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Attending to Relations: Proportional Reasoning in 3- to 6-Year-Old Children

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When proportional information is pit against whole number numerical information, children often attend to the whole number information at the expense of proportional information (e.g., indicating 4/9 is greater than 3/5 because 4 > 3). In the current study, we presented younger (3- to 4-year-olds) and older (5- to 6-year-olds) children a task in which the proportional information was presented either continuously (units cannot be counted) or discretely (countable units; numerical information available). In the discrete conditions, older children showed numerical interference—responding based on the number of pieces instead of the proportion of pieces. However, older children easily overcame this poor strategy selection on discrete trials if they first had some experience with continuous, proportional strategies, suggesting this prevalent reliance on numerical information may be malleable. Younger children, on the other hand, showed difficulty with the proportion task, but showed evidence of proportional reasoning in a simplified estimation-style task, suggesting that younger children may still be developing their proportional and numerical skills in task-dependent ways. Lastly, across both age groups, performance on the proportional reasoning task in continuous contexts, but not discrete contexts, was related to more general analogical reasoning skills. Findings suggest that children's proportional reasoning abilities are actively developing between the ages of 3 and 6 and may depend on domain general reasoning skills. We discuss the implications for this work for both cognitive development and education.

Keywords: proportional reasoning, whole number bias, analogy, discrete and continuous contexts

Learning fraction concepts can be a difficult task for many students (e.g., National Mathematics Advisory Panel, 2008). Children make both procedural and conceptual errors that remain pervasive even in later school grades and into adulthood (e.g., Christou & Vosniadou, 2012; Lortie-Forgues, Tian, & Siegler, 2015; National Mathematics Advisory Panel, 2008; Ni & Zhou, 2005; Vamvakoussi & Vosniadou, 2010). However, fraction concepts have been shown to be an important gatekeeper to many later math skills (Booth & Newton, 2012; Siegler et al., 2012), making it important to understand the specific difficulties children face and how they may be rectified.

These difficulties with formal fraction understanding are surprising given that intuitive reasoning about visually presented proportional information seems to be an early developing skill, with evidence suggesting that even preverbal infants (e.g., Denison & Xu, 2010; McCrink & Wynn, 2007) can process proportional information when presented nonsymbolically. Infants as young as 6 months old have been shown to keep track of the ratio between discrete items (McCrink & Wynn, 2007), and slightly older infants use this ratio information to make probabilistic inferences (Denison, Reed, & Xu, 2013; Denison & Xu, 2010). For example, infants are surprised when an experimenter randomly pulls a red ball from a bin that contains mostly white balls (but some red balls), suggesting that infants track the likelihood of probabilistic outcomes (e.g., Denison et al., 2013).

Despite this early developing proportional reasoning, children show systematic errors in their processing of visual proportional information. For example, Jeong, Levine, and Huttenlocher (2007) taught 6- to 10-year-old children a probability game in which they spun a spinner with two colored sections (red and blue). If the spinner landed on red, they would win stickers, but if it landed on blue, they would lose stickers. Children were then shown pairs of spinners and were asked to judge which spinner was more likely to result in a winning spin. When the probabilistic information presented on the spinners was continuous (i.e., the spinner contained only one large red portion and one large blue portion that were not divided up into smaller discrete pieces), children performed above chance on these judgments. However, when the red and blue sections on the spinner were divided into discrete, countable pieces (examples of discrete and continuous spinners are given later in the text), children performed at chance levels (Jeong et al., 2007). Based on these and similar findings, researchers posit that when discrete countable information is available, children ignore the relation between the number of red and blue pieces, focusing instead on the more salient "numerator"—the total number of winning red pieces, turning a proportional task into a counting one (Boyer, Levine, & Huttenlocher, 2008; Jeong et al., 2007). Importantly, because children succeed (i.e., select the spinner with the greater proportion of red) when presented with continuous spinners but not when presented with discrete spinners, this points to poor

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strategy selection in the presence of specific perceptual features, rather than an inability to process proportional information.

Overattention to whole number information at the expense of proportional information has also been noted when children and adults process symbolic fractions—a phenomenon referred to as a "whole number bias" (e.g., Alibali & Sidney, 2015; Ni & Zhou, 2005). For example, when judging the relative magnitude of two fractions, people often attend to the relative magnitudes of the whole number components (DeWolf & Vosniadou, 2015), while ignoring the relationship between the numerator and denominator (Bonato, Fabbri, Umiltà, & Zorzi, 2007), resulting in poor fraction processing.

Thus, interference of discrete numerical[1] information with relational information processing is pervasive across both symbolic and nonsymbolic representations of proportion in children. Yet, importantly, children's spontaneous attention to relational information has been shown to be an important predictor of formal fraction ability (e.g., McMullen, Hannula-Sormunen, Laakkonen, & Lehtinen, 2016), making it critical that we understand what factors impact whether a child is likely to focus on relational information or instead rely upon discrete numerical information when engaging in proportion tasks. This knowledge can inform our understanding of the difficulties children may encounter when first learning fraction notation, including early precursors to the whole number bias.

In the current study, we aimed to investigate what factors may impact children's strategy selection (i.e., relational reasoning vs. counting) when engaging in discrete proportional tasks. To do this, we explored the impact of four distinct factors thought to impact proportional thinking: (a) direct instruction, (b) prior experience, (c) age (as a proxy for numerical ability), and (d) analogical reasoning with visual patterns.

We hypothesized that children's focus on the absolute number of red pieces, instead of on the relation between the number of red and blue pieces, may be the result of an overattention to the salient "winning" pieces, leading them to ignore other relevant aspects of the display (the blue pieces, total number of pieces, and/or the relative sizes of the pieces). Thus, in our study, we explored whether instructing children to pay attention to the total amount of both the red *and* the blue pieces makes children more likely to focus on the relation between these two amounts.

We also explored whether experience focusing on proportional information, in the absence of conflicting whole number information in continuous trials, may promote the use of relational strategies on subsequent discrete trials. Thus, we varied the order of continuous and discrete spinner blocks in our task, allowing us to assess whether experience on continuous trials promotes performance on subsequent discrete trials. One recent study suggests some promise for this approach in older children; fourth graders (but not second graders or kindergarteners) were less likely to rely on numerical strategies on discrete proportional tasks if initially exposed to practice with continuous proportion (Boyer & Levine, 2015).

A focus on number in the context of a proportional task may be driven by an overall increased focus on whole numbers and counting. If so, then younger children (e.g., preschoolers), for whom numerical information may be less salient—because they are still in the process of mastering the counting procedure and have yet to encounter formal mathematical tasks in school—may be less likely to be swayed by whole number numerical information. Some evidence suggests preschoolers prefer relational information when put in conflict with whole number information (Sophian, Harley, & Manos Martin, 1995); however, other studies report that 3- to 4-year-olds do not use proportional information to make inferences about probabilistic outcomes (Girotto, Fontanari, Gonzalez, Vallortigara, & Blaye, 2016). Thus, there are still several open questions about how (and whether) children this age process proportional information. In the current study, we used an identical paradigm with 3- to 4-year-old and 5- to 6-year-old children in order to explore strategy selection in a discrete proportion task across varying levels of numerical abilities. In addition, because the proportional reasoning abilities of these younger children are still unclear, we wanted to investigate children's ability to think about probability in two contexts: comparing probabilities and predicting outcomes. In this way, we can better look at the development of proportional reasoning through both decision-making type tasks (e.g., "Which spinner is better?") and in estimation type tasks (e.g., "What do you think will happen and how sure are you?") across these distinct age groups.

