Stochastic resonance and internal noise in schizotypal traits: a random dot kinematograms paradigm

Response to reviewers

Reviewer #1

Thank you for the opportunity to review the Stage 1 Registered Report "Stochastic resonance and internal noise in schizotypal traits: a random dot kinematograms paradigm". The research project provides a comprehensive and well-structured exploration of the relationship between Random Dot Kinematogram (RDK) performance and schizotypy traits. The research design, data collection, and analysis methods are rigorous and align with current best practices

Strengths of the Study

Clear Objectives: The study sets clear and relevant research questions that contribute to a deeper understanding of perceptual processing in individuals with varying levels of schizotypy.
Methodological Rigor: The experimental design, is appropriate for the research objectives and measures employed to assess schizotypy are validated and reliable, ensuring the robustness of the findings.
Comprehensive Data Analysis: The statistical techniques used and power estimation are suitable for examining the relationship between schizotypy and task performance. The study effectively handles potential confounding factors and provides a thorough interpretation of the results.
Theoretical Contribution: By linking schizotypy to perceptual differences, the study adds valuable insights to the literature on subclinical psychotic traits.

Suggestions for Further Exploration
Although the study meets high standards, it could benefit from future research directions. For instance, applying a Bayesian Component Analysis (BCA) approach might provide a deeper understanding of the latent structures underlying the perceptual differences observed.

We thank the reviewer for the positive and thorough comments and for the insightful suggestion. We are aware that there may be alternative and valid statistical approaches we have not considered. From our understanding, BCA is used to estimate latent variables from high-dimensional datasets. We assume that in this context the reviewer is suggesting using BCA to estimate the **neural noise** in the data as a latent variable, given that our study does not directly measure it but instead infers its effects over behavioral performance in the RDK task.
While we see the strength of modelling neural noise as a latent factor, we are not sure of how well BCA would apply in our dataset. At the current stage, our study primarily includes two measures: RDK task performance and schizotypy scores, which may not provide enough dimensionality to effectively apply BCA.
At the same time, the other reviewers pointed out how adding at least another questionnaire to measure other variables related to personality traits and perceptual experiences would be particularly relevant in this case. We recognized the merit of this suggestion and decided to adopt it. The inclusion of an additional variable could provide a stronger foundation for future applications of a Bayesian approach in our analyses. However, given that our preregistration is structured around specific hypotheses and includes an a priori power analysis tailored to our primary analyses using GLMMs, we believe it is more appropriate to treat this as an exploratory analysis rather than integrating it into the preregistration at this stage.

Reviewer #2

I enjoyed reading the introduction and methods, it is a very interesting research agenda. I look forward to read the results! Nonetheless I have some comments that I hope will help the authors to clarify and improve their manuscript.
In general, my main concern is with respect the using of PCA instead of the subscales as main analysis. I also think the hypothesis and its link with the analysis need some clarification. I did not see an adequate matching between statistical analysis and hypothesis. More details above.

We appreciate the reviewer's feedback and the attention to the analysis part.
We initially considered using PCA to reduce dimensionality and capture shared variance across schizotypy spectrum. However, after further reflection and the reviewers' comments, we agree that keeping the already set subscales separate would make more sense, as each subscale can adequately represent distinct aspects of perceptual and cognitive variability in schizotypy.

We see the need to clarify the connection between our hypotheses and statistical analysis. Our main hypothesis suggests that schizotypy influences how internal and external noise interact, which in turn affects performance in the random dot kinematogram (RDK) task. To test this, we use a generalized linear mixed model (GLMM), modeling perceptual accuracy based on external noise levels, schizotypy scores, and their interaction.
A fundamental expectation is that the relationship between schizotypy and external noise levels follows a nonlinear pattern, specifically an inverted U-shaped curve, as predicted by stochastic resonance theory. To account for this, we incorporate polynomial terms for external noise in our model, with the expectation that a higher-degree polynomial—most likely quadratic—will provide a better fit to the data compared to a linear model.
To improve the clarity between our hypotheses and analysis, we have adjusted the manuscript to better explain our statistical approach and how it aligns with our research questions. Now we clearly define how each hypothesis relates to specific predictors in our models, and we changed the focus on individual schizotypy subscales rather than PCA components to better reflect the distinct traits measured [lines 426-438].

Also, I have other minor comments. Starting with a too big abstract, you may want to consider make it shorter.

We have now slightly reduced the length of the abstract.

