



# Implicit learning in 3-year-olds with high and low likelihood of autism shows no evidence of precision weighting differences

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## Abstract

Predictive Processing accounts of autism claim that autistic individuals assign higher precision to their prediction errors than non-autistic individuals, that is, autistic individuals update their predictions more readily when faced with unexpected sensory input. Since setting the level of precision is a fundamental part of perception and learning, we propose that such differences should be detectable in various domains at a very early age, before clinical symptoms have fully emerged. We therefore tested 3-year-old younger siblings of autistic children, with a high likelihood of later receiving an autism diagnosis themselves, and low-likelihood children with an older sibling without autism. We used a novel implicit learning paradigm to examine the effect of sensory noise on the predictions participants built. In order to learn a sequence, our participants had to select which visual information to attend to and disregard low-level prediction errors caused by the sensory noise, which the theory claims is more difficult for autistic individuals. Contrary to the proposed higher precision-weighting of prediction errors in autism, the high-likelihood children did not show signs of updating their predictions more readily when we added sensory noise compared to the low-likelihood children, either in their reaction times or in the recurrence and determinism of their response locations. These results raise challenges for Predictive Processing theories of autism, specifically for the notion that prediction errors are inflexibly highly weighted by individuals with autism.

## KEYWORDS

autism, implicit learning, multi-level modelling, Predictive Processing, recurrence quantification analysis, variability

## 1 | INTRODUCTION

Autism is a common neurodevelopmental condition characterized by social communicative differences and restricted repetitive behaviors (Diagnostic and Statistical Manual of Mental Disorders, 5<sup>th</sup> edition; American Psychiatric Association, 2013), and often co-occurs with sen-

sory processing difficulties (Baranek et al., 2006; Tomchek & Dunn, 2007). Despite many attempts to identify the cognitive mechanisms underlying autism, these have not yet been established. Theoretical accounts of autism are generally directed at either social or perceptual domains, and have not been successful at explaining the condition as a whole. Recently, Predictive Processing accounts of autism have been

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developed which claim to explain social communication differences, restricted repetitive behaviors, and sensory symptoms.

These accounts posit that autistic individuals differ from non-autistic individuals in their processing of all incoming sensory information (Lawson et al., 2014; Pellicano & Burr, 2012; van de Cruys et al., 2014), and that this cascades up the cognitive hierarchy, explaining symptoms in all domains. These theories are based on Predictive Processing models of cognition which claim that all sensory information is actively created as opposed to being passively received. They posit that the brain is constantly predicting its environment and that all sensory input is processed as the difference between the predicted value and the observed value (Clarke, 2013). This difference is called a prediction error. As well as the error signal indicating how the observation was different from the prediction, it carries a precision weighting, which indicates how certain it is that this error is useful information that should lead to a change in predictions. If an error has a high precision weighting, the observation is taken as counter-evidence for the previous prediction: it indicates a change in the environment and the prediction will be updated. If, however, an error has a low precision weighting, the environment is assumed to be unchanged and the prediction is kept as it was. Errors can have low precision weightings when the observation is consistent with random fluctuations in sensory input due to chance, or when they are deemed to represent small details that are not relevant (Kwisthout et al., 2017).

There is some initial evidence that prediction error precision-weighting may be higher in autistic individuals (e.g., Crawley et al., 2020; Manning et al., 2015; Van der Hallen et al., 2017; Zaidel et al., 2015), although other studies find no group differences (e.g., Karaminis et al., 2016; Manning et al., 2017; Manning et al., 2017; Van de Cruys et al., 2018).

Although these theories are initially derived from hypotheses about the brain, there are clear avenues for testing them behaviorally. Van de Cruys and colleagues (2017) expand upon how higher prediction error precision would cause autistic individuals to have more difficulty distinguishing signal from noise than non-autistic individuals. When sensory input is noisy, a high precision on prediction errors would mean updating predictions even when the underlying signal has not changed. Since perception is based only on observations with no access to the true underlying structure (Aggelopoulos, 2015; Clark, 2013), it is left to the observer to learn about their sensory input to set precision and decide which elements are signal and which are noise (Van De Cruys et al., 2014; 2017). This means that there is no fixed optimal level of prediction error precision, rather, it must be flexibly adjusted to fit the circumstances. Task demands and contextual information can, therefore, reveal the behavioral effects of setting high precision of prediction errors.

Manning et al. (2015) used a coherent motion paradigm to do just that. They showed that autistic children performed similarly to non-autistic children when they were asked to complete a standard motion coherence paradigm, that is, to indicate the overall direction of a field of dots in which most of the dots—the signal dots—moved in the same direction, while a small number of dots—the noise dots—moved in random directions. However, when the children were asked to indicate the

