

Recent, but not long-term, priors induce behavioral oscillations in peri-saccadic vision

Corresponding Author: Professor David Burr

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter: first round

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Dear Professor Burr,

Thank you for your patience during the peer-review process. Your manuscript titled "Recent, but not long-term, priors induce behavioral oscillations in peri-saccadic vision" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the Reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please highlight all changes in the manuscript text file, and provide a detailed point-by-point reply to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

Editorially, we consider it critical that the referees' concerns regarding alternative explanations and potential confounds are fully addressed. To provide compelling evidence for your conclusions you will need to conduct comprehensive additional analyses and potentially include new data. Additionally, we ask you to include the oscillatory analysis of behavioural error and further analyses as necessary.

As you revise the results section, please also bear in mind that interpretation of non-significant findings derived from NHST is not permitted by the journal's statistical guidelines (<https://www.nature.com/commpsychol/submit/submission-guidelines#statistical-guidelines>). Please provide Bayes Factors to support claims about null results, as you currently do on line 257. Please avoid making claims based on anecdotal evidence following the convention detailed in Schönbrodt, F.D., Wagenmakers, E. Bayes factor design analysis: Planning for compelling evidence. *Psychon Bull Rev* 25, 128–142 (2018) <https://rdcu.be/b6uOC>. Please also ensure to use appropriate language to describe the results. Statements such as 'There is no difference between x and y.' or 'X does not affect Y.' must be revised to read 'We found [no/little] credible evidence of a difference between x and y.' or 'We found [no/little] credible evidence that X affects Y.'

I am attaching a checklist that details critical reporting requirements for the revised manuscript. Please attend to each item and ensure your manuscript is fully compliant. We are requesting that your manuscript aligns with these requirements as this facilitates the evaluation of your manuscript, reducing delays in re-review and potential future acceptance. If your revised manuscript is not aligned with these requests on major issues, such as those concerning statistics, it may be returned to you for further revisions without re-review. Additional information can be found in our style and formatting guide Communications Psychology formatting guide.

If the revision process takes significantly longer than five months, we will be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your

- revised manuscript,
- point-by-point response to the referees' comments,
- cover letter (as a separate document),
- the Editorial Policy Checklist (see below),
- the Reporting Summary (see below), and
- the completed Editorial Request Table (attached):

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Thank you for the opportunity to review your work.

Best regards,

Troby Lui

Troby Lui, PhD
Associate Editor
Communications Psychology

REVIEWER EXPERTISE:

Reviewer #1: rhythmic cognition, saccade/vision, serial dependence/prediction

Reviewer #2: saccade/vision, serial dependence/prediction

Reviewer #3: rhythmic cognition, saccade/vision

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

The authors investigated rhythmic modulations of serial dependence time-locked to saccade onset. They focused on serial dependence effects from both the immediately preceding inducer stimulus (referred to as the 'prior') and the test stimulus presented one trial earlier (referred to as the 'previous stimulus' or 'one-back'). Additionally, they evaluated the orientation bias, also referred to as regression to the mean in this context. They found systematic biases from both sources of serial dependence and from the regression-to-the-mean effect. However, only the serial dependence effects showed periodic modulation by saccade onset, with rhythms observed in the alpha band.

The research question is both interesting and timely, as there is currently no clear link between serial dependence and brain rhythms, and the literature on rhythmic modulations of perception remains mixed. The experimental design and rationale are well thought out. However, I believe there are important methodological choices and interpretations to address.

1. General framing: It is not yet clear whether there is more 'efficient perception' due to serial dependence (line 44) and whether the results are compatible with an optimal combination of past and present. Indeed, several studies have shown that the present but not the previous uncertainty modulates these effects (Ceylan et al., 2021; Gallagher & Benton, 2022; You et al., 2023).

2. This raises the question of whether the findings truly reflect periodic modulation of prior stimulus traces or simply a modulation of current stimulus uncertainty due to alpha rhythms triggered by saccades (e.g., as in a reset of ongoing oscillations). The fact that the analysis is time-locked to saccade onset, not the previous stimulus, suggests that what is observed might reflect modulations in the current stimulus quality, which in turn affects the tendency to be biased or not by previous stimuli, rather than a pure modulation of past stimulus traces.

3. In this study, what they called 'long-term' expectations or 'regression to the mean' may simply be the typical orientation bias effect, where responses are shifted away from the cardinals and toward the diagonals (45° or 135°). The two diagonals correspond indeed to the 'mean' orientations tested. As the orientation bias is typically stronger than other biases like serial dependence, and it seems to be the case also here when comparing the plots, I wonder if one potential explanation of the lack of periodic effects on the regression to the mean is simply due to floor bias (little room for additional modulations).

4. The authors should discuss why the bias towards the previous test stimulus (one-back) appears stronger than the bias towards the 'prior' (or 'inducer'), also referring to previous studies using similar approaches (e.g., comparing the effects of

previously reported or only exposed stimuli) (Abreo et al., 2023; Blondé et al., 2023; Ceylan & Pascucci, 2023; Houborg et al., 2023; Pascucci et al., 2019).

5. The statement that orientation bias (regression to the mean) is seldom discussed is somewhat inaccurate (line 226), as it has become common practice to control for and remove this bias (Gallagher & Benton, 2022; Manassi et al., 2018; Pascucci et al., 2019; Sadil et al., 2023; van Bergen & Jehee, 2019). Indeed, a good control analysis for the orientation bias could have been regressing out this bias before the serial dependence analysis.

