

Interactive comment on “Mid-Holocene regional reorganization of climate variability” by K. W. Wirtz et al.

Anonymous Referee #2

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Review : “ Mid-Holocene regional reorganization of climate variability ” by Wirtz et al.

General comments :

This paper proposes huge spectral analyses on various records covering the Holocene already published in literature. The conclusions of these analyses underline a Holocene climate with a strong regional pattern. However I see major problem in this paper too important to allowed publication in Climate of the Past : - In spite of the huge number of references, I don't think that the approach of the data is good. Indeed, the authors should have selected the data with enough resolution, good age model, etc. They should have ignored the time series that are not different from a random noise. I don't know all the data set but some of them are not structured

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and not different from a random noise. This series should be avoid because can reflect different problem in the analytical processes, sensitivity of the proxy, or reflect a misunderstanding of the proxies: - I don't think that the method used is good for this kind of analyses. Authors do not really explicit why they choose 5000 years as a boundary between different kinds of frequencies (millennial and centennial scale). I think that this choice is the major weak point of the paper. Indeed, if it is true for millennial scale (Debret et al, 2007) that 5000 is a major transition in the frequencies, what about the centennial scale? We do not have any evidence for such transition. Moreover, lots of data set do not allowed the study of 200 years cycle for example because of the age model that's why Debret et al, 2007 stop their analyses at the millennial time scale. - For this reason, I think that the wavelets analysis is the good methods to use. The effect of resampling for the analyses could be control by a lomb periodogram. Moreover Witt and Schumann, 2005 used a method of wavelets analyses that do not need the resampling and can be used for unevenly sampled series. The authors, by sharing their data around 5000 years and removing the 2000 years trend, can not see the 2500 years cycles (solar) for example. - If the authors want to draw a general pattern for the Holocene, their choice to study continental records is not judicious. Internal or local processes more likely perturb continental records (human impact, threshold effect) than more "global" records like marine records for example. Perhaps it would be interesting to distinguish continental pattern and marine/solar pattern (Debret et al, 2007) ?? - I think that the authors should look more carefully at the data set and the meaning of them. For example they use the record of Wang et al, 2005 for Dongge Cave but why they do not used Dykoski et al, 2005? This example is a good example of the problem to select the data set because wavelets or other methods do not highlights the same results for the two records both located in the same cave!!! What's the meaning of this discrepancy? - Authors explain that one part of the data are available on NCDC/NOAA or PANGEA website and other were digitalized. I think that a short test is important to see the effect of digitalizing: First, a spectral analyses on the raw data and a second on digitalized data. Perhaps this method induced a biais?

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1°) Does the paper address relevant scientific questions within the scope of CP ? Yes
2°) Does the paper present novel concepts, idea, tools or data ? Yes 3°) Are substantial conclusion reached ? need more work 4°) Are the scientific methods and assumptions valid and clearly outlined ? no I don't think that Lomb analyses is sufficient.
5°) Are the results sufficient to support the interpretations and conclusions ? No 6°) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (Traceability of the results) ? Yes 7°) Do the authors give proper credit to related work and clearly indicate their own new/original contribution ? Yes 8°) Does the title clearly reflect the contents of the paper ? Yes 9°) Does the abstract provide a concise and completely summary ? Yes 10°) Is the overall presentation well structured and clear ? Yes 11°) Is the language fluent and precise ? Yes 12°) Should any part of the paper be clarified, reduced, combined or eliminated ? Yes see above. 13°) Are the number and quality of references appropriate ? Yes

Specific comments :

p291 L17: I think that moisture and temperature are the parameters the most subject to error because of the low temperature variation during the Holocene, and the high sensitivity to the environmental changes. P291 L 22: 14C flux are coming from a model and are less accurate than 10Be. P292 L 9 : Wrong see witt and Schumann, 2005. P292 L16: The parameters are not clear for me. P292 L 24: This is the weak point explain above. P292 L 28: The starting and ending age are important for the analysis. I would recommend to start their analyses between 10 000 and 11 000 years because before the climate is highly unstable because of deglaciation. P293 Chapter 2.2: Why do you remove the 2000years running means? P294 L 3: sunspot number is not a good example because is extract from 10Be and 14C flux. P295 L 21: I think that the figure 3 is not useful. Indeed some record P298 L 5: sunspot number of ? P298 L 6-7: I think that the 208yr cycle has a more complex pattern during the Holocene however the Lomb methods do not allowed showing that. P298 L 9: Debret et al, 2007 proposed that an oceanic forcing could induce 1500yr cycle. P300 Chp 6: I

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recommend to the authors to read Brooks, 2006 if they don't know because this author made an interesting synthesis on mid-holocene climatic transition and the complexity of civilisation. (Brooks, N., (2006). Cultural responses to aridity in the middle Holocene and increased social complexity. *Quat. Int.*, 151, 29-49.) P300 L 5-6-7-8 : I'm in complete agreement with this part. But this implies that the oceanic and solar forcing were identify. P300 L 8-9 : I'm not in agreement with the conclusion of the authors, because : - According the authors there is no evidence for millennial scale cyclicity in solar activity proxies for the upper Holocene, but it comes from the detrend methods. - The conclusion are, in my opinion, based on a method not adapt to track instationarity and the records were not carefully selected. Yes there is a strong regionalism trend during the Holocene, however the mixing of various records do not permit to highlight some forcing like solar and oceanic influence already shown by Debret et al, 2007 for example with wavelets analyses.

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