their visibility ranging from 1 (low) to 5 (high).
- *Confidence rating*: Children accessed their own confidence for their initial and final judgment on a 5-point scale (5 thumbs ranging from thumb down to thumb up). Adults were asked to rate their confidence ranging from 1 to 5.
- *Deviation from the true value*: The deviation from the true value is the distance of the marks drawn by the judge from the true location of the mark on the stick (measured in cm).
- *Advice-taking (AT)*: The advice-taking measure is the weight the judge gives the advice, defined by Harvey and Fischer (1997) as

$$AT = \frac{final\ estimate - initial\ estimate}{advice - initial\ estimate} \times 100\%$$

.

If the judge fully endorses the advice, AT would be 100%, if the judge equally weighs her judgment and the advice, AT would be 50%, and if she does not consider the advice at all, AT would be 0%. Other AT scores between 0% – 100% represent partial shifts from the initial judgment towards the advice. In order to control for extreme cases and outliers, AT scores are usually truncated. Accordingly, we limited them to 100% and −100% (see Schultze, Mojzisch, & Schulz-Hardt, 2017, for a similar approach).

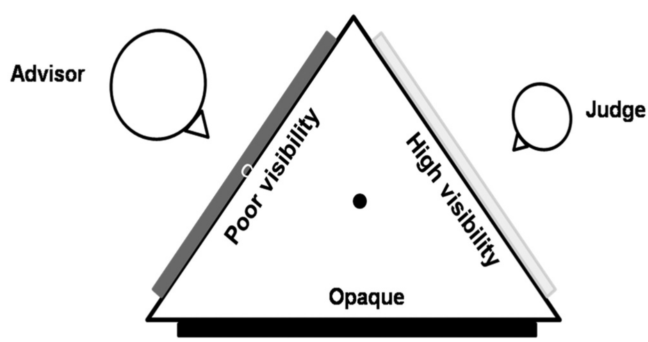

Figure 2. High-poor condition of AT-part with child judge in study 1.

Results

Plan of analysis

In a first step, as a manipulation check, we analyzed objective performance, visibility and confidence ratings in the Introduction Phase to see whether participants indeed were more accurate and judged visibility and confidence higher at the clear compared to the blurred window. In a second step, the main analysis focused on advice-taking in the Test Phase, the main questions being: do participants engage in more advice-taking as a function of their own perceptual access (i.e., more in poor-poor than in high-poor) and as a function of the advisor's perceptual access (i.e., more in poor-high than in poor-poor)? Both for the manipulation check and the main analyses, we conducted omnibus tests (ANOVAs) and the planned comparisons derived from the target questions. For supplementary analyses, see the Appendix.

Manipulation check

Results of the manipulation checks revealed that participants' visibility and confidence ratings as well as their accuracy (indicated by the deviation from the true value) in the Introduction Phase were successfully manipulated by watching either through the clear or blurred window. We calculated an ANOVA for repeated measures with window (clear or blurred) as within-subjects variable and age group (children or adults) as between-subjects factor to analyze whether there were significant differences in ratings and accuracy between the two windows. For post-hoc analyses we used independent and paired sample *t*-tests. Participants rated their visibility and confidence in their judgment significantly higher at the clear than at the blurred window and in fact made more accurate judgment at the clear window (see Figure 3)

For visibility and confidence results, the ANOVAs revealed significant main effects of quality of the window (visibility: $F(1, 76) = 461.39$, $p < .001$, $\eta_p^2 = 0.89$; confidence: $F(1, 76) = 222.87$, $p < .001$, $\eta_p^2 = 0.75$) and age group (visibility: $F(1, 76) = 7.65$, $p = .007$, $\eta_p^2 = 0.09$; confidence: $F(1, 76) = 55.82$, $p < .001$, $\eta_p^2 = 0.42$) and a significant interaction of both factors (visibility: $F(1, 76) = 18.17$, $p < .001$, $\eta_p^2 = 0.19$; confidence: $F(1, 76) = 72.44$, $p < .001$, $\eta_p^2 = 0.49$). Planned comparisons revealed that ratings of both children and adults were higher at the clear than at the blurred window (visibility: children: $t(42) = 9.87$, $p < .001$, $d_z = 1.52$; adults: $t(34) = 46.29$, $p < .001$, $d_z = 7.94$; confidence children: $t(42) = 4.16$, $p < .001$, $d_z = 0.64$, adults: $t(34) = 20.38$, $p < .001$, $d_z = 3.49$). Furthermore, children and adults differed in their ratings when judging sticks at the blurred window (visibility: $t(48.65) = 4.48$, $p < .001$, $d = 1.28$; confidence: $t(56.5) = 10.07$, $p < .001$, $d = 2.68$) but not at the clear window (visibility: $t(56.62) = -1.05$, $p = .298$, $d = -0.28$; confidence: $t(76) = 1.65$, $p = .104$, $d = 0.38$).

