Title:

Selective social belief revision in young children

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Declaration of interest

none

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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 while 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 consider both their own information and that of the advisor in their selective social belief revision.

Selective social belief revision in young children

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 disguise reality, whether out of ignorance, verbal inaccuracy or bad intentions. And not in all situations should one privilege others' testimony 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 and confident over unconfident informants (e.g., König & Harris, 2005; Harris & Corriveau, 2011; 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 et al., 2015, 2018a, 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*, not about selective social belief *revision*. But in many real-life situations, we do not come completely ignorant in search of completely novel information. Rather, we do come with pre-existing beliefs and judgments, and the fundamental challenge is to flexibly and selectively update and revise beliefs in light of new social information.

Such social revision of beliefs, for example, 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; Snizek & 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 subjects 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; Yaniv & Kleinberger, 2000; Soll & Larrick, 2009). Findings of this research show that adults do heed advice in a sensible, yet 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 & Kleinberger, 2000; Yaniv, 2004a). But it is functional such that adults revise their judgments in light of advice in a selective and systematic 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 weight 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; Snizek & 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, other than in adult social psychology, 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, modelled 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 clear from control tasks (probing children's judgments in private rather

than public situations) that children display compliance rather than actual belief revisions, that is, publically they do not express their true judgment or belief, but just pay lip-service to the majority opinion (Corriveau, 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 research has shown that children are sensitive to others’ judgments when their own perceptual evidence is ambiguous (Jaswal & Markman, 2007; Jaswal, 2004). 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 categorisation on visual characteristics (categorization as “cat”), whereas children who heard an adult categorise 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 subjects 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 et al., 1992) or tactile information when asked about an object’s color (Whitecomb & 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). Furthermore, starting at 4 four years of age, children are able to weigh conflicting information that is gathered either by direct observation or by testimony according to its reliability. For example, they excuse past inaccuracy caused by limited informational access (Nurmsoo & Robinson; 2009) or ignore false testimony and rely on what they themselves have perceived (Ma & Ganea; 2010; Lane; 2018; Tamis-LeMonda et al., 2008).

While these studies investigated categorical belief revision, shifting entirely from one belief to the other (e.g., banana versus lemon, or red versus green), Rakoczy, Ehrling, Harris and Schultze (2015) investigated fine-grained belief revision and allowed participants to partially adapt their initial judgments. 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 unclear whether children's ability to selectively revise beliefs holds when the initial beliefs are more than pure guesses, for example, because they are grounded in children's prior knowledge or observation.

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 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 analysed 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. Importantly, this task provides children with actual sensory

evidence to base their judgments on, differentiating it from mere guesses. 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.

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 (König & Harries, 2005) and are willing to revise guesses following an informant that had better visual access (Whitecomb & Robinson, 2000). We anticipated, therefore, that children, just like adults, would take more advice from an advisor with better information, and that children would take more advice when they themselves are poorly informed than when they are better informed. However, our results showed that children (unlike adults) only adjusted their degree of advice-taking as 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.

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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.

Study 1

Method

Participants.

Thirty-five adults (age range: 18-44 years, $M = 24.7$ years, 26 women) and forty 4-to 6 year-olds (age range: 48-71 months, $M = 59.5$ months, 23 girls) were included in the final sample. Subjects 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 ($n = 3$). Participants were tested individually in the lab.

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 20cm length were used. Sticks were placed in a triangular box (edge length of triangle: 57cm, height: 44cm). The box consisted of a clear window side, a blurred window side (window measurements: 27.5cm $\times$ 23.5cm), and an opaque backside (see Fig.1). At the clear window, the black mark was easily visible while at the blurred window it was hardly visible. Participants made their judgment by indicating on an analogue scale (a picture of the stick of the same length) where the mark was. Advice consisted in randomized judgments, ranging from one to 200mm, 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 (8cm $\times$ 29cm) consisting of five eyes ranging from small to big. For confidence

ratings (how confident subjects were of their judgments), we presented them with a 5-point scale consisting of 5 thumbs ranging from one pointing straight down to one pointing straight up (see Fig.1). Adults rated visibility and confidence using a standard 5-point Likert scale.

Fig.1

Design and procedure.

The task was presented in JAS format with participants taking the role of judges. Advisors were confederates of the experimenter who presented the judges with the randomized advice. We used a 2 (age groups) $\times$ 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 subjects received two trials of each condition. Order of conditions was counterbalanced across subjects.