6. The authors present an additional analysis where the prior and previous test stimulus have the same directional slant, but it would be even more informative to show what happens when they are opposite, and which one dominates.

7. In addition, this distinction made throughout the paper about the 'prior' (inducer) and the previous test or one-back is unclear. Both are technically 'previous stimuli', differing only in their temporal proximity to the current target and the requirement for a response. Figures 2e and 3e suggest that the one-back effect is larger, despite the prior being temporally closer. This is important to highlight and discuss.

8. The authors use a GLM with aggregated subject data but do not report whether standard preprocessing steps (e.g., detrending, tapering, zero-padding etc.) were followed (Landau & Fries, 2012; Tosato et al., 2022). Moreover, there has been recent concern about the use of similar methods without accounting for AR (autoregressive) components in the data, and there are improved methods to deal with this that the authors may use as a control or more rigorous analysis (Brookshire, 2022; Harris & Beale, 2024)

9. To show that the results are not due to the use of an aggregate subject, it would be more convincing to use methods that take inter-individual variability into account (Harris & Beale, 2024) or to plot the estimated peak frequency for each individual.

10. The study sometimes refers to 'strong' effects, but no effect sizes are reported, and no justification of the sample size is made. At least the effect sizes are useful to inform future research.

Minor Points:

- The 2SD criterion for removing data seems strict. It would be informative to report the average performance (e.g., error standard deviation) before and after this removal.

- The sentence in line 38 is a bit confusing, as serial dependence is not a mechanism and a phenomenon is also not the same as a mechanism.

Reviewer #2 (Remarks to the Author):

The authors present a very creative and novel experiment testing whether serial dependence is modulated around saccades and whether it shows oscillatory behavior. An oriented "prior" Gabor patch was presented presaccadically and then a test patch presented just around saccade onset. Observers adjusted a probe to match the orientation of the test Gabor patch. The authors found strong serial dependence toward both the prior (within the same trial) and toward previous test Gabor patch. The SD effect was modulated around the saccade onset at about 9Hz. There was also a central tendency effect and the authors were able to dissociate it. The results are very interesting, the writing is clear, and the message is important. The manuscript merits publication with very minor revision.

A few very minor comments:

1. It seems the direction of the upcoming saccade was predictable based on the location of the F0? Would it make any difference if the direction of saccade were not predictable?
2. P 3, line 42, add a reference to Manassi & Whitney, NRP, 2024 after the word "continuity fields"
3. P 3, line 44, add reference to Manassi, et al., Jov, 2023 meta analysis after the words "for reviews see"
4. P. 14 line 226 "This confound [regression to the mean/central tendency confound] is almost always present in serial dependence studies, but seldom discussed." It has been discussed in both of the Manassi papers above (2024 and 2023). Box 1 in Manassi 2024 paper has some references for this as well, eg., 16, 30, 85 in that review paper. Manassi's 2023 meta-analysis paper also discusses how central tendency (regression to the mean) can't explain SD effects reported in the literature. References to these two papers should be added.
5. Re central tendency confound (p. 14), I agree that the authors successfully controlled it. But there are additional methods for ruling it out as well (beyond looking at 45 and 135 degree conditions). Additional control analyses that other papers have used to rule out central tendency are in refs 85 and 30 and 16 (from Manassi 2024) for analytic and modeling controls. Though not necessary, it might be worth pointing out that there are multiple ways central tendency/regression to mean has been controlled before.

Reviewer #3 (Remarks to the Author):

SUMMARY

The study investigated whether perceptual judgements biased by short- and long-term perceptual history oscillates around the saccade onset. The short-term perceptual history is evaluated according to serial dependence, i.e., the perception of the current stimulus is biased towards the prior and the previous (trial-1) stimuli. The long-term perceptual history reflects the strong bias towards the mean perception over numerous trials, the central tendency. Interestingly, they found that the serial dependence effect oscillates before and after saccade onset in the alpha frequency range. This was not the case for the long-term perceptual history.

For several reasons, I found the study really interesting. I really appreciate the approach of looking at both short- and long-term perceptual history within the same experiment. It allows the authors to draw reliable conclusions on how these two types of mechanisms act under the same conditions and how they interact. Second, as pointed out by the authors, many studies on vision did not allow eye movements. I found innovative to investigate serial dependence and central tendency with actual saccades. Eventually, perceptual oscillations remain an under-studied field, likely because the analysis requires lots of trial to be performed. Together, these approaches shape an original study with outcomes that could be of interest for many sub-fields of perceptual psychology.

The experiment is well designed and well documented. Similarly, the analyses are smart and appropriate. The conclusion drawn by the authors that perceptual judgements oscillate in alpha according to saccade onset is strongly supported by the experimental data, and is an important result in the field regarding the increased interest in the link between saccades, alpha oscillations, and perceptual abilities.