For the deviation from the true value, the ANOVA revealed a significant main effect of quality of the window, $F(1, 72) = 86.96$, $p < .001$, $\eta_p^2 = 0.55$. Planned comparisons revealed more accurate judgments at the clear window (children: $t(39) = -5.88$, $p < .001$, $d_z = -0.94$; adults: $t(33) = -7.43$, $p < .001$, $d_z = -1.29$) with adults being more precise than children at the clear window ($t(43.17) = 3.06$, $p < .004$, $d = 0.93$) and no difference between children and adults at the blurred window ($t(62.31) = 0.47$, $p = .638$, $d = 0.12$). In sum, these results confirm that the experimental manipulation was successful.

Main analyses

Advice-taking. Performance in advice-taking as a function of age groups and condition is depicted in Figure 4. A 3 (condition) × 2 (age groups) ANOVA showed a significant main

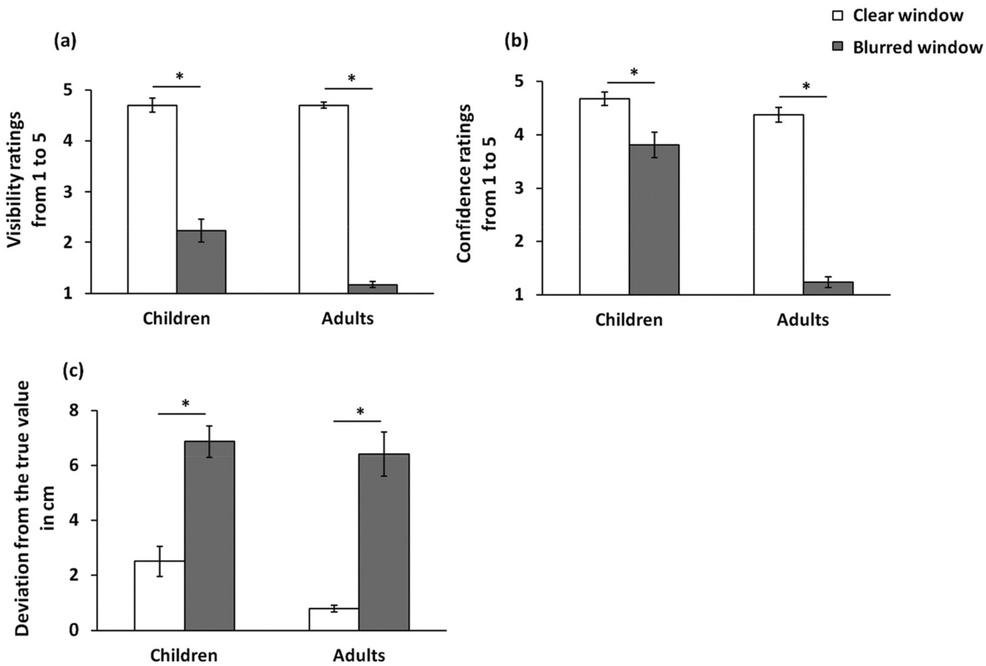

Figure 3. Children's (N = 43) and adults' (N = 35) subjective visibility (a), confidence (b) and deviation from the true value (c) behind the clear and blurred window in study 1. Error bars indicate standard error of the means.

effect of condition ($F(2, 146) = 21.58$, $p < .001$, $\eta_p^2 = 0.23$), no main effect of age group ($F(1, 73) = 1.04$, $p = .311$, $\eta_p^2 = 0.01$) and a significant interaction of both factors ($F(2, 146) = 5.10$, $p = .007$, $\eta_p^2 = 0.07$).

Planned comparisons revealed that while adults differed (hp < pp < ph) in their advice-taking between all three conditions (pp vs. hp: $t(34) = 5.52$, $p < .001$, $d_z = 0.95$; pp vs. ph: $t(34) = -2.84$, $p = .008$, $d_z = -0.49$; hp vs. ph: $t(34) = -8.91$, $p < .001$, $d_z = -1.53$) children's advice-taking differed (hp < pp = ph) only between the *high-poor* compared to the other two conditions (*high-poor* vs. *poor-high* ($t(39) = -2.06$, $p = .046$, $d_z = -0.33$), *high-poor* vs. *poor-poor* condition ($t(39) = 2.29$, $p = .027$, $d_z = 0.37$) and *poor-poor* vs. *poor-high* condition ($t(39) = 0.21$, $p = .838$, $d_z = 0.03$). Contrasting adults' and children's performance, the main difference in advice-taking was found in the *poor-high* condition ($t(69.70) = -2.72$, $p = .008$, $d = -0.65$). In contrast, children and adults did not significantly differ in the *poor-poor* ($t(69.28) = -0.37$, $p = .710$, $d = -0.09$) and the *high-poor* conditions ($t(57.01) = 1.67$, $p = .100$, $d = 0.44$).