INTRODUCTION

Third paragraph add a reference for definition of schizotypy: "Schizotypy refers to a continuum of traits associated with, but less severe than, schizophrenia."

We have now added reference to the sentence.

In the fourth paragraph. You mentioned there is no direct evidence on internal noise in schizotypy. Maybe it would be interesting to describe why using O-LIFE and schizotypy instead of another personality (e.g., any Big-5). Perhaps other personality traits may help better.

When we state that there is no direct evidence on internal noise in schizotypy, we refer to the fact that the existing literature on internal noise focuses on schizophrenia as a clinical diagnosis rather than on schizotypal traits in the general population. Our hypothesis builds on findings from schizophrenia research, where altered internal noise has been documented. The assumption is that this altered internal noise exists on a continuum, ranging from schizophrenia to high schizotypy to low schizotypy. Our aim is to determine whether similar alterations extend to individuals with schizotypal traits who do not meet the criteria for a clinical diagnosis. This is why

we specifically investigate schizotypal personality traits rather than general personality traits: our interest lies in testing a hypothesis derived from schizophrenia research, not in broader personality associations. We test the correlation with schizotypy for two reasons, one theoretical and one practical. Theoretically, we aim to determine whether similar alterations in internal noise extend to individuals with schizotypal traits who do not meet the criteria for clinical diagnosis. Practically, investigating this correlation requires a large sample, which would be infeasible to obtain with a clinical population. Additionally, patients with schizophrenia often undergo medication treatments, which could alter brain noise and confound the results.
Also, we would not expect something like **Big Five personality traits** to be directly related to neural noise. While maybe a subscale like neuroticism could potentially have some relevance, though there is no established link between this trait and neural noise. Additionally, any other traits like conscientiousness, for instance, are unlikely to correlate with performance in the RDK task. We expect the relationship between schizotypy and RDK performance to be driven by differences in internal noise rather than by broader personality dimensions.

Which subscale of schizotypy has more internal noise?

As we state in Hypothesis 2, we expect higher scores on the Unusual Perceptual Experiences (UE) scale to be associated with greater alterations in internal neural noise levels. There is no literature directly showing that internal noise alterations are specific to this subscale. However, it is based on the idea that heightened internal neural noise in the visual perceptual system (Adamek et al., 2023) may be linked to UE. This subscale includes perceptual, hallucinatory, and magical thinking items and aligns with the 'positive' symptom dimension of psychosis (Mason et al., 1995), which makes a good candidate for being the subscale that is more strongly associated with perceptual noise. This would also be consistent with the view that positive symptoms of schizophrenia arise from abnormalities in the brain's inference mechanisms, where new sensory evidence is not properly integrated, leading to false prediction errors (Fletcher & Frith, 2009).

Also, O-LIFE measure schizotypy as personality does not assumes risk of schizophrenia, as other schizotypy questionnaires do. If you want a model or tendency of psychosis, perhaps other schizotypy questionnaire.

There is substantial literature supporting the association between O-LIFE scores and schizophrenia risk (e.g., Polner et al., 2021; Nelson et al., 2013; Pfarr et al., 2023; Dembińska-Krajewska and Rybakowski, 2016). However, this does not imply that individuals with high schizotypy scores will necessarily develop the disorder. Our study does not focus on schizophrenia per se; rather, we assume that along the schizotypal continuum, internal neural noise co-varies (as suggested by previous literature, e.g., Ettinger et al. (2014)). In turn, altered internal neural noise is expected to influence perceptual judgments in tasks such as the Random Dot Kinematogram (RDK). Also, we specifically want to target the general population and normal variations in schizotypal traits, rather than clinically relevant conditions (e.g., schizotypal personality disorder).
Additionally, while some questionnaires may be more sensitive to extreme scores, this would not allow for straightforward correlation analyses. The prevalence of schizotypy is around 38% (Everett and Lindscott, 2022), hence the distribution of scores in the general population is expected to be skewed, with more non-clinical participants scoring at the lower end. As a result, there is limited variance, making it difficult to capture meaningful relationships across the full range of schizotypy traits. We therefore believe that a questionnaire like the O-LIFE, that targets the healthy population and captures schizotypal traits as a continuum, is more suitable for our study. Its broader range allows us to investigate variations in schizotypy without being restricted to extreme cases, facilitating a more comprehensive analysis of its relationship with perceptual processes.