Every session started with an introduction phase prior to the test phase. In the introduction phase, participants were, in a first step, familiarized with the set-up 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 the confidence rating scale. Examples were used to illustrate the different points of the confidence scale. The thumb up symbol was introduced by presenting a toy frog with the sentence “This is a frog. I am absolutely sure about this.”. Point three of the scale - a horizontal thumb - was introduced by presenting a picture of a hybrid animal of pig and fish (see Fig. 8, Appendix) and the sentences “This is a pig. But could also be fish. Now I am not so sure.”. For the thumb down symbol an undefined soft toy was used and presented with the

sentence “I do not know what this is. Now I am not sure at all”. Subsequently children were asked to show the “absolutely sure – thumb”, the “not so sure – thumb” and the “not sure at all – thumb”. When they answered incorrectly the introduction of the confidence scale and comprehension question was repeated two times. When participants failed the comprehension question three times the experimenter moved on. Subsequently, the visibility rating scale was introduced in similar ways. The biggest eye was introduced by pointing at a toy duck located in a corner of the test room and saying “Now you see this duck really clearly.” To introduce point three, a medium size eye, of the scale children were allow to put on sun glasses and the experimenter said “Now you can see the duck not so clearly”. Point one, the smallest eye, was introduced by asking children to close their eye and pointing out “Now you do not see the duck at all.”. Children were then asked to show the symbol for “seeing clearly”, “seeing not so clearly” and “do not see at all”. If they failed to answer this question correctly, the introduction of the visibility scale was repeated two times before the experimenter moved on. 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.

Test phase.

Judge and advisor were each placed at one side of the box (for illustration, see Fig. 2). Then one stick was put in the box (in such a way that subjects could not observe the placement), and both judge and advisor were asked to make their initial judgments. Then, judges were seated at the opaque side of the box, received the scale of the advisor in addition to the scale with their own initial judgment, and were asked to make their final judgment on a

third scale. Judges rated visibility and confidence both for their initial and for their final judgment.

Fig.2

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).

Results

Preliminary analyses.

Analysis of our manipulation check showed that the design of the set-up was valid. The participants' visibility and confidence ratings as well as accuracy (indicated by the deviation from the true value) 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 Fig. 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: $F(1, 76) = 72.44, p < .001, \eta_p^2 = 0.49$). Post-hoc tests 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, $F(1, 72) = 86.96, p < .001, \eta_p^2 = 0.55$. Post-hoc tests 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 the success of our experimental manipulations.

Fig.3

Main analyses.

Performance in advice-taking as a function of age groups and condition is depicted in Fig. 4. A 3 (condition) $\times$ 2 (age groups) ANOVA showed a significant main effect of condition ($F(2, 73) = 21.58, p < .001, \eta_p^2 = 0.23$), 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 ($F(2, 73) = 5.10, p < .001, \eta_p^2 = 0.06$).

Post-hoc tests 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 ($t(57.01) = 1.67, p = .100, d = 0.44$).

Fig.4***Visibility and confidence rating.***

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

Visibility rating.

The 2 $\times$ 3 $\times$ 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.

Confidence rating.

The 2 $\times$ 3 $\times$ 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) $\times 2$ (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: $t(39) = 1.57, p = .124, d_z = 0.25$).

Fig.5

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 functionally in regard to 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; Snizek & 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. In line with the interpretation that children did not consider the advisor's epistemic status, their confidence ratings were not influenced substantially by the quality of the advice. In particular, and other than adults, when in poor epistemic circumstances, children did not become more confident after getting high quality advice. Thereby, the confidence findings largely mirror the findings regarding advice-taking.

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 increased task difficulty relative to the Rakoczy et al. (2015) study. First, advice quality varied 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 disjunct 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 that did not involve any belief revision (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 by presenting to children a categorical instead of a continuous scale (Study 3). This allowed us to test whether children are capable, in principle, to engage 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 material as in Study 1 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 research 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). Study 2 allows us to test whether children's ability to infer the quality of advice holds if this quality varies within advisors.

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. Subjects 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 about the right height of the black mark on a third unused scale (Endorse measure).

In Study 2, children were familiarized with stimuli and box and subsequently participated in eight test trials in total. Trials were of two kinds (four trials each). In half of the trials, the child was sitting at the opaque side of the box. In the other half of 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. To avoid preference for one side, informants switched position every two trials. Data for Study 2 and 3 were collected within one test session lasting 30min. All test session were fully videotaped. Children participated first in Study 3 and afterwards in Study 2.

The dependent measures were as follows:

- *Ask-measure*: Children were presented with a forced-choice question whom 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*: We calculated the mean number of trials (out of eight) for each of three categories (high quality informant, low quality informant, and between). Children's plotting within 2cm around the judgment of the high or low quality informant was coded as choice of this informant, respectively. Judgments in the interval between both radii were coded as "between".

Results and Discussion

Performance in the Ask- and Endorse-measures as a function of informant quality is depicted in Fig. 6.

Only 0.6% of trials fall in the "between" 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 logistic 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. 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). Participants were treated as the level 2 grouping variable, and the

model included random intercepts as well as random slopes for position of the child. 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: $B = 2.95$ ($SE = 0.59$), $z = 5.01$, $p < .001$; Endorse: $B = 1.65$ ($SE = 0.32$), $z = 5.17$, $p < .001$). 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: $B = 0.76$ ($SE = 0.87$), $z = 0.88$, $p = .380$; Endorse: $B = -0.31$ ($SE = 0.44$), $z = -0.69$, $p = .488$). 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.