In order to explore whether children's age had an influence on their ability to take advice selectively, we split children into two age groups of 20 children each using a median split (*md* = 59.5 months). An ANOVA with children's age group as a between-subjects variable, experimental condition as a within-subjects variable, and advice taking as the dependent variable showed a main effect of age group, $F(1, 38) = 4.72$, $p = .036$, $\eta_p2 = .11$, but no interaction of age group and experimental condition, $F(2, 76) = 0.28$, $p = .758$, $\eta_p2 = .01$. While older children generally heeded the advice less, the descriptive patterns was similar for both age groups (see Figure 4, right panel). Arguably due to the reduced sample size of

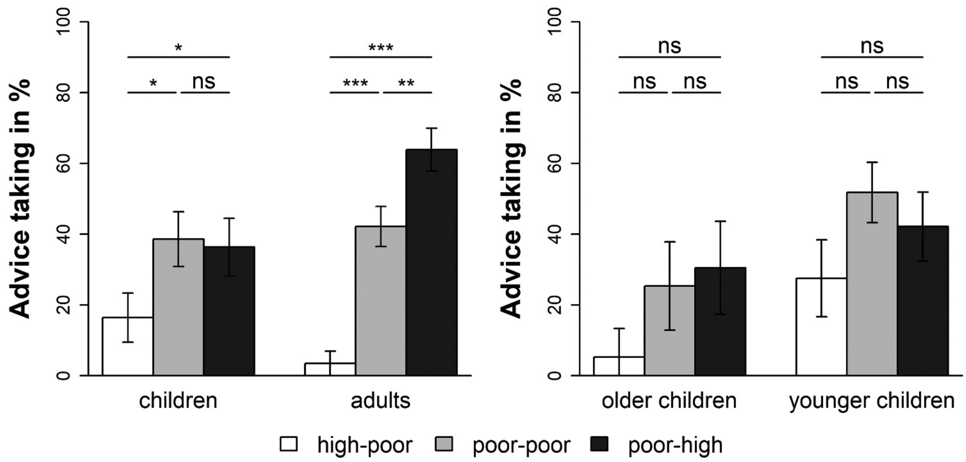

Figure 4. Advice-taking as a function of age group (adults vs. children) and experimental condition in Study 1 (left panel) and advice taking as a function of experimental condition shown separately for younger and older children (right panel). Age groups within children were formed via median split (median = 59.5 months) yielding two groups of $N = 20$ children each. Error bars indicate standard error of the means.

this analysis, the main effect of experimental condition fell short of statistical significance, $F(2, 76) = 2.84$, $p = .065$, $\eta_p 2 = .07$, and statistical comparisons between the experimental conditions within each age group did not yield significant results, all $|t| < 1.82$, all $p > .085$, all $d_z < 0.41$. A simple qualitative analysis of the proportion of children in each group who took advice in a normatively correct fashion (i.e., hp < pp < ph) further supports the notion that children's general ability to heed advice selectively was quite comparable in both age groups. Specifically, only 3 out of 20 children in the group of older children and only 5 out of 20 of the younger children behaved in this fashion (as compared to 20 of the 35 adults).

Visibility and confidence ratings. Participants' visibility (a) and confidence (b) ratings as a function of age groups and condition are depicted in Figure 5. For both visibility and confidence ratings, the target analysis was a 2 (age group) × 3 (condition) × 2 (judgment: initial vs. final) ANOVA.

Regarding visibility ratings, the 2 × 3 × 2 ANOVA revealed main effects of condition ($F(2, 72) = 306.06$, $p < .001$, $\eta_p^2 = 0.81$) and age group ($F(1, 72) = 13.20$, $p = .001$, $\eta_p^2 = 0.15$) as well as a significant interaction of condition and age group ($F(2, 72) = 15.17$, $p = < .001$, $\eta_p^2 = 0.17$). Post-hoc tests revealed that while in the *high-poor* condition children and adults did not differ in their initial ($t(42.4) = - 1.77$, $p = .083$, $d = - 0.54$) or final judgments ($t(52.5) = - 1.57$, $p = .123$, $d = - 0.43$), children rated their visibility for both judgments higher than adults in the *poor-poor* condition (before advice: $t(73) = 3.11$, $p = .002$, $d = 0.73$; after advice: $t(72) = 3.92$, $p < .001$, $d = 0.92$) and the *poor-high* condition (before advice: $t(73) = 3.79$, $p < .001$ $d = 0.89$, after advice: $t(72) = 4.16$, $p < .001$, $d = 0.98$). There was no significant effect of judgment ($F(1, 72) = 1.61$, $p = .209$, $\eta_p^2 = 0.02$). Additionally, no interactions of judgment and age group ($F(1, 72) = 1.85$, $p = .178$, $\eta_p^2 = 0.03$), condition and judgment ($F(2, 72) = 2.86$, $p = .061$, $\eta_p^2 = 0.04$) or condition, judgment and age group ($F(2, 72) = 1.20$, $p = .304$, $\eta_p^2 = 0.02$) were found.