Fifth paragraph in section “Internal Noise in the schizotypal population” contains “a spectrum of traits related to schizophrenia.” This may not be needed because you described the relationship between schizotypy and schizophrenia in previous paragraphs.

We agree that this is redundant and have now removed it.

Pag 8 second paragraph, you are using “aperiodic slope” but for someone outside the field, it may not be clear what is this. Also, provide a summary of what is the evidence suggesting is there a difference in the slopes or not.

Given you are not using EEG or neuroimaging, maybe you can summarize the electrophysiology and neuroimaging into some brief paragraphs. The intro may be long.

We acknowledge that the section on the aperiodic EEG spectrum might be somewhat unclear for non-experts (i.e., those without specialized knowledge in EEG analysis). To address this, we have added a definition of the aperiodic component in the main introduction and clarified the Aperiodic Neural Activity section, ensuring that these explanations remain concise and accessible.

Additionally, for the sake of brevity, we have aimed to be more concise in our descriptions of EEG and neuroimaging findings. **We managed to reduce word count without compromising content.** However, we believe that further reductions would risk undermining the intelligibility of our hypotheses, which are built upon a solid framework derived precisely from EEG and neuroimaging studies. Without this context, it would be difficult to fully appreciate the relevance of our psychophysical study.

METHODS

Add in methods whether if the number of dots for each trial (both presentations: motion and random) will be the same. My guess is yes, but explicitly specify it.

The assumption is correct, the number of dots is always the same in a pair of intervals. We have now specified it at page 11.

What is the effect size similar to “effects found for age in Di Ponzio et al. (2024)“?

Given that generalized eta-squared (η^2) cannot be directly computed GLMMs, we calculated a simplified eta-squared approximation by dividing the χ^2 for each term by the total χ^2 across all terms. Thus, these simplified η^2 values indicate the proportion of the overall variance explained by each term relative to the total model.
The resulting simplified eta-squared values were:

Term	χ^2	df	Simplified η^2
Intercept	709.07	1	0.527
Dot numerosity (cubic polynomial)	607.58	3	0.451
Age	0.46	1	0.0003
Dot numerosity × Age interaction	28.61	3	0.021

This suggests a medium effect size for the interaction between dot numerosity and age. We have also added this information in the manuscript at page 15.

You are mentioning twice the “one-up, two-down staircase procedure”, in “Manipulated variables” and “Procedure”. I wonder if this is a bit redundant. Perhaps this two sections are needed for the PCI registered reports. Same comment for the subsection “Constant Stimuli Block” it seems that you have explained this already in procedures.

In the section “Measured variables” subsection “Thresholding block” you also describe the one-up two-down method and the “correctly in 70.7% of trials”. This is redundant, you are describing this 3 times.

We agree that the sections Procedure, Manipulated variables and Measured variables are quite redundant. This division is not strictly required by the PCI registered report, so we have collapsed all the information in the Procedure paragraph and removed the other two.

In the section “Planned statistical analyses”, why using O-LIFE PCA as your main analysis, and using the already validated subscales: unusual experiences, cognitive disorganization, and introvertive anhedonia as exploratory analysis? I am not sure why you decided to use PCA, why you want to “capture the underlying dimensions of schizotypal traits” if these are already captured in the questionnaire subscales? This part needs a bit more justification.

We agree with the reviewer that the PCA might be unnecessary in this specific case.
We initially considered using PCA to reduce dimensionality and capture shared variance across schizotypy spectrum. However, after further reflection and the reviewers’ comments, we agree that keeping the already set subscales separate would make more sense, as each subscale can adequately represent distinct aspects of perceptual and cognitive variability in schizotypy. For this reason, we have removed the PCA from our planned analyses.

With respect the 4th degree polynomial models. What happened if the model assumptions are not met?

The experimental design is based on a previous study (Di Ponzio et al., 2024) that employed a large sample size (n = 214). Compared to the original study, schizotypy questionnaire scores replace age as the key predictor variable. The fact that our new study builds upon the foundation of the previous one is one of its strengths, which also motivated us to undertake a full preregistration. Indeed, we have access to a highly similar dataset that allows us to model power and statistical analyses with greater precision. We do not anticipate major deviations in the distribution of key variables between the two studies.