Fig.6

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. 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) made the task in Study 1 overly difficult.

Study 3

In Study 3, we simplified the decision process by using categorical rather than continuous belief revision. The rationale is to investigate whether 4- to 6-year-olds are able to revise

existing beliefs as a function of their own and the advisor's epistemic state when task demands are reduced (compared to Study 1).

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 affixed 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. Advice 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 adopted the judgment of the advisor or retained their initial judgment.

Results and discussion.

The proportion of trials in which children engaged in advice-taking as a function of condition is depicted in Fig. 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*) as fixed effect. 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. 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, $B = -2.62$ ($SE = 0.55$), $z = -4.79$, $p < .001$. The proportion of accepting advice differed 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, $B = 2.38$ ($SE = 0.54$), $z = 4.41$, $p < .001$. 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, $B = 4.05$ ($SE = 0.89$), $z = 4.56$, $p < .001$.

Fig.7

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. Adult's advice-taking as a function of their own and the advisor's competence is also a robust finding in social psychology research.

Moreover, this finding is consistent with the idea 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 rather than by a principled limitation in their competence for selective social belief revision. The requirement to make judgments on a continuous scale and then integrate two such continuous judgments rather than choosing one answers could pose cognitively so demanding to children that they no longer consider the quality of advice despite their in-principle ability to do so.

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. Participants were given a perceptual task in the format of a judge-advisor-system (JAS) that was suitable for both adults and children. In this paradigm, subjects make an initial judgment, next hear about an advisor's judgment, and then are asked to make their final judgment – allowing the parametric calculation of the degree to which subjects take the advice into account. In different conditions, the quality of the subject's and the advisor's perceptual access was manipulated by having them look through clear or blurred windows.

The main results of the present studies were the following: In Study 1, adults engaged in selective advice-taking consistent 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 advisors' (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 finding seems at odds with previous research on advice-taking in children which showed that children placed more weight on more competent advisors (Rakoczy et al., 2015).

Thus, we conducted two additional studies to investigate whether children not considering their advisor's visual access in Study 1 reflects true limitations in young children's competence of selective belief revision or mere performance limitations due to specific demands of the current task. Comparing our task to that of the Rakcozy et al. (2015) study, we identified three characteristics that may have made the task in Study 1 cognitively very demanding for children. First, participants had to keep track of both their own and another agent's perceptual access, which varied across trials. Second, they made continuous judgments rather than choosing one out of a small set of responses. Third, they could choose from a wide range of revision strategies in response to the advisor's judgment. 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 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*. Subjects 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. Children now had little difficulty in choosing the agent with the better informational access, even though it varied within advisors as in Study 1. Thus, Study 2 speaks to young children's ability to track the quality of advice by considering relevant situational factors. 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 so difficult that children were no longer willing or able to track the advisor's visual access or to consider it when making their final judgments. To this end, children were faced with a very similar task with the crucial difference that the belief revision required was categorical rather than continuous. Results revealed that children now revised their judgments in a functional manner, heeding advice more often when they themselves saw poorly, and also when the advisor saw more clearly. Thus, children in Study 3 behaved qualitatively similar to

the adults in Study 1, and the way they selectively heeded advice also mirrors earlier research on advice-taking in young children (Rakoczy et al., 2015).

Taken together, these results suggest that young children are capable, in principle, of selective social advice-taking, revising their beliefs quite sensibly as a function of their own and of informants' perceptual access. This capacity, however, seems still fragile and easily gets masked by performance factors. One such limiting performance factor might be the pragmatic structure and demands of the task. The JAS, by its nature, involves repeated requests of a participants' judgment within the same task. Children might have unsystematically revised their judgments on pragmatic ground simply because they thought the repeated question implied that their first answer was wrong. Indeed, previous research suggests that children do tend to revise judgments when asked repeatedly (Siegal et al., 1988). In addition, the task in Study 1 may have been difficult for children since they needed to take into account and bring to bear a lot of background knowledge about conditions for visual perception and its relation to information and knowledge; and recent theoretical and empirical work suggests that sophisticated selective learning builds on and presupposes specific background knowledge (Hermes et al., 2018a; Sobel & Kushnir, 2013).

Another limiting factor may have been due to the fact that, at any time, subjects in Study 1 were only confronted with one single advisor. The lack of a contrast to another informant may have made it difficult not to indiscriminately take into account this advice, independent of its quality. This possibility seems plausible in light of existing research: Most studies that document competence for selective trust have confronted children with two informants that differ with respect to the quality of their information. In contrast, in studies with only one informant, children have often found it very difficult to overcome uncritical trust in an informant even if this informant was obviously unreliable or incompetent (Jaswal

et al., 2010; Vanderbilt et al., 2011; 2014; for a critical discussion see Jaswal & Kondrad, 2016).