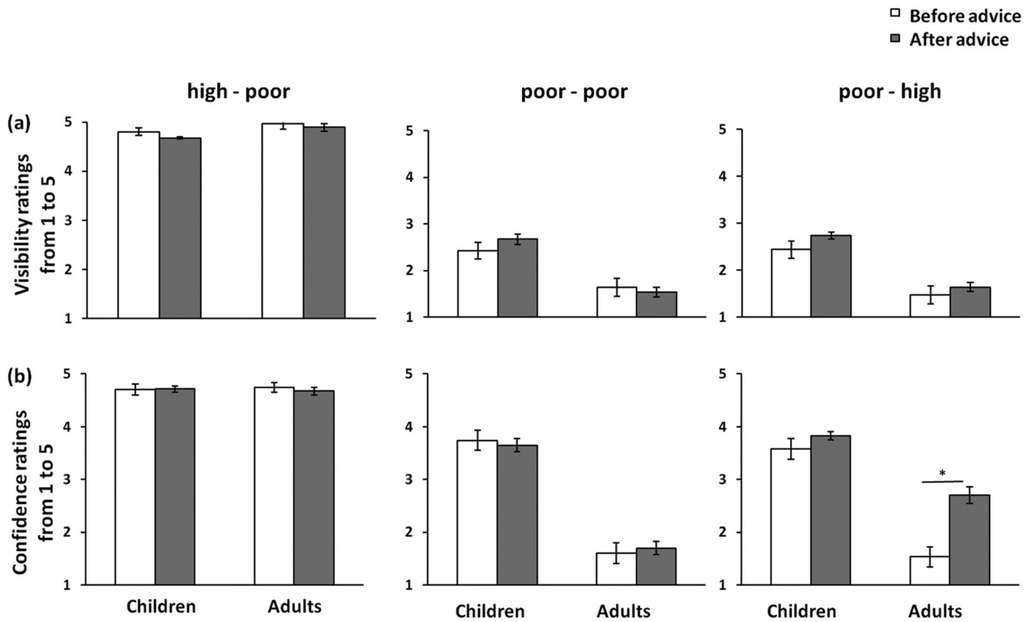

Figure 5. Visibility and confidence ratings before and after advice. Error bars indicate standard error of the means.

With regard to confidence ratings, the 2 × 3 × 2 ANOVA revealed main effects of condition ($F(2, 73) = 164.31$, $p < .001$, $\eta_p^2 = 0.69$), judgment ($F(1, 73) = 11.85$, $p = .001$, $\eta_p^2 = 0.14$) and age group ($F(1, 73) = 39.35$, $p < .001$, $\eta_p^2 = 0.35$), interactions of condition and age group ($F(2, 73) = 37.04$, $p < .001$, $\eta_p^2 = 0.34$), judgment and age group ($F(1, 73) = 6.63$, $p = .012$, $\eta_p^2 = 0.08$), as well as of condition, judgment and age group ($F(2,73) = 6.83$, $p < .001$, $\eta_p^2 = 0.09$). To follow up on this 3-way interaction, separate 2 (age group) × 2 (judgment: initial vs. final) ANOVAs were conducted for each condition. In the *high-poor* condition, this 2 × 2 ANOVA revealed no effects of judgment ($F(1, 73) = 0.15$, $p = .700$, $\eta_p^2 = 0.001$) or age group ($F(1, 73) = 0.02$, $p = .885$, $\eta_p^2 = .001$), and no interaction of both factors ($F(1, 73) = 0.36$, $p = .548$, $\eta_p^2 = .001$). In the *poor-poor* condition, the ANOVA showed a main effect of age group ($F(1, 73) = 53.78$, $p < .001$, $\eta_p^2 = 0.42$), but no effect of judgment ($F(1, 73) = 0.001$, $p = .955$, $\eta_p^2 = 0.001$), and no interaction of both factors ($F(1, 73) = 0.72$, $p = .398$, $\eta_p^2 = 0.01$). In the *poor-high* condition, the ANOVA showed main effects of judgment ($F(1, 73) = 28.99$, $p < .001$, $\eta_p^2 = 0.28$) and age group ($F(1, 73) = 33.65$, $p < .001$, $\eta_p^2 = 0.31$) as well as a significant interaction of both factors ($F(1, 73) = 12.18$, $p < .001$, $\eta_p^2 = 0.14$). Post-hoc tests revealed that this interaction was due to the fact that only adults got significantly more confident after getting advice from the clear window (adults: $t(34) = 5.43$, $p < .001$, $d_z = 0.93$; children: $t(39) = 1.57$, $p = .124$, $d_z = 0.25$).

Discussion

The main findings of Study 1 were the following: In a judge-advisor task designed to be suitable across age groups, adults revised their judgments selectively as a function both of their own and the advisor's epistemic status. Furthermore, adults became more confident after getting advice from the clear window when they themselves saw poorly. These results are in line with findings in social psychology from numerical decision tasks in JAS paradigms: Adults rely on cues to information accuracy and prefer advisors that have better access to information (e.g., Birnbaum & Stegner, 1979; Sniezek & Buckley, 1995) or take more advice when they themselves perceive the task as difficult (Gino & Moore, 2007). Similarly, post-advice confidence was shown to increase with advisors being more accurate (Budescu, Rantilla, Yu, & Karelitz, 2003) or having more access to information (Budescu & Rantilla, 2000).

