

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

K. W. Wirtz et al.

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Referee 2 expressed fundamental criticism against our paper. We think that some of the points help to improve the quality of the work. A number of issues raised by the reviewer were unclear to us, some we found invalid; we give brief explanations below.

1. **ref2:** 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.

We agree on the difficulty to compare records with different aging procedures (as methodology and preciseness developed over the last years), but we disagree on filtering only "good age models". First, how is "good" defined (e.g. in terms of



calibration points)? More importantly, the ubiquitous problem in precise dating was one of the motivation to analyze temporal variability instead of, for example, synchrony of events. We have tested sensitivity against dating uncertainty by small displacements in groups of data points and did not find a remarkable effect on spectral properties.

2. **ref2:** 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 and not different from a random noise. This series should be avoided because it can reflect different problems in the analytical processes, sensitivity of the proxy, or reflect a misunderstanding of the proxies:

We disagree. If a record fulfills the above mentioned criteria, it should be included in the analysis. Any pre-selection of records that is even loosely related to later obtained results (i.e. changes in statistically significant variability) would certainly introduce a systematic bias. Some records do display random (red) noise. This is reflected by the many records lacking significant modes (blue markers in Fig.4). Contrary to the concern of the reviewer, we consider it important to show those records where significant periodic modes are absent. For the analysis of changes in periodicities, these “noisy” records do not affect the regional patterns in variability change identified by our study.

3. **ref2:** I don't think that the method used is good for this kind of analyses. Are the scientific methods and assumptions valid and clearly outlined? No, I don't think that Lomb analyses is sufficient.

The referee is repeating this judgment several times without precise arguments.

4. **ref2:** Authors do not really explicitly 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.

We did not partition at 5000, but at 6 kyr (with an overlap of ± 500 yr) (p292L24). Did the reviewer misread the number 5000 as given for the amount of realisations in the bootstrapping approach in p293L5?

Our choice of 6 ± 0.5 is motivated by changes observed around 6kyr, for example the 5.5kyr event (refs in MS) or the changed vegetation regime after 6.5 in northern Eurasia (Tarasov et al. CPD 5,127). Above all, the value is not as critical as suggested by the referee. We have systematically tested for sensitivity on division time. Less than 4% of records (i.e. 5, 4 of them in East Africa) reveal a different variability trend if the time-series are splitted at 5.5 instead of 6kyr. We will explicitly describe in the new version of the manuscript that the time of the splitting does not affect our results (except for East Africa).

5. **ref2:** 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.

To date, we are aware of only one prominent reference for centennial scale transitions (p296L25). Non-stationarity in centennial modes has apparently not been a major research theme so far. An exception is Debret et al. who did not find clear trends in centennial cycles for the North Atlantic region where also our study wasn't revealing a homogeneous picture, in contrast to other regions.

6. **ref2:** Moreover, lots of data set do not allowed the study of 200 years cycle for example because of the age model that65533;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.

Spectral range depends on the length and temporal resolution of the record,

and much less on the particular methodology (see also below) or the age model (possibly creating a dating shift, but not influencing much the temporal variability). Lomb-Scargle transformation of the Davies-et.al-2003 time-series is truncated at 260yr: this allows for detecting a weakening of the 350yr cycling towards the upper Holocene (Fig.2).

7. **ref2:** The effect of re-sampling for the analyses could be control by a lomb periodogram.

We do not understand the meaning of this sentence. The effect of re-sampling on Lomb-Scargle periodograms is shown in Fig.2.

8. **ref2:** 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.

We were aware of the Wavelet analysis (WA) of Witt and Schumann (cf. Dima-Lohmann,2009) but chose our extended Lomb-Scargle for three reasons: (1) Witt and Schumann tested (technical) applicability to unevenly spaced data in just a single case which, in addition, is rather time-homogeneous (50kyr GISP2 d18O-record). In contrast, Lomb-Scargle has been applied numerous times and, in particular, avoids "reddening" of spectra when interpolation is used (Schulz and Stattegger, 1997) (2) Maraun and Kurths (2004) showed that a careful interpretation of WA results and their significance is required for geoscientific time series including noise. (3) WA produces a large amount of output which has to be aggregated in view of rather simple focus of our study (binary trends in variability vs. intensification patterns as a function of cycling period and time) what would require further assumptions to be made. Despite (1)-(3), we do not want to exclude a validity of a WA and will clarify this point in the MS. Vice versa, pure existence of possible alternatives does not demonstrate inappropriateness of our approach.

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D. Maraun and J. Kurths, 2004. Cross Wavelet Analysis. Significance Testing and Pitfalls, *Nonlin.Proc.Geoph.* 11, 505-514.

9. **ref2:** 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.