As a consequence, this demand to selectively suppress or modulate naïve trust towards the advisor (depending on her epistemic position) may have posed too high demands on children's executive function. Related research suggests that any task that requires the coordination of diverging perspectives, in particular tasks in which one of these perspectives pertains to one's own current beliefs, are taxing in terms of executive function (Fizke et al., 2014; Rakoczy, 2010). In line with this, previous research found that the ability to overcome indiscriminate trust towards single informants builds on the development of executive function (Jaswal & Pérez-Edgar, 2014). Similar research suggests that children have both sophisticated rational and less sophisticated heuristic strategies for social learning in their cognitive repertoire in parallel; and that they use the former if the executive and other demands of the tasks allow for it, but revert to the latter otherwise (Hermes et al., 2018b; in press). Something similar may have happened in the current Study 1: Due to an overwhelming task complexity (presenting a continuous measure in combination with general demands of the task), children may have fallen back on simpler heuristics such as “If you see clearly, take little advice; if you see poorly, take more advice”.

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 capable of revising 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, of the development from these early basic forms of selective advice-taking to fully-fledged and more sophisticated adult forms of social belief revision? Do children simply get more accurate and epistemically sensitive over time, or are there qualitative developmental leaps? One particularly interesting question in this context is whether there may be seemingly paradoxical developmental patterns such that social belief revision, in some sense, gets worse over developmental time. From much research in social psychology we know that adults weight advice in a selective and systematic but not fully rational fashion (for reviews, see Bonaccio & Dalal, 2006; Feng & Mac George, 2006; Snizek & Buckley, 1995). In particular, adult advice-taking is limited by the fact that subjects weight others' advice less than would be optimal, a phenomenon known as *egocentric advice discounting* (EAD, Yaniv & Kleinberger, 2000; Yaniv, 2004b).

From a cognitive point of view, it is not fully clear what the basis of EAD in adults is. And from a developmental point of view, it is unclear how these limits of advice-taking develop. Broadly, two courses of development are possible: First, and in line with naïve intuition, children may become more rational and less egocentric over time (following the general trend of declining egocentrism). Second, and much more counter-intuitively, the reverse may be true: in a sort of developmental paradox, children may actually start out more rational than adults and only get worse over development. Such a possibility is in line with an exciting idea recently discussed in cognitive and comparative psychology. According to this

idea, animals are sometimes more rational than humans; and the reason is that they lack some of the experience it needs to develop less-than-fully-rational heuristics (Stanovich, 2013). Analogous patterns may apply to the development of advice-taking: The cognitive foundations of EAD may be complex and thus require protracted development. One promising account in the advice-taking literature – the so-called “privacy of thoughts” approach – explains EAD in terms of metacognitive processes. The basic claim here is that adults prefer to stick with their own judgment because they have more insight in their own reasons compared to those of the advisor (Yaniv & Kleinberger, 2000). Another account proposes the *mere ownership effect* (Beggan, 1992) as a mechanism underlying EAD. According to this account, adults see opinions as intellectual property, with increased subjective value of one’s own ideas (Abelson & Prentice, 1989). Advice might thus be perceived as devaluing one’s own ideas and discounted as a consequence. From a developmental point of view, we know that both metacognitive abilities (Kloo & Rohwer, 2012) and a basic notion of intellectual property (Shaw et al, 2012) do develop in protracted fashion. If these accounts are right, the development of advice-taking may indeed reveal more complex and seemingly paradoxical patterns. Systematic future research that implement comparable paradigms in various age-groups is needed to test for exciting developmental patterns.

Conclusion

Taken together, the present three studies give an idea about the capacity for and the limits of selective social belief revision in young children. In suitably simplified tasks, 4- to 6-year-olds revised their perceptual judgments selectively as a function of their own and an advisor’s epistemic status. These capacities seem to be still fragile and to get easily masked by performance factors. Future research will need to uncover more systematically how

selective social belief revision becomes more solid over subsequent development and to describe its broader ontogenetic trajectories.

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Figures

Fig.1. Three-sided box with clear and blurred window (a). Colored sticks (b), rating scales for visibility (c) and confidence (d).

Fig.2. High-poor condition of AT-part with child judge.

Fig.3. Children's and adults' subjective visibility (a), confidence (b) and deviation from the true value (c) behind the clear and blurred window. Error bars indicate standard error of the means.

Fig.4. Advice-taking as a function of condition. Error bars indicate standard error of the means.

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

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Fig.4. Advice-taking as a function of condition. Error bars indicate standard error of the means.

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

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

Fig.7. 100% Advice-taking of children as a function of condition.