In contrast, children seemed to consider their own epistemic status (the better the child's visual access, the less advice was taken) but not the advisor's epistemic status (children did not differentiate between well and poorly informed advisors) when making their final judgment. Along the same line, their confidence ratings were not influenced substantially by the quality of the advice. In particular, children did not become more confident after getting high quality advice.

What do these findings show about children's selective social belief revision? One interpretation of the present results is that 4- to 6-year-olds lack the conceptual competence to revise beliefs as a function of advice quality. This interpretation seems at odds with previous research on advice-taking in children reporting that children consider their advisor's expertise in a functional manner (Rakoczy et al., 2015). Alternatively, the present findings may not reflect competence but performance limitations due to extraneous demands of the present task. In fact, we can think of three task characteristics that may have made the present task particular demanding. First, advice quality varied, across trials, within the advisor instead of between several advisors, and children needed to keep track of both their own and the advisor's perceptual access to determine how valuable the advice was in a given trial. Second, our study used continuous judgments rather than asking children to select one out of a small set of options. Other than in the case of disjunctive options with one correct solution, two continuous judgments could be different from each other but equally (in)accurate, which could prove challenging for children to understand. Finally, the response format allowed for a greater number of revision strategies. While in the case of a finite set of answers, judges need to choose one, in the continuous case, they can choose one judgment, average their initial judgment and the advice, or choose from an unlimited number of weighted averages.

All of these task features might have made it difficult for children to consider advice quality in their final judgments, even if they were, in principle, able to selectively revise their beliefs in the light of differently accurate advisors. To investigate whether such performance limitations might potentially explain why children did not consider their advisor's visual access to the stimulus in Study 1, we introduced two simplifications to the original task: First, we implemented a structurally analogous selective social learning task in which children had to choose which of two agents to learn from (placed at the blurred and transparent windows, respectively) (Study 2). This allowed us to test whether children reliably tracked the differences in visual access and thus in the quality of advice of the two advisors in an analogous set-up that did not involve any need to revise beliefs, though. Second, we simplified the original belief revision task of Study 1. In Study 3, we presented to children a task that was analogous to the one in Study 1 with the one crucial exception that it involved a categorical rather than a continuous scale. This allowed us to test whether children are capable, in principle, of engaging in selective social belief revision once the task and the scales underlying judgments and revisions were simplified.

Study 2

In Study 2, children were presented with the same material as in Study 1, but with the crucial difference that they were now not asked to revise existing beliefs, but to form new ones in response to information supplied by other agents with varying epistemic status. This task was thus structurally analogous to classical selective trust tasks widely used in recent resarch on social-cognitive development (see Harris, 2012), and it is similar to the panel-of-experts paradigm used in research on adult judgment and decision-making (Mannes, 2009).

Method

Participants

Forty 4- to 6-year-olds (age range: 49–72 months, $M = 60.1$ months, 21 girls) were included in the final sample. Participants were native German speakers and came from mixed socioeconomic backgrounds. Children were recruited from a database of families who had previously given consent to their participation. One additional child lost interest in the study from early on and could not be included in the final sample ($n = 1$). Participants were tested individually in the lab.

Material

The same stimuli (box and the colored wooden sticks) as in Study 1 were used.

Design and procedure

The child was presented with two informants simultaneously: one with clear visual access and one with poor visual access. While the child was sitting at the opaque side of the box, she was first asked "Which of the two informants knows better where the mark on the stick is? Who would you ask?" (Ask measure). In the second part of each trial, both informants handed over their judgment (the scale with the mark), the child compared both judgments and was asked to make a final decision regarding the position of the black mark on a third unused scale (Endorse measure).

Children were familiarized with stimuli and box and subsequently participated in 8 test trials in total. In 4 trials, the child was sitting at the opaque side of the box. In the other 4 trials, the child was initially sitting at the blurred window together with the poor informant and was asked to closely look at the stick in the box and to identify the color of the stick. For the rest of these trials, the child moved to the opaque side. We conducted four trials of each kind, and the order of condition was counter-balanced. Crucially, the two informants switched position every two trials so that one and the same informant had good perceptual access in some, but poor access in other trials, and vice versa for the other informant.

Data for Study 2 and 3 were collected within one test session lasting 30 min. All test session were fully videotaped. Children participated first in Study 3 and afterward in Study 2.

The dependent measures were as follows:

- *Ask-measure*: Children were presented with a forced-choice question which of the two informants they would ask about where to draw the black mark. We calculated the percentage of trials in which each child chose the respective informant.
- *Endorse-measure*: For each trial, we coded whether the children endorsed the advice of the high quality informant, or of the low quality informant (child plotted her response within 2 cm around the respective informant's judgment), or neither. Across 8 trials, we then calculated the number of trials for each of three categories (endorse high quality informant/endorse low quality informant /neither).