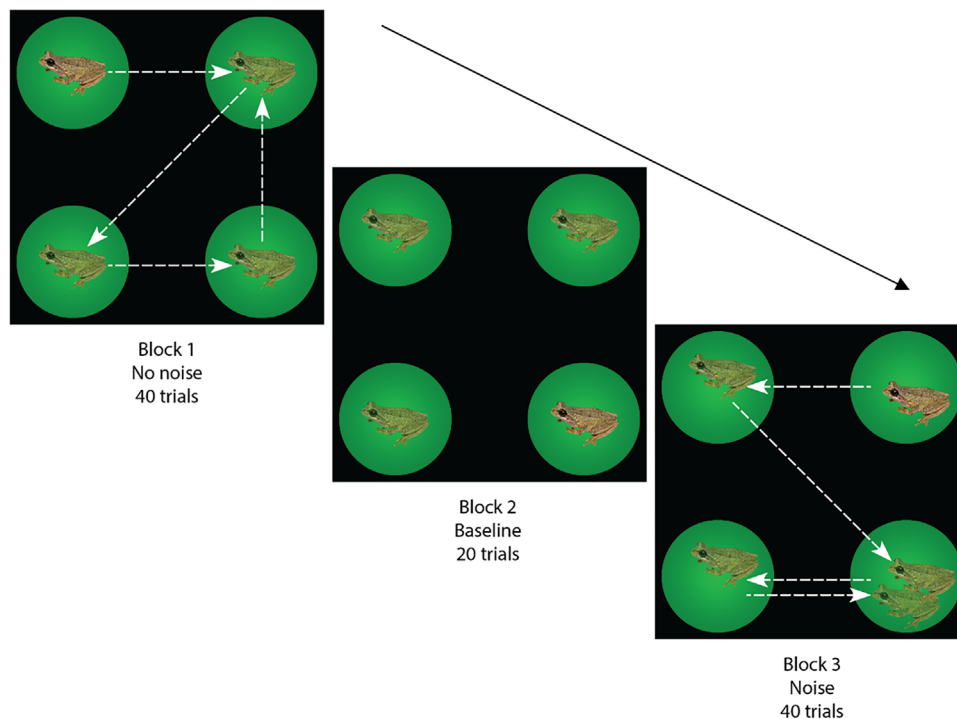
### Research Highlights

- Predictive Processing accounts posit high precision-weighting of prediction errors as a mechanistic explanation of autism
- High precision-weighting of prediction errors would lead to disproportionate disruption in performance on tasks with increased sensory noise
- Three-year-olds with increased likelihood of receiving an autism diagnosis did not differ from low-likelihood peers on an implicit learning paradigm with added noise
- Predictive Processing accounts of autism have found little empirical support in the wider literature so far, and as such these accounts are not currently suited to explain autism mechanistically

average direction of the dots, a task in which every dot is a signal dot and, therefore, high precision of prediction errors would be beneficial, the autistic group outperformed the non-autistic children. This elegant test of the theoretical predictions is some of the first evidence that autistic children may indeed set their prediction error precision higher than non-autistic children, and that this is observable in their behavior.

Since Predictive Processing accounts of autism posit fundamental differences between autistic and non-autistic individuals that should be present from a very early age, we propose that testing these theories can best be done when participants are young. This minimizes the chances that observed behaviors are a mixture of primary autism symptoms and compensatory strategies, and increases the chances of identifying cognitive mechanisms. Longitudinal prospective studies of younger siblings of autistic children are a useful vehicle to study the early development of autism. Autism cannot reliably be diagnosed until around 3 years of age (Charman & Baird, 2002), but around 20% of younger siblings of children with an autism diagnosis will later receive a diagnosis themselves (Ozonoff et al., 2011), and a further 30% will show sub-clinical autism characteristics (the Broader Autism Phenotype; Ozonoff et al., 2014). These siblings can, therefore, be followed longitudinally from a young age to provide a rich dataset describing their development and the emergence of autistic characteristics.

To assess the theoretical prediction about precision weighting of prediction errors in autism in a young sample, we therefore tested two groups of 3-year-olds: younger siblings of children with an autism diagnosis (high-likelihood siblings) and younger siblings of children without autism (low-likelihood siblings). Using an implicit learning paradigm, we asked whether the high-likelihood siblings already show higher precision of their prediction errors than the low-likelihood siblings. There is evidence that children with and without autism do not differ in their implicit learning under standard task conditions (e.g., Brown et al., 2010; Mayo & Eigsti, 2012, although see Gidley Larson & Mostofsky, 2008), suggesting that such a task is well-suited to isolate differences in the groups' responses to our novel manipulation.



**FIGURE 1** Task structure with example stimuli. In blocks 1 and 3, the frog moved from leaf to leaf in a deterministic pattern. In block 2, there was no pattern and the frog appeared 20 times in a pseudorandom series of locations

## 2 | THE CURRENT STUDY

Our novel serial response time task was implemented on a touch-screen and was specifically designed to test the influence of precision of prediction errors. We adapted the traditional serial response time task, in which stimuli are shown in a repeating sequence in order to invoke implicit learning, by adding a condition in which the stimuli contained sensory noise. The participants were not aware that there would be a pattern in the stimulus sequences, and were simply instructed to touch a frog that appeared on one of four lily pads on the screen (see Figure 1).

The task was self-paced, so as soon as a lily pad was pressed the frog appeared in a new location. The first block consisted of the frog moving between the locations on the screen in a fixed, repeating sequence. The second block served as a baseline in which the frog moved between lily pads in a pseudorandom order such that the frog did not appear in the same location consecutively. The third block consisted of a new fixed, repeating sequence, with the addition of randomly-generated jitter to the frog's position on the lily pad. This meant that participants could learn which lily pad the frog would next appear on, but the precise upcoming location of the frog on that lily pad was both unpredictable and unpredictable of any upcoming events, and should therefore have been ignored for optimal sequence learning. This addition of jitter in the added-noise block produces a sequence which is exactly as easy to learn as the sequence without jitter from the no-noise block if participants can identify the jitter as noise and form their expectations based only on which leaf the frog appears on. However, higher preci-

sion of prediction errors would trigger expectation updates about the precise location of the frog on every trial, which would prevent forming of expectations based on the underlying sequence.