Appendix

Pretest for stimuli selection

Method

In Study 1 and 3 we used eight different wooden sticks. These stimuli were selected from a pretest out of 16 designs to ensure that these stimuli produced significantly more accurate judgments behind the clear than behind the blurred window. Thirteen adults participated in this pretest, every adult judged the stimulus first behind the blurred and second behind the clear window. Sticks were judged by the participants in respect to the location of the black mark, the visibility of the black mark and the confidence in their judgment.

Results

Visibility and confidence were judged significantly higher behind the clear than behind the blurred window for all sticks (linear contrasts, always $p < .01$). But only for eight sticks the deviations from the true value of the black mark was significantly smaller behind the clear than behind the blurred window (p -values see Tab. 1). These eight sticks were used in Study 1, 2 and 3.

Stimuli	Blue	Brown	Multicolor	Lightgreen	Purple	Black- White	Black	Lightblue
p-value	.000	.050	.042	.003	.002	.010	.002	.027
Significant	yes	yes	yes	yes	yes	yes	yes	yes

Tab.1. Deviation from the true value of the black mark for the eight sticks that were used in the Study 1, 2, and 3. Linear contrasts between the clear and the blurred window.

Stimuli	3- color	Yellow	Striped	Green	Spotted	Orange	Pink	Lightpink

p-value	.432	.918	.906	.087	.167	.113	.113	.414
Significant	no	no	no	no	no	no	no	no

Tab.1. Deviation from the true location of the black mark for the sticks not used Study 1, 2, and 3. Linear contrasts between the clear and the blurred window.

Study 1

Introduction phase

Fig.8. Picture of the hybrid animal of pig and fish used in the introduction phase to introduce point three of the confidence scale – a horizontal thumb.

Application of the confidence and visibility scales.

Method.

Material.

To control for comprehension of the confidence scale we used three marble runs. The first marble run had one opening (one possibility where the marble could run down), the second marble run had two openings, and the third marble run had three openings.

Procedure.

After being introduced to the five symbols of the confidence scale, children were asked to show the “absolutely sure – thumb”, the “not so sure – thumb” and the “not sure at all – thumb”. When answered wrongly, the introduction of the confidence scale and comprehension question was repeated two times. When participants failed the comprehension question three times the experimenter moved on. Subsequently, we tested whether children could use the confidence scale for an example. We presented three marble runs and children were asked for each marble run in which of the openings the marble would run down and after their decision they were asked to rate their confidence in this decision using the five-thumb-scale.

After being introduced to the five symbols of the visibility scale children were asked to show the symbol for “seeing clearly”, “seeing not so clearly” and “do not see at all”. If they failed to answer this question correctly the introduction of the visibility scale was repeated two times before the experimenter moved on.

Results.*Confidence scale.*

After at most three explanations, five out of 43 children could not match the terms “really sure”, “not so sure”, and “not sure at all” with the correct symbol.

Difference of confidence between the three marble runs.

Confidence ratings as a function of marble run are depicted in Fig. 9. We calculated an ANOVA for repeated measures with number of marble run as within factor. We found significant differences between the three marble runs ($F(1, 42) = 12.69, p < .001, \eta_p^2 = 0.30$).

Post-hoc *t*-tests revealed that participants judged their confidence higher for a marble run with one exit than for a marble run with two or three exits (M1 vs. M2: $t(42) = 4.15, p <$

.001, $d_z = 0.64$; M1 vs. M3: $t(42) = 4.20$, $p < .001$, $d_z = 0.65$) but made no difference between a marble run with two or three exits (M2 vs. M3: $t(42) = 0.45$, $p = .652$, $d_z = 0.07$).

Fig.9. Mean confidence as a function of marble run number. Error bars indicate standard error of the means.

Marble runs (M). We distinguished three levels of understanding:

High level: Children that scored their confidence for a marble run with two exits smaller than for a marble run with one exit and three exits smaller than two exits ($M3 < M2 < M1$). Six children out of 40 children judged their confidence on a high level.

Medium level: Children that scored their confidence for a marble run with two or three exits smaller than for a marble run with one exit ($M3$ and $M2 < M1$). Nine children out of 40 children judged their confidence on a medium level.

Low level: Children that used any other combination of confidence scores for the three different marble runs. Twenty-five children out of 40 children used other combinations.

	High	Medium	Low
Level of understanding	($M3 < M2 <$	($M3$ and $M2 <$	(rest)

	M1)	M1)	
Number of participants out of 40	6	9	25

Tab.2. Number of participants per level of understanding.

Visibility scale.

After three explanations, seven out of 43 children could not match the terms “really good”, “not so good”, and “not good at all” with the correct symbol.

Stimuli and apparatus validation.

Transfer of black line - Blue stick without box.

Method.