Results and discussion

Performance in the Ask- and Endorse-measures as a function of informant quality is depicted in Figure 6. Only 0.6% of trials fell in the "neither" category and were not included in our further analyses. Our hypothesis was that children prefer the high quality informant for both the Ask- and the Endorse-measure. Data were analyzed using two multilevel logisitic regression models, one with the Ask-measure and the other with the Endorse-measure as a binary dependent variable (0 = low quality informant, 1 = high quality informant) (Bolker et al., 2009). Position of the child was effect-coded (−0.5 = blurred window, 0.5 = backside) and entered as fixed effect. We also added children's age in months (z-standardized) as a fixed effect covariate. Thus, the intercept of the model represents the average preference for the high quality informant collapsed across the two possible seating conditions (blurred window vs. backside) and assuming an average age of the child relative to the sample (in our data, 60.1 months). Participants were treated as the level 2 grouping variable, and the model included random intercepts (we did not included random slopes for position of the child because the respective models did not converge). For both dependent variables, the average proportion of the choice of the high quality informant was positively different from chance as indicated by fixed intercepts greater than zero (Ask: B = 2.86 (SE = 0.55), z = 5.17, p < .001; Endorse: B = 1.67 (SE = 0.29), z = 5.45, p < .001). Children's preference for the high quality informant did not differ as a function of their own position, blurred window or backside, for either dependent variable (Ask: B = − 0.07 (SE = 0.38),

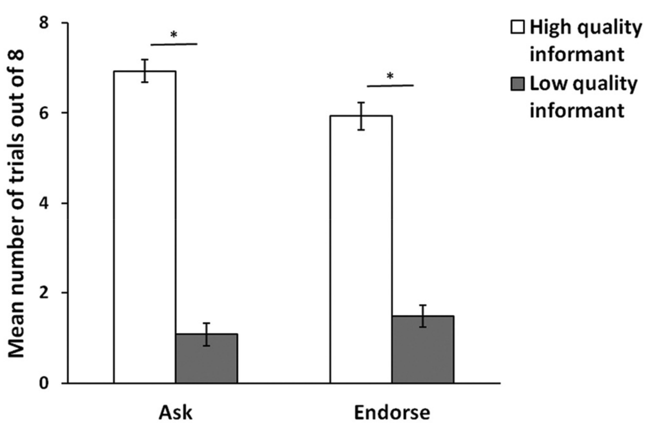

Figure 6. Ask and endorse measurement for children's choice of the high or low quality informant in study 2. Both kinds of trials (child behind the back and child behind the blurred window) are summarized.

$z = -0.19$, $p = .849$; Endorse: $B = -0.000003$ ($SE = 0.30$), $z = -0.000001$, $p = .999$). Consistent with our hypothesis children preferred the high quality informant over the low quality informant. This result, however, did not differ as a function of whether children were placed at the backside or blurred window of the box. Children's age was not significantly related to either dependent variable (Ask: $B = 0.13$ ($SE = 0.40$), $z = 0.32$, $p = .746$; Endorse: $B = 0.51$ ($SE = 0.27$), $z = 1.87$, $p = .061$).

In a selective trust version of the current task children, thus, did form new beliefs selectively as a function of information quality. This speaks to their ability to infer the quality of advice not only in situations in which there are stable differences in expertise between two or more advisors, but also when advice quality varies within advisors as a situational factor (see also Brosseau-Liard & Birch, 2011). Thus, children's neglect of the advisor's visual access when taking advice in Study 1 was likely not be based on difficulty in distinguishing between perceptual information of varying quality in the present kind of task. Study 3 investigates whether the continuous response format or the high number of possible revision strategies inherent to continuous belief revision (e.g., choosing one judgment, forming the mean, or selecting from an infinite number of weighted averages) may have made the task in Study 1 overly difficult.

Study 3

In Study 3, therefore, we administered a task analogous to the one in Study 1, but with a simplified decision procedure: children had to make categorical rather than continuous judgments and thus needed to engage in categorical rather than continuous belief revision. The underlying logic is the following: if children's failure to take into account the advisor's perceptual access quality in Study 1 reflects performance limitation to do with the continuous nature of the belief revision tasks (rather than competence limitation in selective belief revision per se), suitably simplified tasks that remove the continuous nature of belief revision may be more sensitive to tap early competence.

Method

Participants

The same 40 4- to 6-year-olds (age range: 49–72 months, $M = 60.1$ months, 21 girls) as in Study 2 were included in the final sample.

Material

In Study 3, we used the same box as in Studies 1 and 2, but with new stimuli. Stimuli were five animals fixed on a cardboard strip in the order fox, pig, rabbit, sheep, and dog. Pictures of the stimulus were used as scales. During test trials, one of these five animals was marked with a black belt. Children's were asked to identify this animal on a scale. Advices were randomized judgments produced by a random generator and secretly marked on the advisors' scale prior to the test sessions.