If the high-likelihood children weight their prediction errors more than the low-likelihood children, they should update their predictions about the frog's location more often. This would mean that in the jittered sequence in block 3, the high-likelihood children would be more influenced by the frog's exact location than the low-likelihood children, which would make it more difficult to generalize over repetitions to extract the sequence. This would lead to the high-likelihood children responding in more varied locations on each trial and learning the sequence more slowly than low-likelihood children only in the jittered condition, seen in less recurrence in response locations and a slower rate of response time reduction.

## 3 | METHODS

### 3.1 | Participants

In total, fifty-three 3-year-olds took part in the study: 28 high-likelihood and 25 low-likelihood. The two groups did not differ in age ( $t(44.06) = -1.17, p = 0.25$ ), but the high-likelihood group did have a lower developmental level as assessed by the Mullen Scales of Early Learning ( $t(48.20) = -3.06, p = 0.004$ ) and more autism symptoms as assessed by the Autism Diagnostic Observation Scale (ADOS-2; Lord, Luyster, et al., 2012; Lord, Rutter et al., 2012) ( $t(38.88) = 2.42, p = 0.02$ ;

**TABLE 1** Participant characteristics

	N (M:F)	Age in years Mean (SD)	Mullen composite Mean (SD)	ADOS CS <sup>a</sup> Mean (SD)
High-likelihood	28 (16:12)	3.18 (0.15)	98 (20)	3.5 (2.7)
Low-likelihood	25 (15:10)	3.24 (0.19)	112 (14)	2.2 (1.1)
Total	53 (31:22)	3.21 (0.17)	104 (18)	3.0 (2.3)

<sup>a</sup>ADOS Comparison Scores allow for comparison of scores from different ADOS modules. See Gotham and colleagues (2009) for raw score conversion tables.

see Table 1 for descriptive statistics). One low-likelihood participant did not contribute a Mullen composite score due to missing scores for one subscale, and seven low-likelihood participants did not contribute ADOS scores due to limited resources during testing.

We report ADOS comparison scores in order to facilitate interpretation of scores from different ADOS modules (Gotham, Pickles & Lord, 2009). Comparison scores range from 1 to 10, and scores of 1–3 are considered to indicate no suggestions that the child is autistic. Scores of 4 and 5 indicate that the child shows signs which may mean they are on the autism spectrum, while scores of 6–10 indicate that it is likely the child is autistic. Three of the low-likelihood children who had ADOS scores available and three of the high-likelihood children scored in the middle category, indicating some signs they may be on the autism spectrum. Additionally, seven high-likelihood children scored in the highest category indicating that they may well be autistic.

This study was approved by the medical ethics committee (Commissie Mensgebonden Onderzoek Arnhem-Nijmegen, protocol NL42726.091.13). Recruitment was done via the Baby and Child Research Center participant database and through Karakter, a child, and youth psychiatry clinic. Participants received a reimbursement of their travel costs and a small amount of money as a thank you for taking part.

## 3.2 | Materials

### 3.2.1 | Stimuli

Stimuli consisted of an image of a frog taken from Wikimedia images ([https://en.wikipedia.org/wiki/File:Brown\\_Tree\\_Frog\\_2.jpg](https://en.wikipedia.org/wiki/File:Brown_Tree_Frog_2.jpg); CC BY-SA 3.0 license) and four circular leaves created by the authors in Adobe Photoshop, all presented on a black screen (see Figure 1). The frog measured 4.5 cm by 3 cm (201.6 × 136 pixels) and the leaves measured 8 cm (442.2 pixels) in diameter. The leaves were arranged in a square with 6.75 cm between them horizontally and vertically (600 pixels between the centers of the circles). To introduce the game, and in between blocks, photographs of frogs in natural environments were shown. These images were 13.5 by 9 cm.

Sequences were generated at the beginning of the session such that each participant had different sequences for the first and third blocks, and that the sequences contained no immediate repetitions and did not

begin and end on the same leaf, since this would appear as an immediate repetition when the sequence repeated. In the second block which served as a baseline, the frog was presented in pseudorandom locations, with the constraint that there were no immediate repetitions of the same location.

The noise introduced in the third block was implemented by adding jitter to the position of the frog randomly on each trial. The center of the frog was placed at a coordinate drawn randomly from a uniform distribution up to 60 pixels in the x direction and up to 60 pixels in the y direction from the center of the lilypad on which the frog should appear according to the sequence. This resulted in the frog stimulus appearing anywhere on the given lilypad but always completely within the lilypad area. This ensured that the global image formed by the background and lilypad configuration was the same for every trial, and visual information only changed within the lilypads.

### 3.2.2 | Apparatus

Stimuli were presented with Matlab (version 2013a; MathWorks) together with Psychtoolbox (versions 3.0.11; Brainard, 1997; Kleiner et al., 2007) running on a MacBook Pro (OS X version 10.8.5). Stimuli were shown, and participants' touch responses recorded, on an ELO 2244L 22" LCD touchscreen. The location of participants' touch responses as well as their latency after trial onset were recorded by the Matlab scripts. The session was filmed with a Noldus IT camera system from two angles simultaneously, to monitor the child's behavior and to capture any parent or experimenter interference.