Lastly, given the importance of early proportional reasoning in predicting formal fraction ability (McMullen et al., 2016), we were also interested in factors that may contribute to individual differences in this ability. Little is known about the cognitive correlates of proportional understanding in young children before they have been introduced to formal fractions. Given that proportional thinking requires the consideration of the *relation* between two quantities, it may be that a domain-general ability to abstract relational information is a necessary component of engaging in proportional reasoning. In fact, a proportion is a form of analogy by definition, requiring an ability to abstract the relation between two quantities (e.g., the relation 20:25 is the same as the relation 4:5), much like analogical reasoning requires an ability to abstract the relation between two entities (e.g., cat:meow as dog:?). Furthermore, analogical reasoning emerges very early in development (e.g., Ferry, Hespos, & Gentner, 2015) and has been found to be a critical component of learning across many domains (English, 2004; Richland & Simms, 2015). We hypothesized that general analogical reasoning (outside the domain of proportions) may be a critical predictor of proportional reasoning in particular. Thus, in the current study, we explored whether children's general analogical reasoning abilities were related to children's ability to reason proportionally across contexts.

In the current study, we investigated 3- to 6-year-old children's proportional reasoning using a spinner paradigm modeled after Jeong et al. (2007). The focus of our study was to explore factors that contribute to and/or diminish the use of whole number strategies when engaging in a proportional task. Our study addressed four open questions: (a) Does drawing children's attention to both parts of a proportional display through direct instruction promote proportional thinking?; (b) Does experience attending to proportional information diminish the engagement of numerical strategies in a proportional task?; (c) Are younger children (3–4 year olds), who have limited numerical abilities and thus may find numerical information less salient, more likely to succeed in a discrete proportional task?; and (d) Are individual differences in proportional reasoning correlated with general analogical reasoning skills?

[1] Throughout this article, we will use the term "number" or "numerical" to specifically refer to whole number quantities. Although fractions and proportional amount are also defined as numerical quantities, in this context, we refer to these quantities as "proportion" or "proportional," and whole number quantities as being "number" or "numerical."

Method

Three- to 6-year-old children participated in three distinct tasks—the spinner comparison task, a single spinner task (requiring children to judge the outcome of a spin on a given spinner and indicate their confidence in that outcome), and an analogical reasoning task. During the spinner comparison task, half of the participants received direct instruction, highlighting attention to the relative size of the red and blue portions of the spinner, whereas the other half of participants did not. Moreover, the order of presentation of the continuous and discrete blocks of trials was counterbalanced across participants, allowing an exploration of the effect of order on performance.

Participants

Two groups of children: younger children (3- to 4-year-olds; $n = 91$; $M = 4.2$ years; range = 3.2 to 4.9 years; 45 females) and older children (5- to 6-year-olds; $n = 89$; $M = 5.9$ years; range = 5.1 to 6.8 years; 42 females) were included in the study. An additional seven children participated but were excluded because of experimenter or computer error ($n = 6$) or an inability to differentiate the colors used in the task ($n = 1$).

Children were recruited from the greater Boston, Massachusetts, area and were tested at local museums (Museum of Science, Boston, and Boston Children's Museum), day cares, preschools, and after-school programs, as well as in the laboratory. In accordance with the guidelines of each testing facility, children received a sticker and/or small prize for participating. Demographic information was not systematically collected for children tested outside of the laboratory. For children whose parents reported demographic information within the laboratory, the sample was predominantly White and educated, approximately as follows: 72% White, 7% Asian, 2% Native Hawaiian/Pacific Islander, 2% Black or African American, and 17% mixed race. In addition, about 15% reported being Hispanic. Of those who reported their education, all mothers had at least a bachelor's degree and 68% had a master's degree or higher. Although demographic information was not systematically collected, the demographics of children participating outside of the laboratory were expected to be comparable with those collected in our laboratory.

In addition, a sample of 7- to 8-year-old children ($n = 88$; $M = 7.9$ years; range = 7.1 to 8.9 years; 51 females) was tested, but these children performed at ceiling on the tasks (75% of children responded correctly on at least 14/16 trials) and so are not included in the analyses (see the Appendix for more information about these children).

Design

The Boston College Institutional Review Board approved all study procedures (protocol 10.064, Development of Quantity Concepts), and all parents provided informed consent for their child's participation. Children over the age of 7 also provided written assent.

This study involved a 2 (training condition) × 2 (block order) between-subjects design, such that children were randomly assigned to one of four between-subjects conditions: training and continuous first ($n = 26$ younger; $n = 23$ older); training and discrete first ($n = 20$ younger; $n = 21$ older); no training and continuous first ($n = 22$ younger; $n = 23$ older); and no training and discrete first ($n = 23$ younger; $n = 22$ older). These sample sizes are in line with other studies using the same or similar paradigm and investigating similar effects (Boyer & Levine, 2015; Jeong et al., 2007). Based on this prior work, we expected medium to large effects of numerical interference (manipulated within subject) and of block order (manipulated between subject). Thus, these sample sizes within each simple comparison of interest are sufficient to detect medium effects with approximately 70% to 90% power. We were unsure of the expected effect sizes of the training and the relation between proportion and analogy.

All task components except the spinner orientation were presented on a Mac laptop with a 13-in. screen using Xojo programming software. The experimenter recorded all responses using the laptop by pressing the corresponding keys on the keyboard. Each child received two blocks of spinner trials. Each block consisted of (a) spinner orientation, (b) training (if applicable), and (c) spinner comparison. Following the completion of both blocks (one discrete and one continuous), children then participated in two additional tasks. Thus, children participated in the following tasks in a single experimental session lasting approximately 15 min: (a) a spinner comparison task: Block 1 (consisting of: spinner orientation, training [if applicable], spinner comparisons) and Block 2 (consisting of: spinner orientation, training [if applicable], spinner comparisons); (b) a single spinner task; and (c) a pattern analogy task.

Measures

Spinner comparison task.

Orientation. Prior to each set of comparison trials, children were oriented to an actual cardboard spinner (radius = 7.8 cm), half of which was red and half of which was blue. The discrete spinner was broken into quarters, with the diagonally opposite quarters (i.e., nonconsecutive) being the same color. Each circle had a black arrow (6.8 cm long) that could be spun around the center of the circle. Children were initially given three stickers and were told that the experimenter would spin the arrow around the spinner, and if the spinner landed on red, the child would win another sticker, and if the spinner landed on blue, then the experimenter would take one of the child's stickers away. Then, the experimenter spun the spinner twice, each time behaving in accordance with where the spinner landed (i.e., either giving the child another sticker or taking one away).

