

## ***Interactive comment on “On the linearity of the temperature response in Holocene: the spatial and temporal dependence” by Lingfeng Wan et al.***

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### **General Comments**

Wan et al. use the TraCE-21ka simulations in their manuscript "On the linearity of the temperature response in Holocene: the spatial and temporal dependence" to provide an initial assessment of the linearity of the climate response to various assumed forcings. They study this separately for a number of spatial and temporal scales. The idea behind the manuscript can result in a valuable contribution to our understanding of past and future climate changes, to assessing paleo-simulations, and to studying paleo-observational records. I do not have real major concerns but I think various clarifications and additional discussions are necessary before the manuscript could be

accepted for publication. These clarifications should be re-evaluated by a round of revisions and therefore I, nevertheless, recommend major revisions.

## Specific Comments

1: Could the authors please discuss more clearly, why they think that the assumptions on potential linearity hold (see also the major comments by anonymous referee 1). This discussion could also include, how the specific setup of the TraCE simulations hampers or supports the approach. It may help to include in these discussions a priori knowledge/references on forced and internal variability across temporal and spatial scales.

2: Similarly, I think it is necessary to discuss, at least shortly, how the simulations implement the various forcings and how this may influence the results.

3: Could the authors please discuss, why correlation coefficients and the linear index are appropriate measures of the linearity of the responses as studied. Could they please also clarify, which information is added by the linear index and how to interpret the index in this context.

4: Is the linearity assumption even valid for time scales where internal model processes are known to dominate. That is, we can be quite certain a priori that the decadal scale will be dominated by internal climate over the last 11k years.

5: Could the authors please stress that their conclusions really only hold for the specific setup of the TraCE simulations used.

6: More generally, I think the manuscript is missing a dedicated and thorough discussion-section.

7: Page 1 Line 25ff (P1L25): I do not think the authors show this causality conclusively.

Anyway, the SNR plots do not show or are even intended to show, according to the manuscript, why linearity is consistent between millennial and orbital scales, but why linearity is strong in these regions. That is, in my understanding, the manuscript does not support this sentence in this form.

8: P2L22ff: Could the authors please be more specific how this answer can benefit our understanding?

9: P3L3: I do not think the results by Shakun and colleagues or Marsicek and colleagues allow statements about how reasonable CCSM3's climate sensitivity is.

10: P3L7ff: I think the last part of this sentence requires a reference.

11: P3L8: I am not sure, I completely understand how the authors perform their binning.

12: I find much of the method description on page 4 unclear. For example: I am not fully clear how the authors produce their various time series.

13: P4L19: Is an AR1 process appropriate or could other models be more appropriate?

14: P4L23: Could the authors please give more details on their bootstrap. From my point of view, this description does not allow for reproduction of the significance tests.

15: a) I understand that the author's interest is only in the linearity of the response. However, considering Figure 1, I think, the manuscript will benefit if the authors also discuss the visual discrepancy between the SUM and the ALL series. A difference of about 1K between both series between 11K BP and 3K BP is a relevant feature. Indeed, even if the response is largely linear according to the correlation, the potentially smaller nonlinearity appears to be more important here.

b) To add on this, I wonder whether the setup of the simulations really allow for the analyses? However, I am not familiar enough with the setup of the TraCE "single" forcing simulations.

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16: P5L28: Could the authors discuss this complexity in more detail, please?

17: P6L5: As far as I can see the authors do not discuss the reason, at least not in depth.

18: P6L8: Could the authors please be specific, why this should be treated with caution.

19: Considering the centennial time scale: the authors diagnose that the linear response on centennial scales is poor. However, at least in the correlations in Figure 3, there appear to be many regions where linearity still is of modest importance. That is while I agree with the assessment that there is "no strong" linearity, the authors appear to dismiss linearity on centennial scales too easily on page 6.

20: P6L32: The description appears to exclude the continent of Australia.

21: P7L1: a) Could the authors please discuss later on, why the response over the continents should be different from the oceans on these very long time scales. b) Could they please also discuss what the strong internal variability over southern oceans implies for reconstruction efforts.

22: Could the remaining ice sheets and the last freshwater forcing implementations influence the results generally and specifically the poor linearity over North America?

23: P7L18: It suggests so for this set of simulations.

24: P7L29: The authors write at a number of instances "North America" but the results differ notably within North America if I interpret the visualizations correctly.

25: P7L31ff, P8L11ff, Figure 7: I am not sure whether these parts add anything to the other analyses.

26: P8L6: I appear to be unable to see the poor SNR over southern North America.

27: Figure 1: Are the linear errors really the same in panels (a) and (b)?

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## Technical Comments

T1: If I understand it correctly, the manuscript will receive language editing by Copernicus if it is accepted. Nevertheless, I think it will help further reviews if the authors check the language everywhere for clarity and grammatical correctness.

T2: Some of the Figures (particularity Figures 3 to 6) are not publication ready. While I assume that Copernicus is going to assist the authors in this if the manuscript is accepted, it probably would shorten the time between submission and final publication if the authors improve on the Figure-quality for the next round of reviews already.

T3: Could the authors please check that all Figure captions are correct. I was not sure.

T4: P4L20: I think "valid" is not the correct expression, here.

T5: P8L2: Could the authors please skip the exclamation mark.

T6: Acknowledgements: I think the authors have to acknowledge the repository or the persons which/who produced and provided the data. (I assume this was the Climate Data Gateway at NCAR.)

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-177>, 2019.

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