### 3.2.3 | Procedure

The touchscreen was newly calibrated each session with the standard 4-point ELO calibration procedure in the Universal Pointer Device Driver (version 05.01.1482B; Touch-Base Ltd.). At the beginning of the session, the experimenter instructed the parent that only the child should touch the screen, and asked the parent if the child was familiar with playing games on a touchscreen device. All children had chance to touch the screen and see that it reacted to their presses before the game began.

The task began with an image of a frog in a natural scene on the screen and the experimenter giving the child a short introduction. The experimenter told the child, "Can you see the frog? It's sitting on a leaf, see? It's about to jump from one leaf to another, do you want to see? Tap the frog then." Once the child touched the frog on the introduction screen, the array of leaves appeared and the experimenter told the child, "These are the leaves, see? The frog is going to jump, are you ready? Tap one of the leaves." Once the child touched a leaf, the frog appeared on the first leaf of the sequence. The frog stayed on-screen until the child pressed within the bounding box of one of the leaves. In the frame immediately after this touch response, the frog appeared on the next leaf in the sequence, with no inter-trial interval. The experimenter spoke to the child throughout as necessary, repeating phrases



such as “Where is the frog now?” and “Catch the frog!”. Since some children were distracted by this while others were encouraged, the protocol was flexible and some children heard more experimenter speech than others. Between blocks, a new image of a frog in a natural scene was presented and the children were given the opportunity for a short break if they wanted one. The child tapped the frog in this image to move on to the next block.

The task lasted approximately 3 min and consisted of 100 trials in total. Blocks 1 and 3 each consisted of 40 individual frog appearances: eight repetitions of a 5-item sequence presented continuously with no breaks. Block 2 served as the baseline and consisted of 20 individual frog appearances with no underlying sequence.

### 3.3 | Data analysis

#### 3.3.1 | Data inclusion

Of the 53 children tested, five did not produce a full dataset because they did not complete the task (three high-likelihood and two low-likelihood). Furthermore, during data collection the experimenter coded whether the child appeared to understand the task, for example, by monitoring whether the child was looking at the screen while pressing, and on this basis datasets from four children (one high-likelihood and three low-likelihood) were excluded from further analysis, leaving data from 44 children (23 high-likelihood and 21 low-likelihood) in the final sample.

#### 3.3.2 | Data processing

Only responses provided while the frog was on-screen were analyzed. Touch responses made anywhere within the bounding box of the leaf where the frog appeared were marked as correct, and touches made within the bounding box of any other leaf were marked as incorrect. Accuracy scores were calculated for each child, but only correct responses were further analyzed in the model. Finally, response times were log transformed in order to allow fitting of linear predictors.

#### 3.3.3 | Statistical analysis

Data were plotted in R (version 3.5.1; R Core Team, 2018) using RStudio (version 1.1.456; Allaire, 2012) with ggplot2 (Wickham, 2016) and ggpubr (Kassambara, 2020), and analyzed using multi-level modelling with lme4 (Bates et al., 2015). Variance explained by the models ( $R^2$ ) was calculated using the method explained by Snijders and Bosker (2012), in the mitml package (Grund et al., 2019). Recurrence Quantification Analysis was carried out with code made available by Kingstone and colleagues (retrieved from <https://psych-barlab.sites.olt.ubc.ca/files/2018/10/RqaMatlab.zip>). Predictors in multi-level models were considered as significant if the t-value exceeded  $\pm 1.96$ .

#### 3.3.4 | Response latency

We used multi-level modelling to examine sequence learning as indexed by response times over sequence repetitions and blocks. The maximal model was pre-specified as follows:

$$\text{maximalRTModel} \leq - \text{lmer} (\text{RTlog} \sim \text{Block} * \text{Repetition} * \text{Group} \pm \text{Item} \pm (1 \pm \text{Block} | \text{P:Group}))$$

We expected that the high-likelihood children and low-likelihood children would learn at the same rate in the sequence with no added noise, but that if the high-likelihood children assigned higher precision to their prediction errors, they would show a slower learning rate when noise was added. That is, we expected an interaction between Block, Repetition, and Group if the learning rate of the children at high likelihood of autism was affected more by the noise manipulation than the learning rate of the children at low likelihood. The maximal model allowed for these interactions as well as all main effect terms, and included random intercept of participant nested within group, and random slopes of block by participant nested within group. To investigate whether this maximal model explained the data better than a more parsimonious solution, the maximal model was compared to two simpler models as follows:

$$\text{simpleRTModel1} \leq - \text{lmer} (\text{RTlog} \sim \text{Block} * \text{Repetition} * \text{Group} \pm (1 | \text{P}))$$