MAJOR

1. I would be interested in knowing whether the perceptual oscillations around saccade onset occurred for the current perception itself. In other words, could you plot and apply the GLM spectral analysis on the error (i.e., angle difference between test stimulus and response) as a function of time at saccade onset to evaluate if the error also oscillates in the alpha range?
2. I think that one of the strength of study is to looking at both short- and long-term perceptual history within the same experiment. I would expect a bit more discussion on how they have been found to interact together in previous experiments (i.e., Line 53), and in your actual experiment.
3. Figure 5. I found the results a bit confusing. In panel b, the peak around 7 Hz seems significant (above the permutation threshold). But in panel a, the fitted curve does not seem to represent well the data. By eyes, I have the impression that the data could oscillate at a higher frequency. Could you test frequencies above 25 Hz for this analysis?

MINOR

1. I suggest to specify perceptual oscillations and brain oscillations throughout the paper to avoid confusion.
2. I think that the term one-back (used in Figure 4) instead of previous is more explicit to mention the previous stimuli.
3. Lines 54, 60: I would avoid terms such as "more complex phenomena", it is too vague and non-informative, I suggest to replace it with the precise name of the phenomena.
- Figure 1. I would make the Gabor patch more visible for descriptive purposes.
- Figure 3. Line 190. "The amplitude spectrum shows a clear peak around 9.4 Hz." I think the peak is at 9.9 Hz.
4. Line 297: Could you provide time-windows related to the effect of short- and long-term perceptual history?
5. Line 324: Could you develop the mentioned oscillation-based theories of working memory? What are these theories and how do they relate to your findings?
6. Line 373: Could you report the age (mean+std) of the two groups of participants?
7. Line 393: What was the rationale for displaying the stimuli at 8° from the center? Do the authors think that their results would replicate with stimuli closer, i.e., below 5° in the parafovea, or further away (more in the periphery)?
8. Line 425. Could you justify why you restricted the analysis in the [-200; +200] ms time-window according to saccade onset?
9. Line 425. Could you develop your smoothing technique?
10. Line 434. "Linear combination of harmonics at each tested frequency". I am not quite sure to understand what the authors meant here. According to equation 1, they modelled sinusoidal waves at each frequency. What is the link with harmonics?
11. Line 450. The permutation tests are already an approach that takes into account the multiple comparison problem. Why applying another FDR correction?
12. Line 459. Could you justify the selection of a 25-ms time-window?

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We ask that you ensure your manuscript complies with our editorial policies and reporting requirements.

To that end, we require revised manuscripts to be accompanied by two completed items: a reporting summary that collects information on study design and procedure, and an editorial policy checklist that verifies compliance with all required editorial policies

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All points on the policy checklist must be addressed. Your revised manuscript can only be sent back to the referees if these checklists are completed and uploaded with the revision.

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Version 1:

Decision Letter: second round

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Dear Professor Burr,

Thank you for your patience during the peer-review process. Your manuscript titled "Recent, but not long-term, priors induce behavioral oscillations in peri-saccadic vision" has now been by our 3 reviewers, and I include their comments at the end of this message. They continue to find your work of interest and are in large parts satisfied. We remain interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

Reviewer #1 has a number of remaining methodological concerns. We discussed these concerns with Reviewer #3. Asking experts for their opinion on each others' reports is a step we regularly take at the journal when reviewer reports are in disagreement and we feel that the editorial decision will benefit from additional expert input. As an alternative, more standard way to resolve the issues highlighted by Reviewer #1, Reviewer #3 recommended you perform an FFT on the behavioural data. We ask you to follow this advice to provide stronger evidence for your conclusions and alleviate remaining concerns. Please also note that Reviewer #1's comment for more comprehensive statistics reporting aligns with journal requirements and needs to be addressed.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

I am attaching an Editorial Requests Table that details critical reporting requirements for the revised manuscript. Please attend to each item and ensure your manuscript is fully compliant. If your revised manuscript is not aligned with these requests on major issues, such as those concerning statistics, it may be returned to you for further revisions without re-review.

Please submit the following items:

- Revised manuscript
- Point-by-point response to the referees' comments
- Cover letter (as a separate document)
- <https://www.nature.com/documents/nr-reporting-summary.zip>>Nature Research Reporting Summary
- <https://www.nature.com/documents/nr-editorial-policy-checklist.pdf>>Editorial Policy Checklist
- Completed Editorial Request Table (attached).

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Additional guidance is available in our style and formatting guide <https://www.nature.com/documents/commpsychol-style-formatting-guide-accept.pdf>>Communications Psychology formatting guide.

We hope to receive your revised paper within 8 weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Troby Lui

Troby Lui, PhD
Associate Editor
Communications Psychology

REVIEWER EXPERTISE:

Reviewer #1: rhythmic cognition, saccade/vision, serial dependence/prediction

Reviewer #2: saccade/vision, serial dependence/prediction

Reviewer #3: rhythmic cognition, saccade/vision

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

The authors have made progress in addressing several of my previous comments. I continue to believe that this manuscript has an important aim and presents promising results that are potentially relevant to the field. However, this makes it all the more crucial to ensure that the methods and results are clarified. Below, I outline my main concerns about the current state of the manuscript.

1. While I appreciate the inclusion of effect sizes in the statistical reporting, it would be useful to also report effect sizes for the main analysis of interest—namely, the GLM on the periodic modulation effects (including also the GLM coefficients, CI and R²).