Procedure. One stick was placed on a table, after participants could explore the stimulus they were presented with the image of the stick and the experimenter pointed out that the black mark is missing on the image of the stick. Participants were asked to transfer the mark as precisely as possible onto the image of the blue stick. Subsequently, participants were asked for their confidence rating.

Results.

Deviation from the true value and confidence ratings as a function of age groups are depicted in Fig. 10. Children deviated significantly more from the true value of the black mark than adults, adults drew the line more precise ($t(55.49) = 4.13, p < .001, d = 0.94$). Children and adults did not differ when judging their confidence of one stick without box ($t(73) = 0.45, p = .646, d = 0.11$).

Fig.10. Deviation from the true value and confidence for one stick without box. Error bars indicate standard error of the means.

Visibility behind the two windows (without sticks).

After inspecting the box participants judged their visibility behind both windows (without sticks).

Results.

Visibility ratings of children and adults are depicted in Fig. 11. Visibility was analyzed with an ANOVA for repeated measures with window (clear or blurred) as within-subjects variable and age group (children or adults) as between-subjects factor. There were significant main effects of window ($F(1, 76) = 367.49, p < .001, \eta_p^2 = 0.83$) and age group ($F(1, 76) = 18.38; p < .001, \eta_p^2 = 0.19$) but no interaction of window and age group ($F(1, 76) = 3.54, p = 0.064, \eta_p^2 = 0.04$). Post-hoc tests revealed that, first, both children and adults judged their visibility higher at the clear than at the blurred window (children: $t(42) = 10.94, p < .001, d_z = 1.69$; adults: $t(34) = 19.7, p < .001, d_z = 3.38$) but second, that children and adults differed when judging their visibility behind each window (clear window: $t(76) = 2.18, p = .032, d = 0.50$; blurred window: $t(46.17) = 4.79, p < .001, d = 1.41$).

Fig.11. Visibility of the two windows with empty box. Error bars indicate standard error of the means.

Test phase

Individual belief revision (IBR).

Before participating in the advice-taking part of Study 1 children and adults participated in a individual belief revision version. The same material was used like in the advice-taking part.

Method.

Procedure.

In the IBR part of Study 1, the first and second judgment were given by the participant, so that the third judgment was an integration of her own previous two judgments. For the third judgment, the participant moved to the opaque backside of the box. There she had three images of the current stimulus – her first judgment, her second judgment, and an unused image. Participants then combined their first and second judgment into a final judgment on the third image. We collected confidence ratings for all three judgments. Children rated their visibility for their first and second judgment while adults additionally

gave an overall visibility score for their third judgment. We counter-balanced whether participants started behind the clear or blurred window.

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 on a Likert scale ranging from 1 (low) to 5 (high).
- *Confidence rating:* Children accessed their own confidence for all judgments on a 5-point scale (5 thumb ranging from thumb down to thumb up). Adults were asked to rate their confidence on another Likert scale ranging from 1 (low) to 5 (high).

To analyze individual belief revision, we used two alternative measures because of two paradigmatic problems: First, repetitive questioning about an uncertain judgment produced partly random final judgments in children. This behavior, in turn, produced implausibly high IBR values. Second, in adults, the IBR values were artificially inflated in cases where adults intended to make identical first and second judgments. In those cases, imprecisions in the use of the scale resulted in small (i.e., only a few millimeters) but unsystematic differences between the three judgments. These unsystematic differences, in turn, produced implausibly high IBR values that suggested extreme adjustments whereas the de-facto adjustments were negligible and, most likely, unintended.

- *Individual belief revision (IBR):* To calculate an IBR score we adapted the equation defined by Harvey & Fischer (1997) as advice-taking to

$$IBR = \frac{estimate\_3 - estimate\_1}{estimate\_2 - estimate\_1} \times 100\%.$$
IBR scores were limited to 100% and -100%.
- *Correlation of first and second judgment:* As alternative measure we calculated Pearson correlation of the first and second judgment.

Results.

For the sake of completeness we report results using both measures.

IBR.

Performance in individual belief revision as a function of age groups and condition is depicted in Fig. 12. To analyze differences between the three conditions, *high-poor* (hp), *poor-poor* (pp), and *poor-high* (ph), we conducted an 3×2 ANOVA for repeated measures with condition (hp vs. pp vs. ph) as within-subjects variable and age group (children vs. adults) as between-subjects factor. Results showed significant main effects of condition and age group (condition: $F(1,72) = 14.15$, $p < .001$, $\eta_p^2 = 0.16$; age group: $F(1, 72) = 5.65$, $p = .02$, $\eta_p^2 = 0.73$) but no interaction between both factors ($F(1, 72) = 0.06$, $p = .938$, $\eta_p^2 = 0.001$). Post-hoc tests revealed that children and adults did not differ between the *high-poor* and *poor-poor* condition (children: $t(39) = 0.04$, $p = .964$, $d_z = 0.01$; adults: $t(33) = -0.02$, $p = .983$, $d_z = -0.003$) but for both the *poor-high* condition differed from the other two conditions (children - ph vs. pp: $t(39) = -3.01$, $p = .005$, $d_z = -0.48$, children - ph vs. hp: $t(39) = -2.57$, $p = .014$, $d_z = -0.41$; adults - ph vs. pp: $t(33) = -5.18$, $p < .001$, $d_z = -0.90$, adults - ph vs. hp: $t(34) = -4.19$, $p < .001$, $d_z = -0.72$).