Training. Following the orientation, children in the training group participated in a brief instructional phase during which they were shown a single donut shaped spinner on the computer and were told that they were going to "figure out how to decide what is a good spinner and what is a bad spinner." Children were then asked how much red there was on the spinner, how much blue was on the spinner, and whether that meant it was a "good" spinner or a "bad" spinner. Children were encouraged to use the words "a little" (meaning, less than 50%) or "a lot" (meaning, more than 50%) when describing the amount of each color. If children gave specific number words (e.g., "two pieces"), they were asked whether it was a little or a lot in order to promote continuous, relational information as opposed to numerical information. If children used the words "a little" or "a lot" differently than intended, or were incorrect about the spinner being "good" or "bad" (meaning, more or less than 50% chance of "winning," respectively), they were corrected. Children were then provided with an explanation as to why it was a good spinner or bad spinner (e.g., "It's a good spinner, because there is a lot of red and only a little bit of blue!").

Children in the training group went through two training trials before each block: one with a good spinner (more red than blue) and one with a bad spinner (more blue than red). Children in the no-training group did not have any additional training and instead proceeded straight to the comparison trials.

Comparison trials. During the comparison trials, children were shown two spinners on the computer screen. The spinners were presented as the same donut shape as the actual spinners used during orientation, but did not include an arrow and were not spun or acted upon. Instead, children were asked to indicate which spinner would help them win more stickers (i.e., the spinner with the greater likelihood of landing on red). Children were prompted to point to the spinner (either on the left or on the right), and the experimenter recorded the child's responses by pressing corresponding keys on the keyboard. The spinners remained visible until the experimenter recorded the child's response.

The discrete spinners and continuous-matched spinners were presented in two separate blocks of eight trials each (see Figures 1A and B1 for examples of discrete and continuous spinners). Between the discrete and continuous blocks (order counterbalanced across children), children were also told, "This time, the spinners look a little different, but they work the same way: if the spinner lands on red you win a sticker and if it lands on blue I take a sticker away."

The same proportion values were used for both the discrete and continuous blocks. So, for example, if a discrete trial involved a comparison of 2/5 versus 5/9, then a continuous trial involved the same comparison magnitudes. Within the discrete block, half the trials were numerically consistent (meaning that the spinner with *proportionally more* red also had *numerically more* red pieces; e.g., 2/5 vs. 5/9) and half of the trials were numerically misleading (meaning that the spinner with *proportionally more* red pieces had *numerically fewer* red pieces, such that comparing the number of red pieces and comparing the proportion of red across spinners would provide different answers; e.g., 4/9 vs. 2/3). Thus, we manipulated the consistency of whole number and proportional information during the discrete spinner blocks. The continuous blocks did not provide whole number information, but trials in the continuous blocks were matched to the specific proportions provided in the discrete block. Thus, continuous trials were divided into continuous consistent-matched trials and continuous misleading-matched trials based upon the specific proportions presented.

The numerically consistent comparisons presented were: 2/6 versus 5/8; 5/7 versus 8/9; 4/9 versus 1/5; and 3/6 versus 5/8. The numerically misleading comparisons presented were: 2/3 versus 3/9; 1/3 versus 2/9; 5/10 versus 4/5; and 3/5 versus 4/9. The ratios between the two proportions presented across trials ranged from 1.25 to 2.2, with an average ratio of 1.65 for the magnitudes used in consistent trials and 1.61 for magnitudes used in the misleading trials. In order to prevent surface area from being a relevant cue for discrimination, we used three different sizes of spinners: small (diameter = 6.1 cm), medium (diameter = 8.8 cm), and large (diameter = 11.4 cm). Within each block of eight trials, two trials involved a small–large comparison, two trials involved a medium–

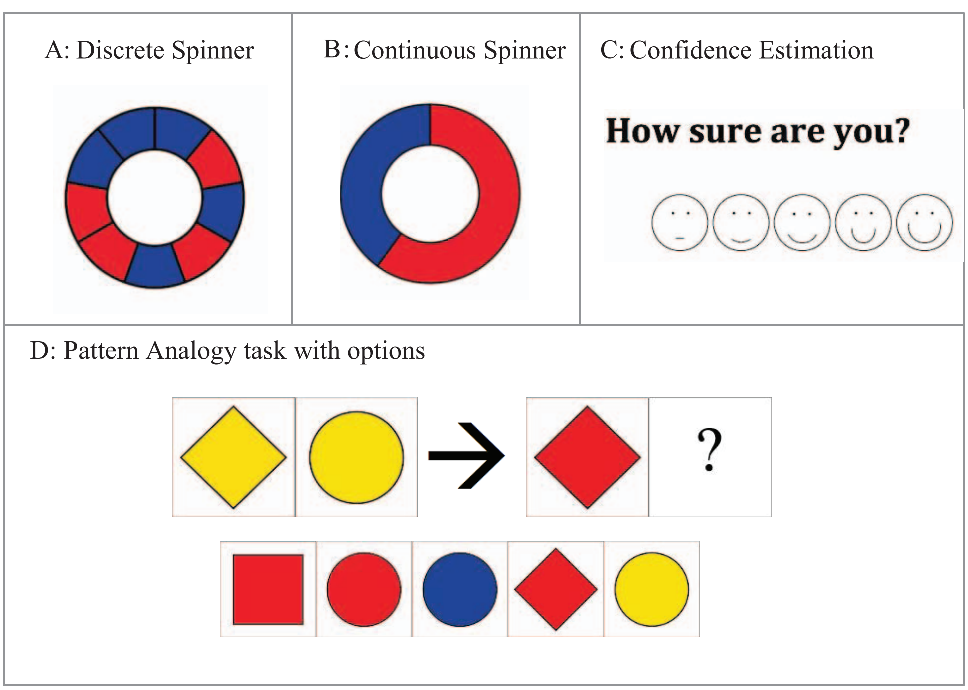

Figure 1. Example stimuli from each task: An example of a discrete spinner (Panel A) and a continuous spinner (Panel B) like those used in the comparison task and in the single spinner task; the scale children used to respond about their confidence level of the single spinner estimation trials (Panel C; note that the experimenter verbally read the question and explained the response scale); and an analogy problem with the options children were given to choose from along the bottom (Panel D). See the online article for the color version of this figure.

large comparison, and four trials involved a small–medium comparison. On half the trials, the larger spinner was the correct answer, and on the other half, the smaller spinner depicted the greater proportion of red, making size an unreliable cue for responding. The order of the trials within each block was randomized across participants.

Single spinner estimation. Following both spinner comparison blocks, children were presented a single donut-shaped spinner and asked to decide whether they thought the spinner would land on blue or land on red when spun (the spinners were never actually spun). Following each decision, children were asked how sure they were that the spinner would land on that color by selecting one of five faces representing "not sure at all/just guessing" to being "really, really sure" (see Figure 1C).