2. Figure 2b remains a key result, but at least qualitatively, it does not give me confidence about the presence of a true oscillation. The red fit struggles in capturing a clear oscillatory pattern in the black running average. This is why it could help to validate this result with alternative approaches, for instance estimating spectral components at the single subject level (slight differences in the peak oscillation across subjects may obscure trends in the aggregate data) or controlling for unwanted components (I understand that the Brookshire's method might be too conservative, according to the authors'

comments, but there are more recent approaches proposed to increase the sensitivity with a similar method, see Harris & Beale, 2024; also, isn't shuffling the response across trials equivalent to shuffling the delay—i.e., the time 'bin'?).

3. It is still unclear why the authors avoided detrending or a Hanning window (which is done also in the GLM context, see Tosato et al., 2022). I thank the authors for pointing me to the additional comments and replies about the Brookshire approach, but the methodological issue seems in the end unresolved. Considering the delicate topic of rhythms in behavior, the methodological debate, and the mixed findings, I think strong justification or validation of a methodological choice is important, even simply demonstrating that the results remain consistent with standard preprocessing steps (or why it is better to avoid them).

4. The analysis of current precision (Figure 6) uses only trials with orthogonal inducers, resulting in a small subset of data. I understand that this approach avoids confounding effects from serial dependence. However, since the current trial is influenced by both the 1-back and the inducer serial dependence, it is difficult to remove all effects of trial history. It might be therefore more robust to assess variability (e.g., error standard deviation) as a function of time from saccadic onset, incorporating all trials.

5. When discussing why the magnitudes of the two effects are similar despite the inducer being closer in time, the authors could mention evidence from studies showing that reported (and thus memorized, task-relevant, as the 1-back here) stimuli often produce stronger serial dependence effects.

Minor:

The part about efficiency remains unclear. While the authors mention studies showing that current uncertainty matters, it is unclear why this is interpreted as 'detailed evidence' of efficiency. In the context of tasks where participants are asked to discriminate each stimulus at their best, this seems more like a 'system flaw'. Thus, what is meant by efficiency and the actual evidence in support of this (or against) could be made clearer.

(Page 3, Line 10)

This part is still sloppy in my reading. Serial dependence does not create a continuity field, maybe it is the other way around.

(Page 22, Lines 17–18)

The references in these lines appear to have inconsistent font sizes.

In the shared dataset, are the included orientation errors (Ori_error) cleaned of outliers? There appear to be multiple cases with errors exceeding 80°.

Reviewer #2 (Remarks to the Author):

The authors have done an excellent job revising the manuscript and they've addressed all reviewer comments.

Reviewer #3 (Remarks to the Author):

I am very pleased with the authors' revisions, which have made the paper clearer and stronger. I congratulate and thank them for their outstanding work.

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Version 2:

Decision Letter: third round

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Dear Professor Burr,

Your manuscript titled "Recent, but not long-term, priors induce behavioral oscillations in peri-saccadic vision" has now been editorially evaluated. I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology.

We therefore invite you to revise your paper one last time to address a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

In particular, we emphasize that the reporting and interpretation of non-significant results in NHST analyses does not yet comply with our policy (<https://www.nature.com/commspsychol/submit/submission-guidelines#statistical-guidelines>). Please ensure that full statistics are reported for non-significant findings just as for significant findings; that null findings obtained in NHST are not presented as positive evidence for the absence of a difference or effect (and not interpreted in the Discussion); and that your interpretation of Bayes Factors is aligned with journal standards. Please avoid making claims based on anecdotal evidence following the convention detailed in Schönbrodt, F.D., Wagenmakers, E. Bayes factor design analysis: Planning for compelling evidence. *Psychon Bull Rev* 25, 128–142 (2018) <https://rdcu.be/b6uOC>

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Troby Lui

Troby Lui, PhD
Associate Editor
Communications Psychology

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General

We congratulate the editor in selecting three excellent, insightful reviewers, who have definitely improved the manuscript. We have incorporated all their advice in the revision, which includes new analysis (resulting in an extra figure), better statistics, giving effect sizes and Bayes factors where appropriate, and more discussion of salient points.

We sincerely thank all the referees for their time.

Reviewer #1 (Remarks to the Author):

The authors investigated rhythmic modulations of serial dependence time-locked to saccade onset. They focused on serial dependence effects from both the immediately preceding inducer stimulus (referred to as the 'prior') and the test stimulus presented one trial earlier (referred to as the 'previous stimulus' or 'one-back'). Additionally, they evaluated the orientation bias, also referred to as regression to the mean in this context. They found systematic biases from both sources of serial dependence and from the regression-to-the-mean effect. However, only the serial dependence effects showed periodic modulation by saccade onset, with rhythms observed in the alpha band.

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1. General framing: It is not yet clear whether there is more 'efficient perception' due to serial dependence (line 44) and whether the results are compatible with an optimal combination of past and present. Indeed, several studies have shown that the present but not the previous uncertainty modulates these effects (Ceylan et al., 2021; Gallagher & Benton, 2022; You et al., 2023).

Thank you, we now point out some caveats towards total efficiency and reference these works.

2. This raises the question of whether the findings truly reflect periodic modulation of prior stimulus traces or simply a modulation of current stimulus uncertainty due to alpha rhythms triggered by saccades (e.g., as in a reset of ongoing oscillations). The fact that the analysis is time-locked to saccade onset, not the previous stimulus, suggests that what is observed might reflect modulations in the current stimulus quality, which in turn affects the tendency to be biased or not by previous stimuli, rather than a pure modulation of past stimulus traces.