Design and procedure

Design and procedure were identical to Study 1 with the following exceptions: First, only children were tested. Second, the judgments required were categorical ("Which animal has a mark?") rather than continuous ("Where on the stick is the mark?"). Third, children did not rate visibility and confidence. The dependent measure was children's categorical advice-taking, that is, whether they changed their initial judgment and adopted the judgment of the advisor.[4]

Results and discussion

The proportion of trials in which children engaged in advice-taking as a function of condition is depicted in Figure 7. Data were analyzed using a multilevel logistic regression with advice-taking as binary dependent variable (0 = 0% advice-taking; 1 = 100% advice-taking). We entered the experimental condition (*high-poor, poor-poor, poor-high*) and children's z-standardized age in months as fixed effects. As condition was a three-level factor, we used dummy-coding treating the *poor-poor* condition as the reference category. That is, the intercept of the model refers to the probability of taking advice in the *poor-poor* condition for a child of average age relative our sample (60.1 months). One dummy variable coded the difference between the *poor-poor* and the *high-poor* condition while the other contrasted the *poor-poor* with the *poor-high* condition. Participants were treated as the level 2 grouping variable. When including random slopes of condition, the model did not converge; thus, we computed the model using random intercepts, only.

The intercept of the model was significantly lower than zero, indicating that given equally low visibility of judge and advisor, children ignored the advice on the majority of trials, $B = -2.61$ (SE = 0.54), $z = -4.80$, $p < .001$. The proportion of accepting advice differed

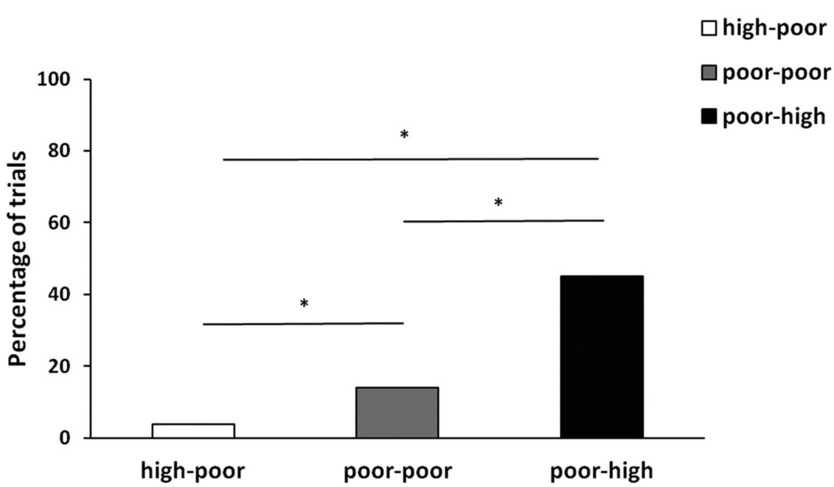

Figure 7. Advice-taking of children as a function of condition in study 3.

[4]Since the advice was determined by a random generator, it sometimes happened that it matched children's initial judgment by coincidence. In these cases, when children retained their initial judgment, a trial could thus not be classified as "advice-taking" even though children went along with what the advisor had said. In the main analysis, we kept these cases and treated them conservatively as "no advice-taking". This happened in 17% of the trials of the poor-poor condition, in 16% of the poor-high, and in 3% of the high-poor condition. Excluding these cases would not alter the main analysis, or it would even make the effects stronger (since relatively more cases would be excluded in the poor-high than in the high-poor condition, relative advice-taking in the remaining sample would increase more in poor-high than in high-poor, increasing the difference between the two).

significantly between conditions. Relative to the *poor-poor* condition, children heeded advice even less frequently in the *high-poor* condition, $B = -1.75$ ($SE = 0.76$), $z = -2.31$, $p = .021$. In contrast, advice-taking was more frequent in the *poor-high* condition relative to the *poor-poor* condition, $B = 2.38$ ($SE = 0.54$), $z = 4.40$, $p < .001$. Children's age was not significantly related to advice taking, $B = 0.27$ ($SE = 0.34$), $z = 0.81$, $p = .419$.

Not surprisingly, a similar multi-level regression excluding the *poor-poor* condition (and, thus, contrasting the *high-poor* and *poor-high* conditions), showed that the frequency of heeding advice was significantly higher in the *poor-high* as compared to the *high-poor* condition, $B = -4.30$ ($SE = 0.91$), $z = -4.73$, $p < .001$. Age was not significantly related to advice taking, $B = -0.06$ ($SE = 0.37$), $z = -0.17$, $p = .866$.

The main finding of Study 3 was, thus, that in a judge advisor task with categorical (rather than continuous) belief revision, children did revise their judgments selectively as a function of their own and the advisor's epistemic status. Our analysis, thus, indicates qualitative similarity between children's categorical advice-taking, investigated in Study 3 and adult's continuous advice-taking found in Study 1. These findings suggest that children's apparent inability to consider the advisor's visual access to the stimulus when revising their judgments as observed in Study 1, might be caused by extraneous demands of the task (related to the continuous nature of the judgments and belief-revision in Study 1) rather than by a principled limitation in their competence for selective social belief revision.