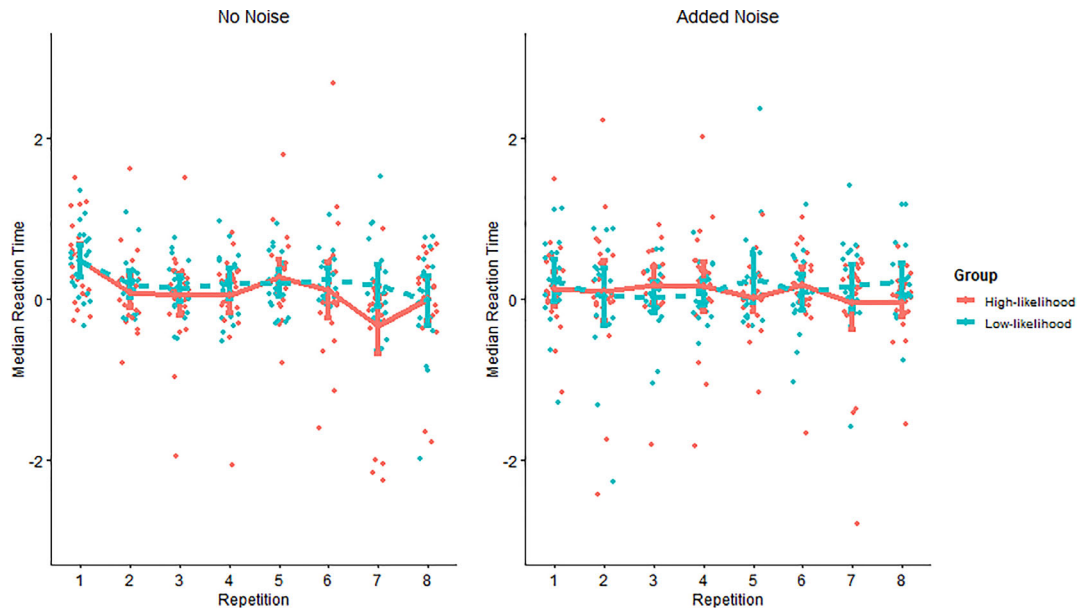
$$\text{simpleRTModel2} \leq - \text{lmer} (\text{RTlog} \sim \text{Block} * \text{Repetition} * \text{Group} \pm \text{Item} \pm (1 | \text{P}))$$

#### 3.3.5 | Response location

We used Recurrence Quantification Analysis in order to examine the spread of touch response locations. Recurrence Quantification Analysis is a dynamical systems analysis which allows comparisons of non-linear process-level information about a changing system such as a learner moving through cognitive states (Coco & Dale, 2014; Richardson et al., 2014). We applied this analysis to the current study to quantify recurrence, or how often the children revisited a previous state and gave responses in the same location as previous responses, and determinism, or how often these revisitations followed each other consecutively.

We expected to see recurrence and determinism at similar levels in the two groups in the first block, when they were presented with a sequence with no noise, and both less recurrence and less determinism in the high-likelihood group in the added-noise block if they indeed showed higher precision of their prediction errors. We therefore expected to see an interaction between Block and Condition in both recurrence and determinism.

For Recurrence Quantification Analysis calculations, we set the radius for neighbor selection at 100, meaning that the Euclidean distance between every pair of responses was calculated, and those pairs within 100 pixels of each other were counted as recurrent. We chose 100 pixels as the value for this parameter after exploring the data



**FIGURE 2** Median response times per child plotted as a function of repetition number. Each repetition of the sequence consisted of five frog appearances, and the median reaction time from each child over these five presentations is shown. Note that models were run on raw response times and individual medians are used for visualization only, to enable comparison to previous results

without block and group information, because this value maximized the information available for the models; lower values led to a floor effect and higher values led to a ceiling effect. The models to compare the levels of recurrence and determinism in each group and each block were specified as follows:

```
recModel <- lmer(recurrence ~ Block * Group ± (1 | P:Group))
detModel <- lmer(determinism ~ Block * Group ± (1 | P:Group))
```

With an expected interaction between block and group on both measures if the high-likelihood group indeed showed higher precision, since this would be more noticeable during the noise-added block. Again, to investigate whether the pre-specified models explained the data better than a parsimonious solution, the recurrence and determinism models were also compared to null models as follows:

```
nullrec <- lmer(rec ~ 1 ± (1 | P:Group))
nulldet <- lmer(det ~ 1 ± (1 | P:Group))
```

## 4 | RESULTS

### 4.1 | Response latency

Visual inspection of the data showed that log-transformed reaction times decreased with repetitions of the sequence, suggesting that participants did learn, in the first block with no noise, but not in the third block in which the frog's position was jittered (see Figure 2)<sup>1</sup>. Additionally, comparison of the response times during block 2, in which the frog appeared in pseudorandom locations showed that the high-likelihood

and low-likelihood children did not differ in their baseline response times ( $\beta = 0.58$ ,  $SE = 0.64$ ,  $t = 0.91$ ).

The pre-specified maximal model was fitted successfully, and two more parsimonious models with simpler random effects structures were run in order to perform model comparisons. The maximal model and two simpler models were then compared using the Akaike Information Criterion. The maximal model explained the data best, with an AIC of 5648, compared to 5686 for the first simple model and 5673 for the second simple model. We also ran an additional exploratory model to investigate contributions of developmental level and the continuous measure of autism symptoms. This model showed that including ADOS and Mullen scores as predictors improved overall model fit, with an AIC of 4983, but these predictors did not reach the significance threshold. We therefore report the pre-specified model here.