Fig. 12. IBR as a function of condition. Error bars indicate standard error of the means.

Correlation of first and second judgment.

Following the two paradigmatic problems of the IBR equation, we calculated an alternative measure of individual belief revision – the correlation of the first and second judgment (see Ta. 3). Similar to a retest-reliability, high correlations indicate relatively similar judgments, whereas low correlations indicate that participants revised their first judgments. Results showed that participants revised their beliefs only when moving from the blurred to the clear window (*poor-high* condition), and for adults this was true only when their first judgment deviated at least 3 cm from the true value (i.e., when their initial error was large enough to warrant correction).

	r	P	r-trial 1	p-trial 1	r-trial 2	p-trial 2	correlation
high-poor							
children	.70	.00	.78	.00	.48	.00	yes
adults	.99	.00	.98	.00	.98	.00	yes
poor-poor							
children	.61	.00	.71	.00	.53	.00	yes
adults	.89	.00	.93	.00	.81	.00	yes
poor-high							
children	-.05	.67	.11	.49	-.04	.79	no
adults	.57	.00	.5	.00	.42	.01	yes
<i>Distance between first judgment and true value smaller</i>							
<i>3cm</i>							
adults	.92	.00	.81	.00	.76	.02	
<i>Distance between first judgment and true value >= 3cm</i>							
adults	.98	.00	.13	.87	-.27	.83	yes/no
<i>Distance between first judgment and true value >= 5cm</i>							

adults	.27	.07	.40	.08	.15	.49	no

Tab.3. Pearson correlation for all three conditions. The first two columns show the results collapsed across the two trials per condition. Columns three to six show the correlations on the level of the individual trials, and the last column summarizes the results of the calculations.

Visibility and confidence rating.

Performance in the visibility (a) and confidence (b) ratings as a function of age groups and conditions is depicted in Fig. 13. For visibility the target analysis was a 2 (age group) $\times$ 3 (condition) $\times$ 2 (number of judgment: 1 vs. 2) ANOVA and for confidence ratings it was 2 (age group) $\times$ 3 (condition) $\times$ 3 (number of judgment: 1 vs. 2 vs. 3) ANOVA.

Visibility rating.

The 2 $\times$ 3 $\times$ 2 ANOVA revealed a significant main effect for condition ($F(2, 73) = 233.36, p < .001, \eta_p^2 = 0.76$) and age group ($F(1, 73) = 7.03, p = .010, \eta_p^2 = 0.09$) but no effect for number of judgment ($F(1, 73) = 3.61, p = .061, \eta_p^2 = 0.05$). There was a significant interaction of condition and judgment ($F(2, 73) = 508.81, p < .001, \eta_p^2 = 0.87$), but no interaction of condition and age group ($F(2, 73) = 1.33, p = .268, \eta_p^2 = 0.02$) or judgment and age group ($F(1, 73) = 0.38, p = .540, \eta_p^2 = 0.005$). Furthermore, we found a significant interaction of all three factors ($F(2, 73) = 12.36, p < .001, \eta_p^2 = 0.14$). To follow up on this 3-way interaction, separate 2 (age group) $\times$ 2 (judgment: 1 vs. 2) ANOVAs were conducted for each condition.

In the *high-poor* condition the 2 $\times$ 2 ANOVA showed a significant main effect for number of judgment ($F(1, 73) = 531.12, p < .001, \eta_p^2 = 0.88$) but no effect for age group ($F(1, 73) = 3.67, p = .059, \eta_p^2 = 0.05$) and a significant interaction of both factors ($F(1, 73) = 12.96, p < .001, \eta_p^2 = 0.15$). Children and adults rated their visibility to be lower after moving to the blurred window (children: $t(39) = 11.04, p < .001, d_z = 1.77$; adults: $t(34) = 37.73, p < .001,$