General discussion

The current study investigated selective social belief revision in children and adults as a function of their own and an advisor's epistemic status. The main results were the following: In Study 1, adults engaged in selective advice-taking consistently with previous research: they took into account both their own informational access (revising their initial judgments more when they themselves saw poorly) as well as the advisor's (revising their initial judgments more in response to advisors with better visual access). Children, in contrast, only revised their beliefs selectively as a function of their own informational access, but did not seem to differentiate according to the advisor's informational access.

This lack of selective advice-taking as a function of the advisor's epistemic status may reflect competence limitation in children's selective social belief revision, or, alternatively, mere performance limitations due to specific tasks demands. Potential performance factors that may have made the task in Study 1 particularly difficult include the following: First, participants had to keep track of both their own and another agent's perceptual access, which varied across trials. Second, they had to make fine-grained continuous (rather than simple categorical) judgments and, thus, could choose from a wide range of revision strategies. Studies 2 and 3 tested children's performance by removing some of these complexities. Study 2 addressed the question whether children may not have differentiated between good and poor advice in the first place because advice quality varied within the advisor and across trials. To this end, children were faced with a very similar task which, however, required belief *formation* rather than belief *revision*. Participants were confronted with two informants who had perceptual access of different quality (one looked through the clear, the other one through the blurred window) and were asked to choose between them (like in Study 1, perceptual access for a given informant varied across trials). Children now had little difficulty in choosing the agent with the better informational access. Study 3 addressed the question whether the continuous response format and the subsequent requirement to integrate two continuous judgments may have made the task in Study 1 artificially difficult. To this end, children were faced with a very similar task as in Study 1 with the crucial difference that the belief revision required was categorical rather than continuous. Results revealed that children now revised their judgments selectively, heeding advice more often when they themselves saw poorly, and also when the advisor saw more clearly. Thus, children in Study 3 behaved qualitatively in analogous ways to the adults in Study 1.

Taken together, these results suggest that young children are capable, in principle, of selective social advice-taking, revising their beliefs as a function of their own and of informants' perceptual access. This capacity, however, seems still fragile and easily gets masked by performance factors. These findings converge with and complement existing research in different areas related to children's developing selective trust, meta-cognition and perspective-taking. They converge with extant research on selective trust in showing that children are selective in who to learn from, and are so in context-sensitive ways: what counts for selectivity is not so much who the informants are, but how they are epistemically situated (Brosseau-Liard & Birch, 2011). They complement and go beyond previous research on selective trust in showing that children not only acquire new beliefs selectively, but revise existing ones selectively in response to other informants' advice. And they do so selectively not only as a function of the informant's epistemic status (revising more in response to epistemically better situated informants), but also as a function of their own (revising more under own poor epistemic conditions). This form of selective belief-revision thus constitutes a form of practical or procedural meta-cognition (e.g. Schneider & Lockl, 2008): in the way they selectively revise their beliefs, children reveal (at least implicit) meta-cognitive sensitivity. How this procedural and implicit meta-cognitive awareness in selective learning and belief revision relates to declarative and explicit forms of meta-cognition is an interesting question for future research (for analogous debates regarding the relation of procedural and declarative meta-cognition in diverse areas of developmental and comparative research, see Beran, Brandl, Perner, & Proust, 2012).

The present findings also complement existing research on children's perspective-taking and selective learning. Many studies have shown that children use level I perspective-taking (Flavell, Everett, Croft, & Flavell, 1981) in their selective learning, tracking whether or not someone can see something and thus has relevant visual access in their selective learning. The present study goes beyond this in showing a more nuanced and fine-grained form of perspective-taking: children not only track *whether* or not someone sees something, but *how well* someone can see. In fact, the development of this form of perspective-taking warrants more attention in and of itself. It clearly goes beyond level I perspective-taking (tracking *whether* someone can see something/*what* someone can see), but also does not simply reduce to level II perspective-taking (tracking how things appear to someone).

Conclusion and outlook

All in all, the present findings show that young children are capable, in principle, of selective social belief revision or advice-taking: Under the right circumstances, children were cable to revise their judgments more when their own initial information was of poor quality, and more so in response to an advisor whose informational access was better.

These findings go beyond previous studies on selective social learning in two crucial ways: they suggest that children do not only selectively acquire new beliefs from others, but also engage in selective belief *revision* – a more complex and demanding epistemic practice. And they suggest that children do so in contexts in which their initial judgments reflect proper beliefs (of varying certainty) rather than mere guesses. At the same time, many crucial questions remain open for future research. First of all, as discussed above, it remains unclear under which circumstances this fragile early competence gets realized, and under which circumstances it remains masked by performance factors. Second, what are the trajectories, and what the underlying motors, regarding both the origins earlier in ontogeny and subsequent development from these early basic forms of selective advice-taking to fully-fledged and more sophisticated adult forms of social belief revision? In particular, what role may growing capacities for perspective-taking and meta-cognition play in the developmental progression toward more sophisticated forms of selective social belief-revision?

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