There were six single spinner trials. Three spinners were discrete and three were continuous. Two of the spinners were small, three of the spinners were medium-sized, and one was large. The order of the trials was randomized across participants. The specific magnitudes presented were 8/9, 2/6, 3/5 (discrete), and 1/5, 4/9, 2/3 (continuous).

Pattern analogy task. Lastly, children participated in the pattern analogy task (modeled after Goswami, 1989). The task consisted of two practice trials followed by six test trials, all of the form "A:B → C:?." Children were presented with three pictures at the top of the computer screen, two on the left (A:B; e.g., yellow diamond: yellow circle) and one following a black arrow and followed by a question mark (→C:?; e.g., red diamond:?). At the bottom of the screen were five picture options, which differed from the given analogy in specific ways: (a) the same as B (e.g., yellow circle), (b) the correct shape but incorrect color (e.g., blue circle), (c) the correct color but incorrect shape (e.g., red square), (d) the same as C (e.g., red diamond), and (e) the correct answer (e.g., red circle; see Figure 1D for an example). During the practice trials, the experimenter covered up the options with a piece of paper and helped the child through the trial by asking the child what was the same and what was different in the A:B pair (e.g., yellow circle, yellow square: both are yellow, but one is a circle and one is a square). Then, children were instructed to pick the picture that went in the spot with the question mark in order to make the two sides match. After going through the pattern, the experimenter revealed the options and asked the child to choose. After the child selected, they were given feedback and an explanation (e.g., "The red square goes with the red circle, so that they're both red but one is a circle and one is a square just like the other pictures"). Following the two practice trials, children were presented six test trials in which they did not receive any feedback.

The order of the five options was randomized across trials, but was the same for each child. The order of the practice trials was identical across children, but the order of the test trials was randomized. All shapes were inside a small white box (27.0 cm^2) and approximately the same size.

Data Scoring

Accuracy was the primary dependent variable on all tasks. All children included in the analyses completed both spinner comparison blocks (discrete and continuous). One child in the younger group did not complete the single spinner task because of time constraints, and that child's data were excluded from those analyses. Twenty-seven younger children and nine older children did not participate in the pattern analogy task because of time constraints, loss of attention during the task, or lost data because of computer error ($n = 1$). Thus, a total of 142 children ($n = 62$ younger; $n = 80$ older) had complete data across all tasks. Notably, because these data are likely not missing at random (children with lower attention spans probably had a higher likelihood of not completing the pattern analogy task), regression analyses involving this task should be interpreted with this limitation in mind.

Results

Spinner Comparison Task

First, children in both age groups performed significantly above chance in the continuous block of the spinner comparison task (younger: $M = 59\%$, $SE = 2.6$, $t[89] = 3.4$, $p < .005$; older: $M = 80\%$, $SE = 2.2$, $t[88] = 13.4$, $p < .001$), revealing that on average children were able to engage in proportional reasoning. However, as a group, the younger children were only slightly above chance and a closer look at these children suggests that many of them did not perform above chance. Differences between these children are discussed after the main analyses.

Accuracy scores on the spinner comparison task were subjected to an ANOVA with Block Type (2: discrete, continuous-matched) and Trial Type (2: misleading, consistent) as repeated measures, and Age Group (2: younger, older), Training (2: yes, no), and Order (2: continuous first, discrete first) as between-subjects factors. Analyses revealed a small main effect of training, $F(1, 172) = 5.1$, $p = .026$, $\eta_p^2 = 0.03$, with those who received training ($M = 72\%$, $SE = 2$) outperforming those who did not ($M = 65\%$, $SE = 2$). However, training did not significantly interact with any other variables ($ps > 0.05$). In line with previous studies, analyses revealed a numerical interference effect, as indicated by a significant Block Type × Trial Type interaction, $F(1, 172) = 13.03$, $p < .001$, $\eta_p^2 = 0.07$, revealing that children relied upon numerical information when it was available, performing better in discrete trials (compared with magnitude matched continuous trials) when numerical information was consistent ($M_{\text{Continuous}} = 66\%$, $SE = 2.1$; $M_{\text{Discrete}} = 71\%$, $SE = 2.4$; $p = .04$) but performing worse in discrete trials (relative to magnitude matched continuous trials) when numerical information was misleading ($M_{\text{Continuous}} = 72\%$, $SE = 2.1$; $M_{\text{Discrete}} = 65\%$, $SE = 2.1$; $p = .01$).[2]

Furthermore, these factors (Block Type × Trial Type) significantly interacted with age group (three way interaction), $F(1, 172) = 11.9$, $p < .001$, $\eta_p^2 = 0.07$, and Age Group × Order (four way interaction), $F(1, 172) = 4.5$, $p = .034$, $\eta_p^2 = 0.03$. Given that the interference effect (seen in the Block Type × Trial Type

[2] It is worth noting that although it appears as though performance on the continuous "misleading" trials is higher than continuous "consistent" trials, this difference does not have a meaningful interpretation within the context of our manipulation (because the "misleading" and "consistent" manipulation is nonexistent in the continuous trials). Rather, any differences here may reflect unintended differences between the specific stimuli and ratio comparisons used for these trials. However, we matched the specific magnitudes across the continuous and discrete trials specifically to control for this possibility of ratio differences. Moreover, the direction of this effect is the opposite of the numerical interference effect. Thus, this unintended difference in stimuli is not the cause of the numerical interference effect and, if anything, may be muting the numerical interference effect reported here.

interaction) differed across both age and block order, we looked at the impact of order, block type, and trial type in the younger and older groups separately using two 2 × 2 × 2 ANOVAs.

Analyses of data from the older children revealed both a Block Type × Trial Type interaction, $F(1, 87) = 25.1$, $p < .001$, $\eta_p^2 = 0.2$, and an Order × Block Type × Trial Type interaction, $F(1, 87) = 7.3$, $p = .008$, $\eta_p^2 = 0.08$. These interactions indicated that performance in the older group was significantly impacted by whole number information on the discrete trials, but this interference differed depending on which block of trials children received first. Specifically, older children who received the discrete block prior to the continuous block demonstrated the expected numerical interference effect (Block Type × Trial Type interaction, $F[1, 42] = 22.97$, $p < .001$, $\eta_p^2 = 0.4$; see Figure 2, left panel). That is, performance was higher on discrete trials (compared with the equivalent continuous trials) when whole number information was consistent ($M_{\text{Discrete}} = 87\%$, $SE = 3.5$ vs. $M_{\text{Continuous}} = 80\%$, $SE = 3.2$), $t(42) = -2.23$, $p = .03$, Cohen's $d = 0.34$, and lower (compared with the equivalent continuous trials) when whole number information was misleading ($M_{\text{Discrete}} = 62\%$, $SE = 4.9$ vs. $M_{\text{Continuous}} = 89\%$, $SE = 2.8$), $t(42) = 4.48$, $p < .001$, Cohen's $d = 0.68$. On the other hand, data from older children who received the continuous block first only showed a marginal Block Type × Trial Type interaction, $F(1, 45) = 3.6$, $p = .06$, $\eta_p^2 = 0.08$ (see Figure 2, right panel), with no statistically significant difference in performance on discrete and continuous-matched trials when numerical information was misleading ($M_{\text{Discrete}} = 76\%$, $SE = 4.46$; $M_{\text{Continuous}} = 80\%$, $SE = 4.0$), $t(45) = 0.94$, $p = .35$, Cohen's $d = 0.14$, or when whole number information was consistent ($M_{\text{Discrete}} = 78\%$, $SE = 4.0$; $M_{\text{Continuous}} = 72\%$, $SE = 4.2$), $t(45) = 1.41$, $p = .17$, Cohen's $d = 0.2$.