There was a significant main effect of block ( $\beta = 0.07$ ,  $SE = 0.03$ ,  $t = 2.28$ ) and repetition ( $\beta = -0.02$ ,  $SE = 0.005$ ,  $t = -3.82$ ), and no main effect of group ( $\beta = 0.009$ ,  $SE = 0.06$ ,  $t = 0.15$ ) or item ( $\beta = -0.005$ ,  $SE = 0.008$ ,  $t = -0.61$ ). There was a significant interaction effect between block and repetition ( $\beta = -0.01$ ,  $SE = 0.005$ ,  $t = -2.34$ ) confirming that the participants overall learnt more slowly in the added-noise block than the no-noise block. The interaction between repetition and group was marginally significant ( $\beta = -0.01$ ,  $SE = 0.005$ ,  $t = -1.97$ ) indicating that the groups may differ in their learning rates when their response times were averaged over blocks. In order to explore this interaction effect, we ran two linear regression models with repetition as the only predictor, separately for each group. These post-hoc models showed that, if anything, the high-likelihood group learnt faster ( $\beta = -0.02$ ,  $SE = 0.008$ ,  $t = -3.02$ ) than the low-likelihood group ( $\beta = -0.006$ ,  $SE = 0.007$ ,  $t = -0.83$ ), with a larger absolute beta value indicating a steeper slope. Most importantly, there was no interaction between Block, Repetition and Group ( $\beta = -0.003$ ,  $SE = 0.005$ ,

<sup>1</sup> Pilot data with adults ( $N = 9$ ) showed that participants learnt the sequences in both conditions, confirming the sequence is learnable with added jitter.

**TABLE 2** Summary of best-fit response latency model

Response latency model			
$\text{maximalRTModel} \leq -\text{lmer}(\text{RTlog} \sim \text{Block} * \text{Repetition} * \text{Group} \pm \text{Item} \pm (1 \pm \text{Block}   \text{P:Group}))$			
Fixed effects	Estimate	Std. error	t value
Block	0.07	0.03	2.28
Repetition	-0.02	0.005	-3.82
Group	0.009	0.06	0.15
Item	-0.005	0.008	-0.61
Block * Repetition	-0.01	0.005	-2.34
Block * Group	0.001	0.03	0.04
Repetition * Group	-0.01	0.005	-1.97
Block * Repetition * Group	-0.003	0.005	-0.59
Random effects	Variance	Std. Error	
Intercept	0.12	0.35	
Block	0.009	0.10	
Residual	0.36	0.60	

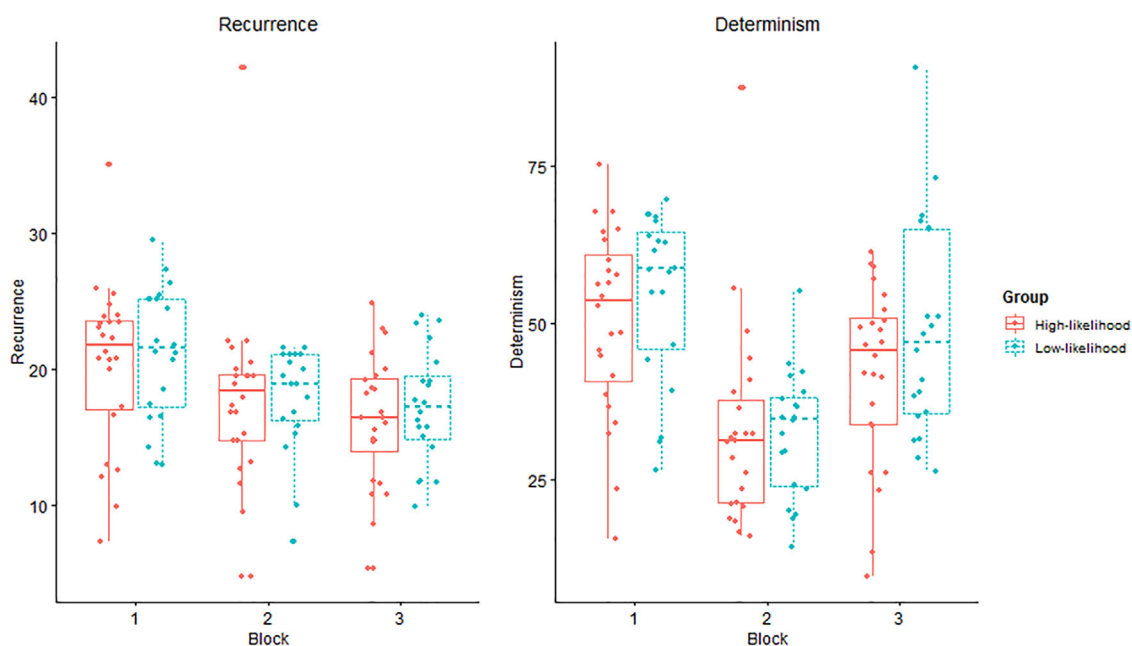
$t = -0.59$ ), showing that the noise manipulation did not affect the learning rate differently in the high- and low-likelihood children (see Table 2 for full model output). The effect sizes are rather small, with this model explaining approximately 2.8% of the total variance in the response times. We expected that our manipulation would induce a subtle effect, especially given the other factors influencing response times, such as hand-eye co-ordination and motor planning and execution, which are still quite immature in 3-year-olds (Schneiberg et al., 2002).