This is a very astute comment, thank you. Given that serial dependence is modulated by the reliability of the current trial, modulation of that reliability would cause serial dependence to modulate too. There is a suggestion that this is not the case, as long-term priors would also be modulated. Also, previous work, including

our own (Ho, Leung, Burr, Alais, & Morrone, 2017) suggests that modulation of sensitivity occur at theta rather than alpha frequencies. However, it is an excellent point that we now test directly, showing that the variance (inverse reliability) of the response does not oscillate when preceded by orthogonal inducers (although there is a hint it may oscillate at 5 Hz). It does when preceded by $\pm 15^\circ$ (not shown in ms), as there the serial dependence will appear as error, and this is modulated. We have added this graph to a new figure, greatly strengthening the manuscript. Thank you.

3. In this study, what they called 'long-term' expectations or 'regression to the mean' may simply be the typical orientation bias effect, where responses are shifted away from the cardinals and toward the diagonals (45° or 135°). The two diagonals correspond indeed to the 'mean' orientations tested. As the orientation bias is typically stronger than other biases like serial dependence, and it seems to be the case also here when comparing the plots, I wonder if one potential explanation of the lack of periodic effects on the regression to the mean is simply due to floor bias (little room for additional modulations).

This is a good point, which we now discuss (p21). The orientation repulsion bias could well be related to central tendency in orientation, and has been investigated with Bayesian techniques (which we now reference). However, it remains a long-term *prior*, so does not really change our argument. We also mention the possibility of a floor or ceiling effect (p23).

4. The authors should discuss why the bias towards the previous test stimulus (one-back) appears stronger than the bias towards the 'prior' (or 'inducer'), also referring to previous studies using similar approaches (e.g., comparing the effects of previously reported or only exposed stimuli) (Abreo et al., 2023; Blondé et al., 2023; Ceylan & Pascucci, 2023; Houborg et al., 2023; Pascucci et al., 2019).

It is not obvious that the bias towards the previous stimulus is stronger than towards the "inducer" (a far better term, which we now use). When considering only 45° and 135° trials (free from central tendency) the average was very similar. And the fact that the two cancelled each other out suggests that the oscillations are of similar magnitude. Of course we do not have the precision to detect subtle changes in magnitude, but given the differences in the two classes of stimuli (temporal proximity to the current stimulus and task demands) we are reluctant to speculate too much at this stage. We have however added some text on page 24.

5. The statement that orientation bias (regression to the mean) is seldom discussed is somewhat inaccurate (line 226), as it has become common practice to control for and remove this bias (Gallagher & Benton, 2022; Manassi et al., 2018; Pascucci et al., 2019; Sadil et al., 2023; van Bergen & Jehee, 2019). Indeed, a good control analysis for the orientation bias could have been regressing out this bias before the serial dependence analysis.

We have removed the claim that it is seldom discussed, and added these references. We agree that regressing out the effect could have been a good alternative approach, and attempted the regression analysis referenced in the

literature (Xia, Leib, & Whitney, 2016) and found that the inducer, 1-back, and mean stimuli all significantly influence current perception. However, believe that our approach of considering only the $\pm 45^\circ$ conditions (immune to central tendency) deals with the issue more directly.

6. The authors present an additional analysis where the prior and previous test stimulus have the same directional slant, but it would be even more informative to show what happens when they are opposite, and which one dominates.

Excellent idea, which we have now done (Figures 4e & f). They cancel each other out, further evidence that they interact quasi-linearly.

7. In addition, this distinction made throughout the paper about the 'prior' (inducer) and the previous test or one-back is unclear. Both are technically 'previous stimuli', differing only in their temporal proximity to the current target and the requirement for a response. Figures 2e and 3e suggest that the one-back effect is larger, despite the prior being temporally closer. This is important to highlight and discuss.

As mentioned above, those figures include the central tendency effect. The more appropriate comparison is figures 2f and 3f, at 45 and 135° (discussed on p14). There the mean effects are very similar. And that the modulations cancel each other suggests they are of similar magnitude. We now discuss this on page 23.

And we like the term "inducer" and now use that term in the revised manuscript (*prior* was a confusing choice). We have also changed the term "previous stimulus" to "1-back" (ref 3), also more precise.

8. The authors use a GLM with aggregated subject data but do not report whether standard preprocessing steps (e.g., detrending, tapering, zero-padding etc.) were followed (Landau & Fries, 2012; Tosato et al., 2022). Moreover, there has been recent concern about the use of similar methods without accounting for AR (autoregressive) components in the data, and there are improved methods to deal with this that the authors may use as a control or more rigorous analysis (Brookshire, 2022; Harris & Beale, 2024).

In part because of the concerns raised by Brookshire (2022), which we have addressed [elsewhere](#), we used the single-trial GML approach, which does not require the binning (and shuffling of bins) that could lead to the artifacts mentioned. We now make that clear.

9. To show that the results are not due to the use of an aggregate subject, it would be more convincing to use methods that take inter-individual variability into account (Harris & Beale, 2024) or to plot the estimated peak frequency for each individual.

We agree that inter-participant variability is important, and we do try to address this issue. Figures 2d and 3d are vector plots of all the participants (at the best common frequency) on which we perform a Hotelling T-square test. Panels 2e, 2f, 3e and 3f show peak and trough variability, with standard errors between participants (and ANOVAs were performed). As all these statistics are across participants, we believe we have addressed the issue adequately.