Analyses of data from the younger group, on the other hand, revealed no main effects or interactions ($ps > 0.1$; see Figure 3). That is, data from the younger group, in contrast to their older peers, did not reveal evidence of a statistically significant numerical interference effect.

However, this younger group also performed much worse than the older group. Although, on average, they performed significantly above chance ($M = 59\%$ on continuous trials), a closer look at these children reveals stark individual differences with many children performing below 50% correct. In order to investigate what might be happening with these younger children, we restricted additional analyses to data from only those children who scored above chance on the continuous trials (>50% correct), who demonstrated some understanding of the task demands and of continuous proportional information ($n = 49$; $M_{\text{age}} = 4.2$ years, range 3.3–4.9 years; $M_{\text{Continuous}} = 77\%$, range = 63% to 100% correct). Using a Block Type (2: discrete, continuous) × Trial Type (2: consistent, misleading) repeated measures ANVOA, with Order (2: continuous first, discrete first) as a between-subject comparison on only this subset of data, there was a main effect of block type, $F(1, 47) = 7.45$, $p = .009$, $\eta_p^2 = 0.137$, with lower performance on the discrete trials ($M = 70\%$) than the continuous trials ($M = 77\%$). However, there were no other significant main effects or interactions ($ps > 0.1$), revealing that these younger children performed worse on *both* numerically misleading trials and numerically consistent trials relative to performance on continuous-matched trials (misleading: $M_{\text{Continuous}} = 77\%$, $SE = 2.6$, and $M_{\text{Discrete}} = 66\%$, $SE = 3.8$; consistent: $M_{\text{Continuous}} = 78\%$, $SE = 2.5$, and $M_{\text{Discrete}} = 74\%$, $SE = 3.9$). Thus, data from younger children who performed above chance on the continuous trials still did not provide evidence of a numerical interference effect, but rather performed worse on discrete trials overall.

Single Spinner Trials

In addition to children's ability to compare probabilities (in the comparison task), we were interested in investigating children's abilities to predict the outcome of single probability events and whether the uncertainty of these probabilities was reflected in children's confidence judgments about the outcome. We did this by looking at performance on the single spinner trials in which children were asked to judge which color they thought the spinner would land on (i.e., what color had the highest probability). On average, both younger ($M = 70.1\%$, $SE = 2.4$) and older ($M = 89.8\%$, $SE = 1.6$) children performed above chance (50%; younger, $t[89] = 8.29$, $p < .001$; older, $t[88] = 25.2$, $p < .001$), but the older group outperformed the younger group, $t(152.98) = 6.83$, $p < .001$.[3] Given the poor performance of the younger group on the comparison task, it is worth noting that even those younger children who performed at or below chance on the continuous trials of the comparison task ($n = 41$) also performed significantly above chance on the single spinner trials ($M = 67\%$, $SE = 3.7$), $t(40) = 4.5$, $p < .001$.

Children's confidence judgments were analyzed by computing a slope between confidence judgments and how far the ratio of the spinner was from 50% (i.e., the highest level of uncertainty). A positive slope would indicate that the child adjusted their confidence judgments based on the degree of uncertainty in the spinner. That is, the further the probability was from 0.5, the higher confidence the child reported. Thirty-six children are excluded from these analyses: one younger child for experimenter error as well as 15 younger and 20 older children for responding with the same confidence rating on every trial (generally highest confidence). On average, data from younger children did not reveal statistically significant positive slopes, either on all trials ($M_{\text{slope}} = 0.09$, $SE = 0.1$), $t(73) = 0.85$, $p = .4$, Cohen's $d = 0.1$, or on correct trials only ($M_{\text{slope}} = 0.08$, $SE = 0.18$), $t(71) = 0.65$, $p = .65$, Cohen's $d = 0.05$. Older children, however, did produce confidence judgments with significantly positive slopes on both correct trials ($M_{\text{slope}} = 0.59$, $SE = 0.13$), $t(68) = 4.6$, $p < .001$, Cohen's $d = 0.56$, and across all trials ($M_{\text{slope}} = 0.60$, $SE = 0.12$), $t(68) = 4.9$, $p < .001$, Cohen's $d = 0.59$.

Predicting Analogical Reasoning

Our primary interest was whether children's performance on proportion-based tasks was related to their more general ability to reason about patterns analogically and whether this depended on the context of the proportional reasoning. Thus, we computed partial correlations between pattern analogy scores (calculated as proportion correct; although see the Appendix for an alternate scoring system that takes into account the type of errors children made) and comparison scores on discrete trials, comparison scores

[3] Note that although both continuous and discrete trials were included in this task, they were not perfectly matched on magnitude, making it impossible to make direct comparisons of performance on these two trial types.

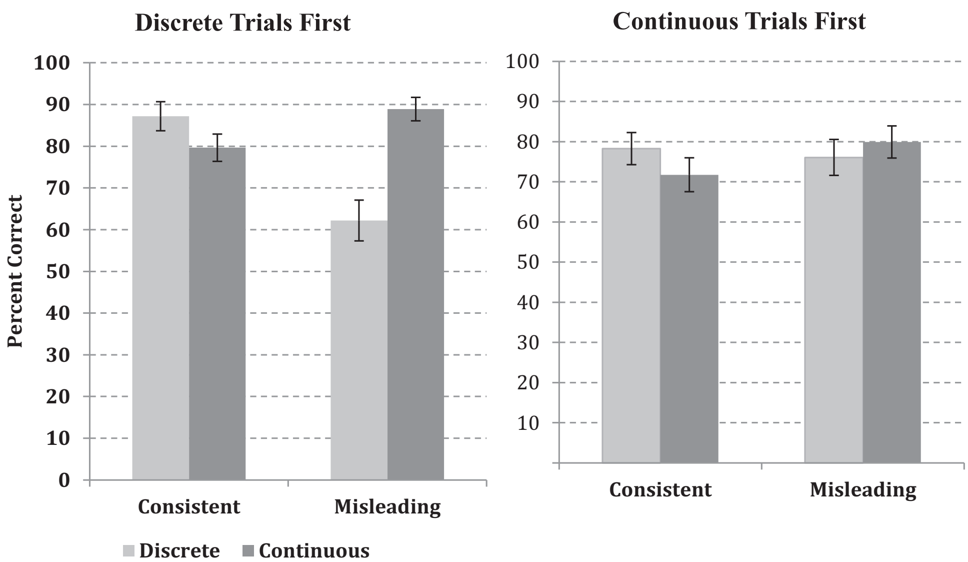

Figure 2. Older children's performance on the spinner comparison task separated into children who received the discrete block first (left) and children who received the continuous block first (right).

on continuous trials, and a measure of the interference effect, controlling for age[4] and the other measures (see Table 1 for means and standard deviations of each measure). Our interference measure was calculated as *performance on numerically consistent discrete trials – performance on numerically misleading discrete trials*. If children consistently relied upon numerical information, then this value should be close to 1 (near ceiling on consistent trials and near floor on misleading trials), whereas if children did not rely upon numerical information when making their judgments, this value should be near zero (about equal performance, regardless of the nature of the discrete information).