For instance, in our preregistered model, we included only the random intercept as a random effect, excluding random slopes. While including random slopes is generally preferable, our previous dataset demonstrated that such a model was too complex, leading to convergence issues and an unidentifiable correlation matrix, even with a large amount of data (glmer returns singularity). Therefore, we scaled the random-effects structure down to only the intercept. This approach may result in slight inflation of effect sizes, but it ensures model convergence while maintaining valid assumptions, which were checked using the DHARMa package (add citation). Given these considerations, we are reasonably confident that the preregistered model can be effectively applied.

However, if significant fit issues or assumption violations arise, our first approach would be to reconsider the inclusion of random slopes. In a more extreme case, we could adopt a semi-parametric approach, such as an ANOVA on ranks using the ARTool package, which allows for similar mixed modeling on ranked data, or alternatively, a Bayesian approach. That said, given the success of our modeling choices in the previous study, we expect them to work well in this case as well. To clarify this, we have also added the R script of our power analysis to the preregistration directory.

In the exploratory analysis, are you planning median split based on the total score of the OLIFE?

Yes, this would be the case—we would perform a median split based on the total score. In our previous work (Di Ponzio et al., 2024), a reviewer requested that we conduct a parallel analysis

using age both as a continuous predictor and as a categorical two-level factor (young vs. old). The rationale behind this suggestion was that it would strengthen the results. Since this approach was explicitly recommended in our prior work, we decided to maintain it in this new study, despite the different focus.

Good exclusion criteria, well justified and clear.

We thank the reviewer for the positive feedback

The first figure shows high and low schizotypy, this is only for visualization purpose, right? Because I though the high and low was only for exploratory purposes. Perhaps this needs to be specified. Also, I though the main analysis would be conducted with PCA. I am confused.

Yes, the high and low schizotypy groups in the first figure are only for visualization purposes. In our main analysis, schizotypy is treated as a continuous variable, as we have decided to remove PCA from the analysis. However, since the previous study on which we base our hypotheses explicitly requested a median split, we considered including it in our paper as well for consistency with prior work. Importantly, this would be presented alongside—not as an alternative to—the analysis using schizotypy as a continuous variable.

Why "Hypothesis 2" use Unusual Perceptual Experience. Hypothesis 2 is the same as exploratory analysis? Clarify please.

Hypothesis 2 specifically focuses on Unusual Perceptual Experience (UE) because this subscale is the most theoretically relevant to the perceptual effects we are investigating. There is no literature directly showing that internal noise alterations are specific to this subscale. However, it is based on the idea that heightened internal noise in the visual perceptual system (Adamek et al., 2023) may be linked to UE. This subscale includes perceptual, hallucinatory, and magical thinking items and aligns with the 'positive' symptom dimension of psychosis (Mason et al., 1995), which makes a good candidate for being the subscale that is more strongly associated with perceptual noise. This would also be consistent with the view that positive symptoms of schizophrenia arise from abnormalities in the brain's inference mechanisms, where new sensory evidence is not properly integrated, leading to false prediction errors (Fletcher & Frith, 2009). This analysis is part of our pre-registered hypotheses, not an exploratory analysis. We have added a section in the introduction where we provide more theoretical background that clarifies our choice of focusing on this subscale.

Given the previous results with Age, are you planning to control by age? It would be nice to have that given you already know this effects.

Yes, we plan to control for age by setting an age limit of **49** for participants. This decision is based again on the findings from Di Ponzio et al. (2024), which showed no substantial differences in performance among individuals up to 49 years old, while the most pronounced differences emerged when comparing those under 50 with those 50 and older. This was reported in the Data exclusion section, but we have now included it in the Sampling Plan section as well to highlight its relevance.

Reviewer #2

This seems like an interesting experiment investigating the relative contribution of internal vs external noise in perceptual inference in schizotypy. There are already a few studies out there investigating the contribution of noise in perceptual inference and psychosis-like experience (Haarsma et al., 2023; Benrimoh et al., 2023), limiting its novelty. However, the approach seems solid and could provide further evidence for altered mechanisms in psychosis-like conditions.