$d_z = 6.47$). Adults rated their overall visibility different from their first ($t(33) = 4.6, p < .001, d_z = 0.80$) and second judgment ($t(33) = 15.9, p < .001, d_z = 2.77$). Furthermore, children and adults differed in their visibility ratings for their first ($t(73) = -2.35, p = .018, d = 0.55$) and second judgment ($t(73) = 2.87, p < .001, d = 0.67$). In the *poor-poor* condition, the ANOVA showed significant main effects for number of judgment ($F(1, 73) = 4.26, p = .043, \eta_p^2 = 0.05$) and age group ($F(1, 73) = 4.76, p = .032, \eta_p^2 = 0.06$) but no interaction of both factors ($F(1, 73) = 0.57, p = .452, \eta_p^2 = 0.01$). Adults increased their visibility ratings after judging the stimulus a second time through the blurred window (judgment 1 vs. judgment 2: $t(34) = -2.22, p = .033, d_z = -0.38$). In the *poor-high* condition, the ANOVA showed significant main effects for number of judgment ($F(1, 73) = 522.33, p < .001, \eta_p^2 = 0.88$), age group ($F(1, 73) = 9.04, p < .004, \eta_p^2 = 0.11$) and a significant interaction of both factors ($F(1, 73) = 12.81, p < .01, \eta_p^2 = 0.15$). Post-hoc test revealed children and adults rated their visibility higher after moving from the blurred to the clear window (children: $t(39) = 11.53, p < .001, d_z = 1.85$; adults: judgment 1 vs. judgment 2: $t(34) = 27.64, p < .001, d_z = 4.74$, judgment 1 vs. judgment 3: $t(33) = 17.84, p < .001, d_z = 3.10$, judgment 2 vs. judgment 3: $t(33) = 4.32, p < .001, d_z = 0.75$).

Confidence rating.

The $2 \times 3 \times 3$ ANOVA revealed a significant main effects of condition ($F(2, 73) = 141.79, p < .001, \eta_p^2 = 0.66$), judgment ($F(2, 73) = 61.46, p < .001, \eta_p^2 = 0.46$) and age group ($F(1, 73) = 33.08, p < .001, \eta_p^2 = 0.31$). Furthermore, all interactions were significant: condition and age group ($F(2, 73) = 69.96, p < .001, \eta_p^2 = 0.49$), judgment and age group ($F(2, 73) = 14.74, p < .001, \eta_p^2 = 0.17$), condition and judgment ($F(4, 73) = 115.19, p < .001, \eta_p^2 = 0.61$) as well as condition, judgment and age group ($F(4, 73) = 21.88, p < .001, \eta_p^2 = 0.23$). To follow up on this 3-way interaction, separate 2 (age group) $\times 3$ (judgment: 1 vs. 2 vs. 3) ANOVAs were conducted for each condition.

In the *high-poor* condition the 2×3 ANOVA showed a significant main effect of number of judgment ($F(2, 73) = 27.06, p < .001, \eta_p^2 = 0.27$) but no effect of age group ($F(1, 73) = .12, p = .729, \eta_p^2 = 0.002$) and a significant interaction of both factors (confidence: $F(2, 73) = 4.67, p = .011, \eta_p^2 = 0.06$). Children and adults decreased in confidence between their first and second judgment (children: $t(39) = 3.19, p < .003, d_z = 0.51$; adults: $t(34) = 6.72, p < .001, d_z = 1.15$). Adults rated their overall-visibility different from their first ($t(34) = 3.01, p < .001, d_z = 0.52$) and second judgment ($t(34) = -5.33, p < .005, d_z = -0.91$). In the *poor-poor* condition the 2×3 ANOVA showed significant main effects of number of judgment ($F(2, 73) = 4.41, p = .014, \eta_p^2 = 0.06$) and age group ($F(1, 73) = 79.17, p < .001, \eta_p^2 = 0.52$) but no interaction of both factors ($F(2, 73) = 2.11, p = .125, \eta_p^2 = 0.03$). Children increased in confidence for their final judgment ($t(39) = -3.01, p < .005, d_z = -0.48$). Adults differed in confidence between their first and second judgment ($t(34) = -2.35, p = .025, d_z = -0.40$) as well as between their second and overall judgment : $t(34) = -2.28, p = .029, d_z = -0.38$). In the *poor-high* condition the 2×3 ANOVA showed significant main effects of number of judgment ($F(2, 73) = 236.28, p < .001, \eta_p^2 = 0.76$), age group ($F(1, 73) = 16.9, p < .001, \eta_p^2 = 0.19$) and a significant interaction of both factors ($F(2, 73) = 47.47, p < .001, \eta_p^2 = 0.39$).

Post-hoc tests revealed children and adults rated their confidence for their second and third judgment higher than for their first judgment higher after moving from the blurred to the clear window (children: judgment 1 vs. judgment 2: $t(39) = -5.61, p < .001, d_z = -0.90$; judgment 2 vs. judgment 3: $t(39) = -5.09, p < .001, d_z = -0.81$; adults: judgment 1 vs. judgment 2: $t(34) = 25.39, p < .001, d_z = 4.35$; judgment 1 vs. judgment 3: $t(34) = 28.13, p < .001, d_z = 4.82$).

Fig.13. Visibility and confidence ratings for all judgments given in the IBR condition. For children visibility ratings were only documented for the first and second judgment. Error bars indicate standard error of the means.