## 4.2 | Response location

Visual inspection shows levels of recurrence consistent with previous work (e.g., López-Pérez et al., 2018). Levels of both recurrence and determinism were lower for block 2, which was expected since there was no sequence to learn, and block 3, which was expected since there was jitter added to the sequence, compared to block 1 (see Figure 3). This was reflected in a significant main effect of block in the recurrence model ( $\beta = -2.07$ ,  $SE = 0.50$ ,  $t = -4.15$ ), but not in the determinism model ( $\beta = -4.18$ ,  $SE = 2.25$ ,  $t = -1.86$ ). Crucially, there was no significant main effect of group on either recurrence ( $\beta = 0.42$ ,  $SE = 2.00$ ,  $t = 0.21$ ) or determinism ( $\beta = 0.79$ ,  $SE = 7.37$ ,  $t = 0.11$ ), and no significant interaction between block and group on either measure (see Table 3 for full model output). Both models outperformed the null models, and the recurrence model explained approximately 7% of the total variance in the response locations, while the determinism model explained approximately 2% of the total variance in response locations.

## 5 | DISCUSSION

The current study tested whether young children with high likelihood of autism weight their prediction errors more highly than low-likelihood children, using a novel implicit learning task. During the game, 3-year-olds with both high and low likelihood learnt while interacting with a sequence without noise, but did not learn with a similar sequence when noise was added in the form of jitter in the stimulus location. While the model confirms that the high-likelihood group learnt faster overall than the low-likelihood group, and the noise manipulation did in fact influence learning rate, it did not influence



**FIGURE 3** Recurrence and determinism (as percentage of recurrent points) by group and block. In blocks 1, the frog moved from leaf to leaf in a deterministic pattern with no noise added. In block 2, there was no pattern and the frog appeared 20 times in a pseudorandom series of locations. In block 3, the frog moved from leaf to leaf in a deterministic pattern with noise added in the form of jitter

**TABLE 3** Summary of best-fit recurrence model (above) and determinism model (below)

Recurrence model			
$\text{recModel} \leq -\text{lmer}(\text{recurrence} \sim \text{Block} * \text{Group} \pm (1   \text{P:Group}))$			
Fixed effects	Estimate	Std. error	t value
Block	-2.07	0.50	-4.15
Group	0.42	2.00	0.21
Block * Group	0.08	0.74	0.11
Random effects			
	Variance	Std. error	
Intercept	15.65	4.00	
Residual	11.93	3.45	
Determinism model			
$\text{detModel} \leq -\text{lmer}(\text{determinism} \sim \text{Block} * \text{Group} \pm (1   \text{P:Group}))$			
Fixed effects	Estimate	Std. error	t value
Block	-4.18	2.25	-1.86
Group	0.79	7.37	0.11
Block * Group	1.34	3.33	0.40
Random effects			
	Variance	Std. error	
Intercept	26.75	5.17	
Residual	242.18	15.56	

the high-likelihood and low-likelihood groups differentially. In an additional exploratory model, we also saw no significant effect of developmental level as measured by the Mullen Scales of Early Learning, or autism symptoms as measured by the ADOS-2 (Lord et al., 2012; Lord, Rutter et al., 2012) on the learning rates. Further, the recurrence quantification analysis showed that the groups did not differ in either the recurrence or determinism of their response locations. The results as a whole suggest that the high-likelihood children did learn differently than the low-likelihood children, but not in a manner consistent with over-adjusting their expectations based on high-precision prediction errors. These results challenge the Predictive Processing theory of autism that claims that autistic individuals assign higher precision to their prediction errors than non-autistic individuals.

The lack of a group difference in the added-noise block is surprising, but is qualified by the fact that neither group appears to learn while interacting with the sequence in the block with added noise, as evidenced by a relatively flat response time profile over sequence repetitions. It is unclear why the participants did not learn the sequence in the added-noise condition. It is possible that participants either became fatigued since the game required sustained attention or so practiced that the learning effect would be masked. However, the data do not appear to support a practice or fatigue effect upon visual inspection: if there were a strong practice effect, we would expect to see faster response times and less variance, and vice versa for a strong fatigue effect, but the mean response times and the variance in the added-noise condition are not remarkably different than in block 1. The pattern of responses also appears relatively symmetrical around the mean, which suggests that the participants have not reached a floor or ceiling

in response times. We conclude therefore that the learning profile in the added-noise condition is likely due to the jitter itself, and therefore informs us about how difficult it is for 3-year-olds to learn from noisy input. It seems unlikely that the jitter entirely obscures the sequences, since adult pilot participants did learn the sequence in both conditions, but the 3-year-olds may have found it harder than older participants to inhibit their responses to the jitter, leading to difficulty abstracting away from individual noisy events and attending to the underlying pattern.

The fact that the high-likelihood group showed faster learning overall but that their response locations were not affected differently than those of the low-likelihood group by the noise manipulation does not support the theory of higher precision of prediction errors in autism (Van De Cruys et al., 2014; 2017). This result adds to a body of converging evidence using different task modalities, methodologies, and age groups showing that predictions of Predictive Processing theories have not been confirmed (e.g., Manning et al., 2017; Utzerath et al., 2018; Van der Hallen et al., 2017; Ward et al., 2020). While there is some existing evidence consistent with altered precision weighting in autism (e.g., Crawley et al., 2020; Manning et al., 2015; Manning et al., 2017; Van der Hallen et al., 2017; Zaidel et al., 2015), it is not currently strong enough to constitute a convincing unifying theory of autism.

