

Cuernavaca, Morelos, Mexico, 22 May 2016.

#### Answers from the Author

Firstly, I would like to greatly thank the reviewers for both their favorable and unfavorable comments. Both kinds of comments have reinforced or changed my thoughts, and all of them have helped me to substantially improve my document.

NOTE: Text on different colors indicate answers (changes) motivated by reviewer 1 and reviewer 2. Green color, **bold** and underlined texts indicate last changes.

Reviewer:

**The revised version of the paper “Multi-millennial-scale solar activity and its lagged influences on climate: Empirical and tested evidence of recurrent cosmic and terrestrial patterns“ by Sanchez-Sesma is not technically sound.**

Author:

In the final version, reviewers will find a paper devoted only to solar variability. Our results about solar activity for the next centuries are comparable to those obtained by Steinhilber et al. (2013).

Reviewer:

**The answers provided by him in many cases are ambiguous, not specific to the question, and in some cases totally irrelevant. Suggestions were given to modify the text or provide more explanation for further clarifications, but that have been ignored in certain cases.**

Author:

Our results about solar activity for the next centuries are comparable to those obtained by Steinhilber et al. (2013).

Reviewer:

**The concerns raised by me in some cases have not been addressed properly but avoided and irrelevant discussion presented. For example, one concern I had was whether the Congo River Basin Surface Air Temperature was independent of oceanic influence or not. Once the author claimed that this temperature record is relatively free of ocean influence but in another context he explained how it may get affected by the THC. Instead of explaining this dichotomy the author invoked an irrelevant discussion- such as, the role of crustal movement. Though the crustal movement mediated by the tectonic processes does influence climate on million year time scales, but discussion of this effect vis-à-vis the effect of THC having a few thousand year life span is not sensible. Hence the discussion presented in Appendix C is not relevant and actually avoids the question that had been asked.**

Author:

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Reviewer:

**In another context the discussion on the intensification of the THC after the last deglaciation was sought. Though it is well known that the THC started strengthening at the termination of the last glacial age approximately 18,000 years ago (see Toggweiler and Russel Nature 2008, Vol 451) the author cited a reference of Bond et al. (2001; but not shown in the reference list) on hematite-stained grains and how it**

revealed '50% decrease' during the early Holocene. Again instead of addressing the question raised the author deviated from the issue.

Author:

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Reviewer:

Some of the analytical treatments are also questionable. For example, y-axis in Fig B2(a) shows y axis decreasing upward; though this is not uncommon but two pints are erroneously shown as '-2E+00'. Also as per the figure caption the minimum is around -2500 BC though the y-axis labeling tells it a maximum! The figure B2(b) shows the same unit for TSI and Lateral force!

Author:

Please note that a new Figure B3 was developed

Reviewer:

The idea of verification of TSI recurrent pattern of 9.5Kys using a bivalve population reconstruction is vague and simply not tenable because of a circular problem. Harzhauser et al. (2013) attempted to connect the demographic pattern of bivalve with the solar forcing. Hence solar forcing can be considered an independent variable and the bivalve population a dependable variable. Now if a functional form is devised to describe the population, the same function cannot be used to verify the independent variable; this is what exactly done by Sanchez Sesma.

Author:

Harzhauser et al. (2013) developed a comparison, in the frequency domain, of their reconstruction, BVpop, with solar activity, or TSI, (BVpop, TSI) and found significant oscillations with periods from 20 to 200 yrs. Motivated by their findings, in an independent effort, we have developed the same comparison (BVpop, TSI), but realized in the time domain over the complete 8000 yr record, with an extrapolated and adjusted TSI pattern. Thus, our comparison was in a different domain,

Reviewer:

The paper is poorly organized. The appendices are accompanied by figures and references but the same for the main text are presented at the end. An appendix usually provides supporting information or detail calculation of a formula or derivation but it does not have discussion or conclusions. If any discussion/conclusion is required that should be presented in the main text. The presentation of appendices appears to be standalone paper! Grammatical errors of the original ms though corrected to some extent but they still exist. Indiscriminate use of acronyms makes the reading exasperating. Even after a major revision the quality of this manuscript did not reach the level for peer review.

Author:

We have changed the Appendix (without discussion, and without conclusions) and relocate the references.

Reviewer:

**The scientific merit of this paper is not very significant. The author claimed in the introduction that “the main signals of climate millennia scale forcings have not been well described, neither forecasted and require further work, with new methods and new data...” The statement is not right; climate forcing on millennium timescale has been studied by several investigators. On the other hand the author did not produce any new data and the methodology he has used is not technically robust. I am afraid I cannot recommend this manuscript for publication.**

Author:

Solar forecast in centennial and millennia scale is dynamic area of research. Our results about solar activity for the next centuries are comparable to those obtained by Steinhilber et al. (2013).