Accuracy on the pattern analogy task (when controlling for age and the other measures) was significantly correlated with performance on continuous proportion trials ($r = .2$, $p = .0190$) but not with performance on discrete proportion trials ($r = .006$, $p = .79$) or numerical interference scores ($r = -0.08$, $p = .33$).

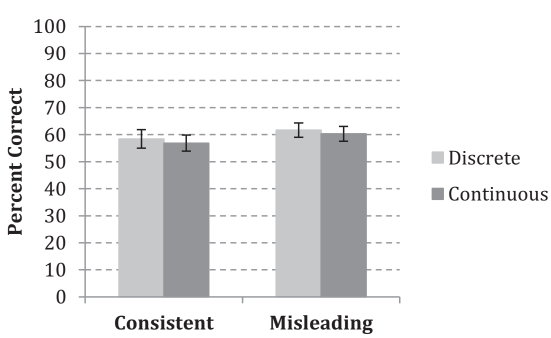

Figure 3. Younger children's performance on the spinner comparison task, across all children regardless of the order in which they received the blocks.

Discussion

The current study investigated 3- to 6-year-old children's attention to relational versus whole number information in discrete probabilistic contexts. In particular, we aimed to investigate whether specific context effects and individual differences may impact children's strategy selection when proportional information is presented discretely as opposed to continuously. In doing so, the current data reveal substantial insights into the malleability of children's attention to whole number information when making proportional judgments and how individual differences in proportional reasoning may be related to more domain general skills.

The current study was the first to investigate numerical interference using this paradigm with 3- and 4-year-olds. Although as a group they performed slightly (and significantly) above chance on the comparison task, many children (approximately 50%) performed below chance on the continuous trials, which did not involve an opportunity for numerical interference. On the other hand, even those younger children who performed at or below chance on the comparison task were successful on the single spinner trials (judging whether they thought it would land on blue or red). Thus, it may be that deciding the probability of a single outcome is an easier task than comparing two outcomes (when the total number of pieces differs). Given that correctly judging a single spinner requires assessing the relationship between only two amounts (red vs. blue), whereas comparing two spinners requires comparing the relations among four different amounts (red vs. blue in each spinner, then comparing across spinners), it may not be surprising that children performed better on the single spinner task. Thus, our findings suggest that 3- to 4-year-old children are able to

[4] Age was treated categorically for consistency across analyses. However, an identical pattern of results is found when age is treated continuously.

Table 1
Descriptive Statistics for Correlational Analyses

Subgroup of children based on age	n	Mean proportion correct (SE)			
		Pattern analogy	Discrete trials	Continuous trials	Numerical interference
Younger children	62	.22 (.03)	.64 (.03)	.59 (.03)	−.024 (.04)
Older children	80	.58 (.03)	.75 (.02)	.80 (.02)	.13 (.05)

Note. SE = standard error.

use proportional information to make inferences about outcomes (i.e., that the probability of an outcome is determined by the proportional relationship between the pieces), but excessive cognitive demands (e.g., working memory, strategy selection, comparing multiple outcomes) of the comparison task set up may limit their ability to demonstrate proportional understanding.

Interestingly, even younger children who performed above chance on the comparison task did not produce data revealing a statistically significant numerical interference effect. Rather, these younger children generally performed worse on the discrete trials overall than the continuous trials, regardless of whether or not numerical information conflicted or aligned with proportional information. Given that the discrete trials involved nonconsecutive red pieces, using a proportional strategy on these trials may have required more mental spatial manipulation than the continuous trials, potentially leading to their lower performance. Although we cannot claim that the younger group did not use whole number information, the findings that children performed worse overall on discrete trials than continuous trials, but not in a way consistent with whole number information, hints that maybe young children were less likely to use numerical strategies. In other words, given the young age of these children, they may not have seen the discrete trials as being a numerical task at all. This may be because these children likely had lower numerical abilities (e.g., might still be in the process of learning to count) and/or because numerical information is simply less salient to them (consistent with Sophian et al., 1995). Because we did not measure the numerical abilities of the children in the current study, our data cannot speak to this explicitly. However, given that between the ages of 3 and 6 years, children are gaining a substantial amount of formal whole number knowledge (e.g., Hurst, Anderson, & Cordes, 2017; Le Corre & Carey, 2007; Wagner & Johnson, 2011) as well as proportional reasoning ability (e.g., Girotto et al., 2016; Sophian, Garyantes, & Chang, 1997), this may be a key period for investigating the developmental progression of these two concepts and how they may rely (or not) upon each other.

In contrast to the younger group, 5- to 6-year-olds performed quite well on both the comparison task and the single spinner task. Replicating previous findings (e.g., Boyer et al., 2008; Jeong et al., 2007), when whole number information was made available (i.e., the comparisons were discrete with different denominators), these older children relied on this information—performing better when whole number information was helpful and performing worse when whole number information was misleading.

This numerical interference effect, however, was diminished when children were first exposed to continuous trials prior to the discrete block. That is, children primed to use a proportional strategy (in the continuous block) were less likely to switch to a counting strategy, even when numerical information became available. It is notable that this transfer occurred despite quite an obvious stopping point between blocks that was highlighted by the experimenter (e.g., "now the spinners look different"). This finding is consistent with studies investigating formal fraction instruction, which suggest that introducing discrete fraction content as an extension of more continuous instruction with decimals and percentages (e.g., Moss & Case, 1999), rather than the other way around, may promote rational number understanding. By introducing content in this order, children may be more likely to extend the continuous proportional strategies into these new discrete contexts and rely less upon discrete numerical information overall. The benefits of introducing proportional information in the context of continuous representations prior to discrete representations are one potential avenue for future research.