10. The study sometimes refers to ‘strong’ effects, but no effect sizes are reported, and no justification of the sample size is made. At least the effect sizes are useful to inform future research.

We have added effect sizes (partial eta squared) to all ANOVA and T-test analyses. We have also added Bayes factors to the non-significant tests.

Minor Points:

- The 2SD criterion for removing data seems strict. It would be informative to report the average performance (e.g., error standard deviation) before and after this removal.

We now report the standard deviations both before and after removing trials beyond 2SD. Additionally, we conducted a GLM analysis using all trials, which yielded results consistent with the conclusions presented in the paper. However, to exclude data with high noise—likely due to occasional random responses by participants—we opted to report the results from the outlier-excluded dataset in the main text.

- The sentence in line 38 is a bit confusing, as serial dependence is not a mechanism and a phenomenon is also not the same as a mechanism.

Agree, it was sloppy.

Reviewer #2 (Remarks to the Author):

The authors present a very creative and novel experiment testing whether serial dependence is modulated around saccades and whether it shows oscillatory behavior. An oriented “prior” Gabor patch was presented presaccadically and then a test patch presented just around saccade onset. Observers adjusted a probe to match the orientation of the test Gabor patch. The authors found strong serial dependence toward both the prior (within the same trial) and toward previous test Gabor patch. The SD effect was modulated around the saccade onset at about 9Hz. There was also a central tendency effect and the authors were able to dissociate it. The results are very interesting, the writing is clear, and the message is important. The manuscript merits publication with very minor revision.

A few very minor comments:

1. It seems the direction of the upcoming saccade was predictable based on the location of the F0? Would it make any difference if the direction of saccade were not predictable?

Excellent point, but one we cannot answer with this current data. We add it as a discussion point (p24).

2. P 3, line 42, add a reference to Manassi & Whitney, NRP, 2024 after the word “continuity fields”

We have rephrased this, and added the reference.

3. P 3, line 44, add reference to Manassi, et al., Jov, 2023 meta analysis after the words “for reviews see”

Added

4. P. 14 line 226 “This confound [regression to the mean/central tendency confound] is almost always present in serial dependence studies, but seldom discussed.” It has been discussed in both of the Manassi papers above (2024 and 2023). Box 1 in Manassi 2024 paper has some references for this as well, eg., 16, 30, 85 in that review paper. Manassi’s 2023 meta-analysis paper also discusses how central tendency (regression to the mean) can’t explain SD effects reported in the literature. References to these two papers should be added.

Yes, referee 1 made this point too. We have reworded the sentence, and added the references.

5. Re central tendency confound (p. 14), I agree that the authors successfully controlled it. But there are additional methods for ruling it out as well (beyond looking at 45 and 135 degree conditions). Additional control analyses that other papers have used to rule out central tendency are in refs 85 and 30 and 16 (from Manassi 2024) for analytic and modeling controls. Though not necessary, it might be worth pointing out that there are multiple ways central tendency/regression to mean has been controlled before.

We have included discussions of these studies on methods to rule out central tendency in the serial dependence effect (p21-22). We agree that regressing out the effect could have been a good alternative approach. We also attempted the regression analysis referenced in the literature (Xia, Leib, & Whitney, 2016) and found that the inducer, 1-back, and mean stimuli all significantly influence current perception. However, we believe that our approach of considering only the $\pm 45^\circ$ conditions (which are immune to central tendency) also effectively addresses this issue. Therefore, the additional analysis was not included in the manuscript.

Reviewer #3 (Remarks to the Author):

SUMMARY

The study investigated whether perceptual judgements biased by short- and long-term perceptual history oscillates around the saccade onset. The short-term perceptual history is evaluated according to serial dependence, i.e., the perception of the current stimulus is biased towards the prior and the previous (trial-1) stimuli. The long-term perceptual history reflects the strong bias towards the mean perception over numerous trials, the central tendency. Interestingly, they found that the serial

dependence effect oscillates before and after saccade onset in the alpha frequency range. This was not the case for the long-term perceptual history.

For several reasons, I found the study really interesting. I really appreciate the approach of looking at both short- and long-term perceptual history within the same experiment. It allows the authors to draw reliable conclusions on how these two types of mechanisms act under the same conditions and how they interact. Second, as pointed out by the authors, many studies on vision did not allow eye movements. I found innovative to investigate serial dependence and central tendency with actual saccades. Eventually, perceptual oscillations remain an under-studied field, likely because the analysis requires lots of trial to be performed. Together, these approaches shape an original study with outcomes that could be of interest for many sub-fields of perceptual psychology.

The experiment is well designed and well documented. Similarly, the analyses are smart and appropriate. The conclusion drawn by the authors that perceptual judgements oscillate in alpha according to saccade onset is strongly supported by the experimental data, and is an important result in the field regarding the increased interest in the link between saccades, alpha oscillations, and perceptual abilities.

MAJOR

1. I would be interested in knowing whether the perceptual oscillations around saccade onset occurred for the current perception itself. In other words, could you plot and apply the GLM spectral analysis on the error (i.e., angle difference between test stimulus and response) as a function of time at saccade onset to evaluate if the error also oscillates in the alpha range?