Despite these challenges, Predictive Processing theories of autism have benefits that other existing autism theories do not: they posit a singular cause of all symptoms and experiences, with relatively well-specified underlying mechanisms, based on neural dynamics that are well-understood in the non-autistic population. Refinement or revision may therefore render its claims more generalizable and allow further insight into autism and its development. One of the most appealing aspects of Predictive Processing theories of autism is their simplicity: the available parameters that can be implicated are limited to expectations, prediction errors, and their relative weighting. This simplicity could serve to make the theories falsifiable, but can also be a disadvantage, as there is little flexibility to make adjustments when the data consist of a larger number of latent dimensions, meaning that observed differences could not be modelled by a simple shift in those parameters. Adding nuance to the broad claims of the existing accounts of autism may help accommodate thus far conflicting results. For example, recent work on Predictive Processing accounts of schizophrenia posit different precision-weighting strategies for low-level sensory prediction errors compared to higher cognitive prediction errors (Sterzer et al., 2018). If this model proves to account well for data from participants with schizophrenia in the future, it would be one example of a direction for further development of autism accounts. The Predictive Processing theoretical framework may not be currently suited to explain all of the mechanisms underlying autism, but it should not yet be dismissed.

Predictive Processing accounts of autism may not find much support in these data, but Predictive Processing has barely been tested in participants other than neurotypical adults. It does seem that predictions and prediction errors are already observable in infancy (Emberson et al., 2015; Kayhan et al., 2019; Zhang, Jaffe-Dax, Wilson & Emberson, 2019), but the framework has yet to incorporate an account of





how infants ever come to start making predictions and computing prediction errors, and how these capacities change over time (Kayhan & Kwisthout, 2017). Whereas there is still much work to be done to make the Predictive Processing framework fully developmental, it does allow us to make predictions about development: a noisier system, which is still being finetuned, leads to less reliable estimates of incoming sensory information and therefore requires more weighting of predictions based on prior knowledge. We would expect then that the noise in the senses of infants and young children would lead to less weighting of prediction errors in infants than in adults. This change in weighting can be seen, for example, in a study in which children of different ages performed a temporal estimation task (Karaminis et al., 2016). Here younger children, whose temporal discrimination was still quite noisy, weighted their prediction errors less and therefore their prior experiences more heavily, compared to older children, who had more finely tuned temporal discrimination abilities and consequently weighted their prediction errors more and their prior experience less. This concept applies equally to noise in the motor system, such that an infant or young child making a certain action plan will perform that action with more variance than an adult with fine-tuned dexterity (von Hofsten, 1991). In fact, it seems that children reaching to grasp an object do not show adult-like kinematics until 8–10 years of age (Schneiberg et al., 2002). Therefore, in order to make inference about the consequences of an action, for example, a causal inference over a light coming on when a button is pressed, children should optimally down-weight errors if the light does not come on, to allow for the possibility that the inference is correct but the action was planned or performed imperfectly.

Any interpretation of the current results must then take into account the fact that young participants necessarily have noisier estimates than adults, and compare participants with similar levels of noise in their sensory and motor systems. We attempted to achieve this in the current study by measuring the participants' perceptual and motor abilities as part of the Mullen Scales of Early Learning, and accounting for these scores in one of the models. Since the model including Mullen scores did not explain the data better than the model without this predictor, we do not see evidence that differences in noise in the sensory and motor systems between the two groups influenced their performance on the task. This could be more directly analyzed in future studies by recording motion tracking during the task, a method which has recently shown interesting insights into the development of autism and neurodevelopmental disorders (Achermann et al., 2020; Caruso et al., 2020).

The fact that 3-year-old participants still have rather noisy motor planning and execution (Schneiberg et al., 2002) certainly exerts a large influence on our current findings. The variability in reaction times explained only by the noisy motor system is likely very large, and may have drowned out the small effect of our experimental manipulation. Previous studies on the influence of stimulus variability on learning in infants have shown effects of around Cohen's  $d = 0.8$  (Tummeltshammer & Kirkham, 2013; Tummeltshammer et al., 2014; Tummeltshammer & Amso, 2018), but these studies used eye movements as their dependent measure. Eye movements become adult-like already around 6 months of age (Hunnis, 2007) and are therefore much less vari-

able in the early years than limb movements (Schneiberg et al., 2002). It may therefore be necessary to limit future studies to less variable measures such as eye-tracking, to characterize motor responses more finely using motion tracking, or to collect more data to allow statistical inference despite large variability between trials.

In conclusion, we observed no evidence in the current task that 3-year-olds with high-likelihood of a later autism diagnosis assign higher precision weighting to prediction errors than 3-year-olds with a low-likelihood, in either response latency or location, although this should be interpreted with caution due to the lack of learning by either group in the added-noise condition. Future studies should take advantage of the richness of reaching data by using motion tracking technology, or otherwise reduce variability in responses in order to detect small true effects.

#### CONFLICT OF INTEREST

The authors declare no conflict of interest.

#### DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available on the Open Science Framework at <https://osf.io/k258d/>.

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