Recently, Boyer and Levine (2015) reported a similar finding using a slightly different paradigm (a match-to-sample paradigm, in which a child is asked to pick which of two options is the proportional match to a target while ignoring the numerical match). Boyer and Levine reported that fourth graders who were given prior experience with continuous trials were less likely to use a numerical strategy on the subsequent discrete trials. Extending these findings, our data reveal this pattern as early as 5 to 6 years of age (corresponding to approximately kindergarten), suggesting that even at this young age, children's strategies for engaging in proportional tasks may be fairly malleable. It should be noted, however, that although Boyer and Levine tested younger children, they did not find the prior-experience effect in second graders or kindergartners. This contrast may be due to several minor differences across paradigms. For example, match-to-sample tasks (as those used in Boyer and Levine) may be more cognitively taxing than comparison tasks, as they require making comparisons across a greater number of stimuli (three stimuli vs. two stimuli). In fact, the kindergarteners and second graders studied by Boyer and Levine were less accurate overall in their task than the 5- to 6-year-olds were in the current study (the average score for kindergarteners in Boyer and Levine was between 50% and 65% correct, whereas the 5- to 6-year-olds in the current study had average scores between 62% and 89% correct). Thus, it may be that 5- to 6-year-old children are able to inhibit their reliance on numerical strategies, but only in contexts that are otherwise relatively easy for them. That is, when the task is challenging, children may be more likely to resort to relying on whole number information and less likely to use proportional strategies. However, it remains unclear why children would rely on numerical information in challenging contexts, given evidence that proportional reasoning may be a particularly early developing tool for encoding information (Duffy, Huttenlocher, & Levine, 2005; Huttenlocher, Duffy, & Levine, 2002). It may be that for children this age, numerical strategies are more salient or easier to match exactly than proportion (because counting and comparing the two salient sets is seemingly less taxing than comparing the relative size of four quantities). In contrast, in relatively simple tasks, children's reliance upon numerical information may be more malleable, allowing them to switch back to the more approximate proportional strategy when given the right experience.

Although experience with continuous trials encouraged children to rely on a proportional, rather than whole number, strategy, we did not find an effect of our direct instructional training. Overall, children in the training group did outperform their peers who did not receive training, suggesting that highlighting the relevant parts of the displays may have increased children's attention to the task at a more global level. However, the training did not have a specific effect on strategy selection (i.e., did not result in a reduction of numerical interference). It may be that our instructional training was simply not strong enough to help children overcome their tendency to focus on numerical information or that there may be a more effective way to highlight relevant information. For example, we aimed to highlight the parts (i.e., number of red and number of blue), but it may be that children would benefit more from highlighting attention to the total number of pieces (i.e., the denominator) in addition to the number of red pieces (i.e., the numerator).

In addition to generally succeeding on the proportion tasks, there was a significant, positive relationship between older children's judgments of how confident they were of a particular outcome (e.g., that the spinner would land on blue) and the actual probability of the outcome. That is, children reported being more confident in their response when the likelihood of the outcome was actually much higher than the likelihood of the other outcome. Thus, by 5 to 6 years old, children already show relatively sophisticated probabilistic reasoning in both estimation-type (what color do you think it will land on) and comparison (which spinner is more likely to land on red) contexts in which they are unable to rely solely on numerical information. Younger children, on the other hand, did not show this same pattern with their confidence judgments. It may be that 3- to 4-year-olds simply had trouble understanding the question or response scale. Research looking at preschoolers' abilities to provide confidence judgments in other domains suggest that this skill is still developing between 3 and 4 years old (Ghetti, Hembacher, & Coughlin, 2013; Hembacher & Ghetti, 2014), so it may be that our younger participants were not able to articulate their uncertainty in this context. Alternatively, our data may reflect a limit to 3- to 4-year-olds' understanding of probability even in these single estimation contexts. Additional studies should investigate how early understanding of probability in this preschool age range may depend on specific aspects of the task demands. In particular, given the varying cognitive skills (e.g., working memory) and specific knowledge (e.g., whole number knowledge, estimation skills) required across distinct tasks commonly used in the literature (e.g., single display estimation tasks, comparison tasks, match-to-sample tasks), these tasks are likely not interchangeable. However, performance differences across these tasks may shed important insight into how children's understanding of proportion and ability to reason about the relevant relational information develops.

Lastly, the current study suggests that children's more general ability to reason analogically using geometric patterns is significantly related to their proportional reasoning in continuous contexts but not in discrete contexts. Given that children engaged in numerical strategies on discrete trials, this finding might suggest that relational reasoning is specifically related to proportional reasoning (used on continuous trials) more so than counting or numerical reasoning (presumably the dominant strategy used on discrete trials). However, analogical reasoning (as measured here) was not significantly correlated with the extent to which children relied upon numerical information in particular. Furthermore, as noted in the Method section, given the not at random missing data for this analysis, these findings should be interpreted with caution. Rather, our goal is to suggest some directions for future work to further investigate the potentially unique role of analogical reasoning in proportional thinking over other quantitative and mathematical knowledge. In particular, it is critically important to investigate the causal nature of this relationship (if any) and, in particular, whether analogical training, aimed at facilitating children's analogical reasoning through structural alignment, can foster recognition of the relational nature of proportion contexts (as has been shown in other domains, e.g., Gentner et al., 2016).

In sum, the current study provides substantial insight into the context effects and individual differences in children's use of numerical and proportional information in discrete and continuous probabilistic contexts. Future research should continue investigating the relation between numerical and proportional information in order to further elucidate the factors that may lead to or prevent numerical interference (e.g., the saliency of numerical information in the fraction symbol system, global attention to the relation vs. local attention to features during specific task demands), how the relationship may be leveraged to benefit learning rather than lead to interference (e.g., teaching continuous proportional information before discrete proportional information), and how domain general analogical reasoning skills may be leveraged to help children attend to relational information.

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(*Appendix follows*)

Appendix

Additional Analyses

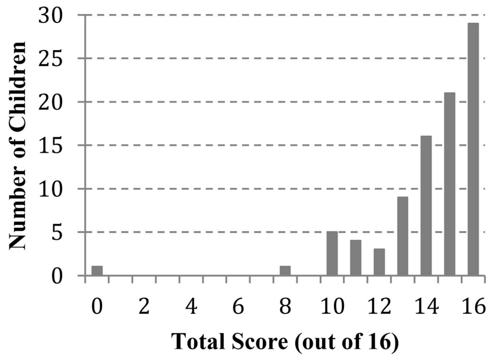

Figure A1. Histogram of comparison task performance.

Additional 7- to 8-Year-Old Sample

An additional sample of 7- to 8-year-old children also participated in this study. This group performed near ceiling levels on the task, leading to substantial skew in the distribution of their responses, violating assumptions of normality and making it difficult to analyze children's responses given the few number of questions per cell (four trials per cell when broken down into discrete vs. continuous and misleading vs. consistent). On average, children performed at least 80% correct on all trial types: consistent trials, $M_{\text{Continuous}} = 88\%$ ($SD = 20\%$), $M_{\text{Discrete}} = 93\%$ ($SD = 15\%$); misleading trials, $M_{\text{Continuous}} = 93\%$ ($SD = 18\%$), $M_{\text{Discrete}} = 80\%$ ($SD = 30\%$). Figure A1 displays a histogram of overall performance (total possible score of 16, across the four trial types). Notably, almost one third of the children (29/88) answered all of the questions correct and more than half (50/88) answered, at most, one incorrect. Below, we provide an analysis of this group's performance using analogue analyses as those used in the article. It is worth noting that one child got all the problems incorrect (0/16). When this child is excluded from the analyses, the overall pattern of results reported below does not change.