We have now added a new figure 6 which includes this analysis. There is no modulation at all at 9 Hz, and no significant modulation at any frequency. Thanks, this strengthens the paper.

2. I think that one of the strength of study is to looking at both short- and long-term perceptual history within the same experiment. I would expect a bit more discussion on how they have been found to interact together in previous experiments (i.e., Line 53), and in your actual experiment.

We have added a little text on page 21.

3. Figure 5. I found the results a bit confusing. In panel b, the peak around 7 Hz seems significant (above the permutation threshold). But in panel a, the fitted curve does not seem to represent well the data. By eyes, I have the impression that the data could oscillate at a higher frequency. Could you test frequencies above 25 Hz for this analysis?

There is a peak at 7 Hz, above the surrogate permutation line, but this is not significant after FDR correction, suggesting it is more likely a random error rather than a genuine oscillation.

We agree that there does seem to be a high (> 25 Hz) beta-frequency oscillation in the smoothed time-trace. However, given our data sampling rate of 120 Hz (monitor frequency), these high-frequency oscillations may be ringing artifacts resulting from

data sampling and smoothing: 30 Hz would have only 4 samples, approaching the Nyquist sampling rate.

MINOR

1. I suggest to specify perceptual oscillations and brain oscillations throughout the paper to avoid confusion.

In several sections of the manuscript, we have now clarified that we are referring to perceptual oscillations.

2. I think that the term one-back (used in Figure 4) instead of previous is more explicit to mention the previous stimuli.

We agree, and have changed throughout. We also changed the term *prior* to *inducer* (ref 1). Both are more explicit and less confusing.

3. Lines 54, 60: I would avoid terms such as “more complex phenomena”, it is too vague and non-informative, I suggest to replace it with the precise name of the phenomena.

Agreed, we have changed to “...with other phenomena”.

Figure 1. I would make the Gabor patch more visible for descriptive purposes.

We have updated Figure 1 to make the Gabor patch more visible.

Figure 3. Line 190. “The amplitude spectrum shows a clear peak around 9.4 Hz.” I think the peak is at 9.9 Hz.

Thanks for pointing out this error. The peak is indeed at 9.9 Hz. We have corrected it.

4. Line 297: Could you provide time-windows related to the effect of short- and long-term perceptual history?

We are thinking in the order of 10 seconds, like the “continuity field” (now added, p22).

5. Line 324: Could you develop the mentioned oscillation-based theories of working memory? What are these theories and how do they relate to your findings?

This is an excellent point! Unfortunately we do not have any firm ideas of how to relate the two in detail, but point to the similarities between the two processes: both require storage of information over a short term. We have added a paragraph on page 22 mentioning some studies showing the relationship of alpha rhythms to working memory, and some discussion on their role (excitatory or inhibitory). We also allude briefly to the connection in the introduction (p5).

6. Line 373: Could you report the age (mean+/std) of the two groups of participants?

We have now reported the gender and age of the two groups of subjects separately.

7. Line 393: What was the rationale for displaying the stimuli at 8° from the center? Do the authors think that their results would replicate with stimuli closer, i.e., below 5° in the parafovea, or further away (more in the periphery)?

The stimuli were placed at 8° to be halfway between the two fixation points, so the eccentricity was identical for pre- and post-saccadic stimuli (now mentioned on page 28). We expect the results to generalize to all eccentricities. However, the higher the eccentricity, the more serial dependence we expect.

8. Line 425. Could you justify why you restricted the analysis in the [-200; +200] ms time-window according to saccade onset?

There were very few trials with SOA less than 200 ms, as that is the typical saccadic latency. We did not want to present the stimuli before the saccadic target as it would have generated a stimulus-driven saccade. The data analysis time-window was -200 to 200 ms to ensure that most trials at each time point were included (less than 3% of trials at time points earlier than -200 ms).

9. Line 425. Could you develop your smoothing technique?

We used a moving average approach to plot smooth curves, with detailed methods provided on p29.

10. Line 434. "Linear combination of harmonics at each tested frequency". I am not quite sure to understand what the authors meant here. According to equation 1, they modelled sinusoidal waves at each frequency. What is the link with harmonics?

The term "linear combination of harmonics" here was misleading. It simply refers to combining sine and cosine components at each frequency. We have changed to "sinusoidal functions".

11. Line 450. The permutation tests are already an approach that takes into account the multiple comparison problem. Why applying another FDR correction?

At each frequency, we constructed the null hypothesis distribution by shuffling the responses over timestamps, and applying the GLM to this surrogate data. This does not compare across frequencies, and therefore requires FDR correction to control for Type I errors across multiple comparisons. In other papers we have used different ("maxstat") statistics that does include the multiple comparison within the null-hypothesis distribution, but it was not the case here.

12. Line 459. Could you justify the selection of a 25-ms time-window?

Since the alpha oscillation we report is 9-10 Hz, a duration of 25 ms corresponds to roughly one-quarter of a cycle, twice the Nyquist frequency. We now mention this.

REFERENCES

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REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

The authors have made progress in addressing several of my previous comments. I continue to believe that this manuscript has an important aim and presents promising results that are potentially relevant to the field. However, this makes it all the more crucial to ensure that the methods and results are clarified. Below, I outline my main concerns about the current state of the manuscript.