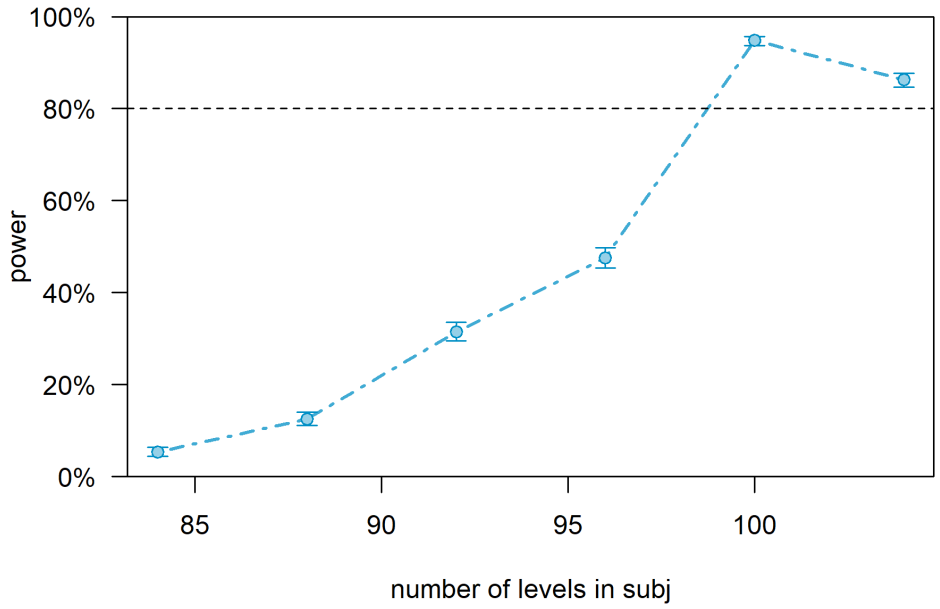
We thank the reviewer for suggesting these references we were not aware of. They appear quite relevant to our work, and we have now added them to the manuscript. At the same time, we believe the most innovative aspect of our study would be to explore the modulation of schizotypy on stochastic resonance, which is something that's never been explored before and is not mentioned in either Haarsma et al., 2023 or Benrimoh et al., 2023.

I would encourage the researchers to ensure that the selection criteria do not undersample individuals with high schizotypy, which might limit the chance of finding effects. Further, the sample size seems to be on the low side for finding correlations in online samples, which more commonly recruit multiple hundreds of subjects. The limitation to a single questionnaire could provide issues. Including some questionnaires tapping into general anxiety or other psychopathological continuums might proof beneficial in controlling for other parts of schizotypy. Further, other questionnaires like the PDI and CAPS could help finding symptom specific correlations in the sample.

Following the reviewer's suggestion, we have decided to increase the sample size to 120 (pending confirmation). While this number does not reach the hundreds of participants mentioned by the reviewer, we hope that this adjustment demonstrates our willingness to strengthen the study while maintaining feasibility. There are clear reasons why we cannot further increase the sample size:

1) Power analysis and ethical considerations

The sample size was determined based on a power analysis that relied on variability and effect size estimates obtained through simulations using data from our previous study (Di Ponzio et al., 2024). While the hypotheses and specific aims of that study differed, the experimental design was identical, allowing us to obtain a reliable power estimation. We attach a plot of the power estimation for clarity below. Additionally, participants in this study are compensated, and our ethics committee has emphasized the importance of balancing statistical power with ethical considerations and budget constraints. Ensuring adequate power is crucial, but we must also avoid unnecessary resource expenditure. We confirm that our study has received ethical approval and have added this information in the manuscript at page 11, which we previously hadn't specified.



2) Extensive data collection per participant

The reviewer should also consider that each participant completes 1,100 behavioral trials and two questionnaires, one of which includes three subscales. As a result, our dataset is highly comprehensive. While other studies may feature larger sample sizes, they often have significantly fewer observations per participant, potentially leading to lower total statistical power than in our case.
Additionally, we have conducted a new power analysis using a slightly different approach which calculated power for the schizotypy * dots interaction in the omnibus test (Walt type III test with Anova() function from CAR package, see the corresponding section in the manuscript), which resulted in a sample size estimate similar to our previous one. While larger samples are common in online studies, our study involves a 35-minute behavioral task that demands sustained attention and focus from participants. This constrains our ability to increase the number of measuring scales due to feasibility and participant fatigue concerns.
For these reasons, we designed our power analysis based on a previous study with a very large sample size (>200 participants) and made reasonable adjustments to ensure sufficient sensitivity while maintaining study feasibility.

Regarding questionnaire selection, we agree with the reviewer on the value of additional measures but also recognize the need to limit participant fatigue. For this reason, we have included the CAPS questionnaire as it is directly relevant to our research aims and is limited to 32 items. We believe that adding further additional scales would compromise feasibility of the study.