Despite this extreme left skew, data from 7- to 8-year-olds also showed evidence of an interference effect. We conducted a 2 (block type) × 2 (trial type) repeated measures ANOVA, with Training (2) and Order (2) as between subject measures. There was a main effect of trial type, $F(1, 84) = 4.22$, $p = .04$, $\eta_p^2 = 0.05$, and a marginal effect of block type, $F(1, 84) = 3.9$, $p = .053$, $\eta_p^2 = 0.045$. However, these effects were qualified by a significant Block Type × Trial Type interaction, $F(1, 84) = 24.27$, $p < .001$, $\eta_p^2 = 0.22$. In particular, as expected, children performed better in the discrete trials compared with the matched continuous trials when numerical information was consistent, $t(87) = 2.65$, $p = .01$, and worse in discrete trials compared with the matched continuous trials when numerical information was misleading, $t(87) = 4.3$, $p < .001$.

There was also a marginal Block Type × Training interaction, $F(1, 84) = 3.9$, $p = .053$, $\eta_p^2 = 0.045$, and a significant Block Type × Order interaction, $F(1, 84) = 12.7$, $p < .001$, $\eta_p^2 = 0.13$. In particular, those in the no-training condition performed similarly on the discrete and continuous blocks ($M_{\text{Discrete}} = 89\%$; $M_{\text{Continuous}} = 89\%$), $t(43) = 0.198$, $p = .8$, whereas those in the training condition performed better on the continuous block (92%) than the discrete block (85%), $t(43) = 2.86$, $p = .007$. When looking at order effects, those who received the discrete block before the continuous performed better on the continuous block (94%) than the discrete block (84%), $t(45) = 3.72$, $p = .001$, and those who received the continuous block before the discrete block did not perform significantly different across the two blocks ($M_{\text{Continuous}} = 87\%$; $M_{\text{Discrete}} = 90\%$), $t(41) = 1.1$, $p = .28$. However, these interactions did not further interact with trial type, and there were no other significant main or interaction effects (ps > 0.1).

These findings are consistent with other work suggesting that this age group shows a numerical interference effect when evaluating discrete proportional displays. In addition, the effects are overall consistent with, but less nuanced than, the pattern shown by the 5- to 6-year-olds in the current article. That is, children who received the discrete trials before the continuous trials performed worse on the discrete trials relative to the continuous trials, whereas those children who received the continuous trials before the discrete trials did not perform differently across the two blocks of trials. This gives some hint that having the continuous trials before the discrete trials may impact these children's strategy choice and, in particular, lead to similar performance across the two blocks. However, these interactions did not significantly interact with trial type, suggesting that the pattern is less specified to particular trial types in this sample (as they were with the 5- to 6-year-olds) and may not be entirely explained by overcoming a

(*Appendix continues*)

numerical bias. Furthermore, the small (and marginal) Training × Block interaction suggests that our training led to a larger difference in performance between continuous and discrete blocks relative to those in the no training condition. This further emphasizes that our training may not have been best suited to improving performance on the discrete trials specifically. It is possible that the less nuanced pattern of effects found for these 7- to 8-year olds (relative to the 5- to 6-year-olds) is because of lower variability in responses at the upper end of performance. However, given the extreme skew in the distribution suggesting ceiling-level performance, both overall and within each trial type, the interpretation of these effects is limited. The analyses are provided here only for transparency and completeness as well as to provide some potential insight into the performance of this oldest age group.

Alternate Scoring of Pattern Analogy

Many of the children, particularly the younger children, performed fairly poorly on the analogy task (e.g., over one third of the children received less than 35% correct) based on using the dichotomous correct–incorrect scoring. However, this measure of accuracy treated all errors as equal, despite the fact that some of the "wrong" options were more related to the pattern than other options. Thus, in order to differentiate children who had some knowledge of how to make an analogy from those who did not, we also scored the analogy task using a slightly different method. In this error-based scoring scheme, the scores were not based on perceptual similarity to the correct answer, but rather whether the perceptual match was one that took into account the first half of the analogy (suggesting some understanding of needing to make a "match" *across* these relations) versus a perceptual match that ignored the first half of the analogy, which is likely a less difficult match (given the perceptual and temporal proximity). However, these distinctions were not tied to specific perceptual features (e.g., color vs. shape matches), but instead based on the location of the perceptual match in the analogy.

In our alternate scoring method, each trial of the pattern analogy task was scored with 0 to 3 possible points, so that children were given partial credit for making perceptual matches, even if they did not make the relational match. Selecting the correct analogical match was worth 3 points, reflecting a complete understanding of the analogical relation (e.g., the red circle in Figure 1D of the main text). Children who selected the same image as "B" (in A:B::C:?), were considered to have made the far-perceptual match (2 points; the yellow circle in Figure 1D). Meaning, they understood that they were required to reference the first half of the pattern (A:B) but selected the perceptual match rather than the relational match.

Table A1
Descriptive Statistics for the Alternate Scoring of Pattern Analogy

Subgroups of children, separated by age as in the main text	Pattern analogy: Mean proportion correct (*SD*)
Younger children ($n = 62$)	.48 (.02)
Older children ($n = 80$)	.74 (.02)

Children who selected the same image as "C" were considered to have made a close-perceptual match (1 point; the red diamond in Figure 1D of the main text), meaning that they understood the need to make a pattern but selected the simple perceptual match on the second half of the analogy only (C:?) without referencing the first half, neither perceptually or relationally. The other two options were not directly related to the pattern, either perceptually or relationally (blue circle or red square in Figure 1D of the main text), and so selection of these options indicated no understanding of completing any clear pattern (0 points). Thus, the total possible score on the pattern analogy task ranged from 0 to 18 (across all six trials) and is reported as a proportion out of 18 (see Table A1 for descriptive statistics of this scoring system). This scoring method incorporated error type in order to illuminate meaningful differences among low-scoring children. Thus, this scoring system creates a continuum from little to no understanding of patterning, to understanding basic aspects of patterning, to understanding relational and analogical patterning in particular.

Using this scoring scheme, pattern analogy score was significantly correlated with performance on the continuous trials ($r = .245$, $p = .004$) but not on discrete proportion trials ($r = .04$, $p = .618$), mirroring findings reported in the main text using the dichotomous scoring scheme. Furthermore, pattern analogy performance was significantly correlated with interference scores ($r = -0.17$, $p = .045$), suggesting that children who were more likely to use number during the discrete trials were also likely to have lower analogical reasoning scores.

Given that this relation is small, and not reproduced using an alternate scoring scheme, it should be interpreted with caution. However, it may point to the idea that children with lower analogical reasoning skills may be more likely to rely on numerical information and not proportional information.

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