1. While I appreciate the inclusion of effect sizes in the statistical reporting, it would be useful to also report effect sizes for the main analysis of interest—namely, the GLM on the periodic modulation effects (including also the GLM coefficients, CI and R^2).

We plot the GLM coefficient (amplitude) as a function of frequency, together with the baseline noise (evaluated by permutation). The ratio of those gives a visual impression of set size. We also do a FDR-corrected t-test on all major results, complementing this main result. Unfortunately, R^2 is not appropriate in single trial, binary (0,1) data with over 10,000 trials.

2. Figure 2b remains a key result, but at least qualitatively, it does not give me confidence about the presence of a true oscillation. The red fit struggles in capturing a clear oscillatory pattern in the black running average. This is why it could help to validate this result with alternative approaches, for instance estimating spectral components at the single subject level (slight differences in the peak oscillation across subjects may obscure trends in the aggregate data) or controlling for unwanted components (I understand that the Brookshire's method might be too conservative, according to the authors' comments, but there are more recent approaches proposed to increase the sensitivity with a similar method, see Harris & Beale, 2024; also, isn't shuffling the response across trials equivalent to shuffling the delay—i.e., the time 'bin'?).

Our paper does show individual data, in panels 2d and 3d. However, we have now performed an additional FFT on the single participant data, using the suggested software pipeline, in a supplemental figure. We also show that a t-test on the individual participant vectors is significant.

Btw, Brookshire's method is not too conservative, it is too liberal!

3. It is still unclear why the authors avoided detrending or a Hanning window (which is done also in the GLM context, see Tosato et al., 2022). I thank the authors for pointing me to the additional comments and replies about the Brookshire approach, but the methodological issue seems in the end unresolved. Considering the delicate topic of rhythms in behavior, the methodological debate, and the mixed findings, I think strong justification or validation of a methodological choice is important, even simply demonstrating that the results remain consistent with standard preprocessing steps (or why it is better to avoid them).

We do not do a linear detrend because there is no linear trend in our data (which are bias, not sensitivity). There is a peak near saccadic onset that may be reduced by non-linear detrending, but we do not see any advantage in introducing unnecessary free parameters. However, the supplemental figure uses the entire pipeline of Tosato et al, including detrending, smoothing and FFT, and broadly confirms our results.

4. The analysis of current precision (Figure 6) uses only trials with orthogonal inducers, resulting in a small subset of data. I understand that this approach avoids confounding effects from serial dependence. However, since the current trial is influenced by both the 1-back and the inducer serial dependence, it is difficult to remove all effects of trial history. It might be therefore more robust to assess variability (e.g., error standard deviation) as a function of time from saccadic onset, incorporating all trials.

It is true that we cannot eliminate all serial dependence by choosing only trials with orthogonal inducers, but this does minimize the secondary effects induced by serial dependence. When we use all trials, there is an oscillation in variance: but this is to be expected, as the inducers near the test orientation will cause rhythmic biases, both clockwise and anti-clockwise, resulting in no net bias but an increase in variance. Therefore we prefer to leave the figure as is.

5. When discussing why the magnitudes of the two effects are similar despite the inducer being closer in time, the authors could mention evidence from studies showing that reported (and thus memorized, task-relevant, as the 1-back here) stimuli often produce stronger serial dependence effects.

Thanks, we now mention Whitney's work here.

Minor:

The part about efficiency remains unclear. While the authors mention studies showing that current uncertainty matters, it is unclear why this is interpreted as 'detailed evidence' of efficiency. In the context of tasks where participants are asked to discriminate each stimulus at their best, this seems more like a 'system flaw'. Thus, what is meant by efficiency and the actual evidence in support of this (or against) could be made clearer.

That was unintended, sorry: we now say "but see also REF", making it clear it is not in support. As this is not a major issue for us we do not want to digress too much.

(Page 3, Line 10)

This part is still sloppy in my reading. Serial dependence does not create a continuity field, maybe it is the other way around.

Yes, that was sloppy. We now separate the two issues.

(Page 22, Lines 17–18)

The references in these lines appear to have inconsistent font sizes.

The font size does not change, but the hyphen does generate a line break that is hard to fix. Presumably this will be fixed during printing.

In the shared dataset, are the included orientation errors (Ori_error) cleaned of outliers? There appear to be multiple cases with errors exceeding 80°.

The data set contained all data, with flags indicating which to be ignored. However, we have now removed the outliers, as it would cause confusion. Thanks you.

Reviewer #2 (Remarks to the Author):

The authors have done an excellent job revising the manuscript and they've addressed all reviewer comments.

Reviewer #3 (Remarks to the Author):

I am very pleased with the authors' revisions, which have made the paper clearer and stronger. I congratulate and thank them for their outstanding work.

I am attaching an Editorial Requests Table that details critical reporting requirements for the revised manuscript. Please attend to each item and ensure your manuscript is fully compliant. If your revised manuscript is not aligned with these requests on major issues, such as those concerning statistics, it may be returned to you for further revisions without re-review.

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We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Troby Lui

Troby Lui, PhD
Associate Editor
Communications Psychology

REVIEWER EXPERTISE:

Reviewer #1: rhythmic cognition, saccade/vision, serial dependence/prediction

Reviewer #2: saccade/vision, serial dependence/prediction

Reviewer #3: rhythmic cognition, saccade/vision