

REVIEWER 1

After reviewing all the previous comments and responses, I find myself mostly in agreement with the arguments presented. However, I have reservations about the handling of the concepts in the initial comments of review 1. Specifically, the authors' explanation of the wavelet coherence scalogram seems to conflate the terms 'delay' and 'timescale'. During their discussion of wavelet coherence scalogram, they appeared to mistakenly interchange the terms 'delay' and 'timescale'. For instance, in their response to the comments, they asserted that " ... if we see a patch of a high coherence with the 12-month delay given in the y axis it means that the pattern we observe now in the meteorological variables is correlated to the pattern that we see 12 months later in NDVI. The signals are in-phase, because e.g. exceptionally high temperatures are followed by similar pattern of exceptionally high values of NDVI. If the delay/lag was shorter, then we would see a patch of high coherence in the same place on x axis, but shifted toward smaller values on the y axis (e.g. 6-months, or at the edge of the scalogram, which would suggest that the response of the vegetation is almost immediate – this is also the case in 2018, and this is described in lines 301-304)...". In reality, the y-axis of the wavelet coherence scalogram signifies the timescale of synchronism between two signals, not the delay. The actual delay (in months) of the two signals at a specific timescale can only be determined by combining the phase difference (or phase-delay) indicated by the arrow direction (in radians) and the timescale. Upon reviewing the references mentioned in the response, namely Mbatha and Xulu (2018) and Ghaderpour et al. (2023), I found no indication attributing the y-axis of the scalogram as a delay in these papers.

In the revised manuscript, the authors argued in Lines 276-278 that "The high cohesion values indicate that the data sets exhibit high correlation in a year given on the x-axis, with a delay indicated on the y-axis." I am inclined to disagree with the assertion that the delay is depicted on the y-axis.

In these case, I suggest the manuscript should be further improved before acceptance for publication.

While I still believe that my explanation to the initial comment no. 1 is valid and justified, I agree with the Reviewer to some extent. Hence, I removed this quoted sentence (lines 277-278) from the manuscript. I also made some further adjustments, in order to fully comply with the Reviewer's comment.

REVIEWER 2

Thank you for making substantial edits to the manuscript. The addition of the new supplementary figures and the adjustment of the axis in Fig. 3 are great. I believe the changes to the text have improved the manuscript and made the presented research clearer. Overall, I have no major points left that I would like to see changed in the manuscript. I leave it up to you whether you want to include the following minor points, I believe they would further improve the manuscript.

Remaining minor points

I would suggest another potential figure adjustment, that I previously missed. It might be beneficial for the reader to have the same y-axis scaling for both NDVI and EVI in Fig. 8. This would further enhance the idea that the correlation coefficients with NDVI, especially for time lags smaller than 5 are higher than the ones for EVI.

Yes, it might be beneficial, I changed accordingly.

I still believe some of the discussion related to very different ecosystems might be confusing rather than helpful. For example, around line 420, discussion relative to very specific vegetation in Brazil. I think the manuscript would be completely fine without it.

Yes, I removed this part describing the Caatinga vegetation (lines 419-421), as it is – indeed – referring to the specific vegetation type in Brazil. However, there are still lines referring to general vegetation patterns in Brazil and South Africa (lines 381-384, and 394-395). I believe they should stay.

EDITOR'S COMMENT

The reviewers found the responses to be sufficient, especially with regard to the major comments. Both reviewers have remaining comments that I agree all should be addressed (especially about the discussion of other biomes not included in the study and the difference between phase and coherence in the wavelet analysis) before the manuscript is published.

I made all necessary changes. I hope you will find the manuscript suitable for publication now. Thank you.