The 2500yr cycles were left outside the focus of this study (concentrating on 200-1800 yr cyclicity) since the length of the periods of interest (two halves of the Holocene) did not allow for reliable results at regional and global scales. In the Wavelet periodograms shown by Debret et al. 2007, the 2500yr cycling is therefore mostly outside the "cone of influence".

10. **ref2:** 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)

We agree that the dominance of terrestrial records points to a weakness of our study. But, as we explained in the MS, this bias reflects the difficulty in obtaining high resolution marine time-series (due to low accumulation rates in the open ocean). The referee adds a valuable remark to the analysis since fluctuation trends might indeed vary between oceanic and atmospheric systems due to different feed-back mechanisms. We will cover this issue by a more elaborated differentiation of marine/terrestrial trends in the Discussion part.

11. **ref2:** 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 highlight the same results for the two records both located in the same cave!!!

We do not understand the connection between data and method selection. Of course, despite the high number of records we would never claim complete coverage. We are therefore grateful for further hints (Dykoski-etal) and will integrate these and other data in our future work.

12. **ref2: What's the meaning of this discrepancy?**

Context for this comment is unfortunately missing.

13. **ref2: 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 bias?**

No. In two cases where original data are available, we checked for possible effects of digitalizing and did not observe a difference. We will include this relevant information into Chap. 2.

14. **ref2: 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.**

Our choice to start at 12kyr is a compromise between an earlier and later onset of the Holocene in the Southern and Northern Hemisphere, respectively. A common start at 10kyr would weaken trend analysis independently from the method as about 50% of all records already terminate before 12kyr (Tab.2). As a consequence, few Northern Hemispheric records indeed reached to the

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- late Glacial, moderately affecting the results of the non-cyclic analysis. We will account for this effect in the MS.
15. **ref2: P293 Chapter 2.2: Why do you remove the 2000years running means?**
see above.
 16. **ref2: P294 L 3: sunspot number is not a good example because is extract from 10Be and 14C flux.**
Instead of 10Be and 14C flux we chose 10Be and SSN since the latter facilitates a discussion of solar activity forcing.
 17. **ref2: P295 L 21: I think that the figure 3 is not useful. Indeed some record.**
The relatively flat histogram shown in Fig.3 contradicts our own previous expectations and we found it instructive in the context of Holocene climate variability "modes".
The meaning of the second sentence remains unclear.
 18. **ref2: 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.**
Currently limited availability of records with resolution below 50yr does not permit a conclusive analysis of the 208yr cycle at a global scale.
 19. **ref2: P298 L 9: Debret et al, 2007 proposed that an oceanic forcing could induce 1500yr cycle.**
That is why we cited Debret-et.al.2007 for questioning the role of solar activity as

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proposed by Bond-etal.2001a.

20. **ref2:** P300 Chp 6: I 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.

We read Brooks' work before drafting the manuscript. Brooks gave an interesting review and discussed an old hypothesis. In our opinion, a link to his work would not add a novel aspect to the MS.

21. **ref2:** P300 L 5-6-7-8 : I65533;m in complete agreement with this part. But this implies that the oceanic and solar forcing were identify.

Meaning of last part unclear.

22. **ref2:** P300 L 8-9 : I65533;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.

We do not understand the intended relation between negative eigenvalues of simulated overturning loops (L8-9) and "millennial scale cyclicity in solar activity". For methods, see above

23. **ref2:** 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.

As the records are climate related proxies, they can be used to obtain information

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about climate variability. The same argument for the timing: We do not use spatio-temporal patterns for this reason, which can be applied when using similar proxies (see as an example: Rambu et al. 2003, GRL; 2004, Clim.Dyn). The interpretation of possible mechanisms is of course hindered by the heterogeneity of proxies and must therefore kept tentative. An interesting aspect is that marine and terrestrial records behave differently, which will be emphasized in the revised version of the manuscript (see above).

To wrap up, the re-examination of the Debret.etal paper which the referee advertised, would re-assert our methodological choice. Given the rich spectral behavior obtained for each record individually, a test for a simple trend (i.e. weakening/amplification of variability) requires an equally simple and robust approach. The Wavelet periodograms presented by Debret-etal seem to be compatible with our results for this region. However, we have not seen compelling evidence to repeat the study with wavelets; on the contrary and as mentioned above, significance testing against a red-noise hypothesis would be complicated or even not possible (Maraun and Kurths, 2004) because of the additional reddening of the spectra (Schulz and Statteger, 1997).

Additional note: During the compilation of the MS a reference was not linked correctly and is therefore missing. "?" in p298L5 and p321L3 should refer to S. K. Solanki, I. G. Usoskin, B. Kromer, M. Schüssler and J. Beer, 2004. Unusual activity of the Sun during recent decades compared to the previous 11,000 years. Nature 431, 1084-1087.

On behalf of all authors

Kai Wirtz

Interactive comment on Clim. Past Discuss., 5, 287, 2009.

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