

## ***Interactive comment on “Coupled climate model simulation of Holocene cooling events: solar forcing triggers oceanic feedback” by H. Renssen et al.***

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### **1 Review summary**

The paper presents a clear account of recent simulations with the earth model of intermediate complexity ECBILT-CLIO, using a new reconstruction of total solar irradiance during the Holocene. The discussion is generally well supported by appropriate referencing, and most of the questions that arise when reading the paper are addressed in the result discussion. A number of comments detailed below need to be addressed by the authors.

## 2 Comments to be addressed by the authors

### 2.1 On the solar reconstruction

1. The statement "on the decadal to centennial time scales (...) the potential for carbon cycle induced change in  $\Delta^{14}C$  is not very large" needs to be better substantiated. There is evidence from data and modelling that changes in ocean circulation modified the distribution of  $^{14}C$  between the ocean and the atmosphere. Admittedly, changes in ocean circulation during the Holocene were not as large as during the deglaciation, but it needs to be demonstrated (at least discussed) that changes in Solar magnetic activity indeed dominate the signal. It has also been shown (see, e.g., Legrande et al., 2006) that  $^{10}Be$ , which is attached to aerosols, has a varying concentration depending on atmospheric circulation. For example, in LeGrande et al., a 40 % reduction in NADW formation causes a  $^{10}Be$  concentration increase by 24 %. Is this taken into account ?

2. About the process of first removing long-term trend using a 5th-order polynomial fit, then running-averaging. The 5-th order P-fit does not guarantee that the long-period component of the signal will be entirely suppressed. Why having adopted this technique rather than a simple band- pass filter?

3. Lean et al. (2002) (see, also, Froehlich and Lean, 2004) re-estimated the Maunder Minimum TSI change with respect to today to 0.08 %, instead of 0.2 % used here. Admittedly, this is a point briefly commented on in the discussion section.

### 2.2 On the climate model

4. How well does ECBILT-CLIO capture the location and position of present-day convection sites in the North Atlantic ? Any discrepancy with observations is likely to modify the likelihood of spontaneous convection shut-off.

## 2.3 On result discussion / presentation

5. The model is not well appropriate to study tropical dynamics. Hoelzmann et al., in the DPER 6 book (same book as the Elenga reference) reviews our present knowledge of hydrological history in the Sahel Sahara region. The main message is that desertic conditions were established since 4 kyr BP, which does not support the changes in precipitation simulated in that area by the model. The Elenga reference provided by the authors is not appropriate because it discusses "Atlantic equatorial regions" (between  $-10$  and  $10^\circ$  latitude), where the model does not simulate significant changes in precipitation.

6. Figure 2b. The likelihood of large deviation is calculated with respect to the average over the 9,000 years. I recommend using a running average over 1,000 years, in order to avoid the apparent increase in likelihood that is solely due to the long-term cooling trend.

## 2.4 Editorial / minor comments

page 211, line 11: note that Shindell et al. (2001), but also Palmer et al. (2004) studied the response of the stratosphere (incl. ozone) to TSI changes

page 212 : "T21 horizontal" : too jargonic

page 214, l. 7-10 : Note that ensemble-mean reduces internal variance (assuming it is normally distributed) by a factor  $\sqrt{5}$ . It does not suppress it.

page 215, line 1 : hampers  $\rightarrow$  prevents

page 215, line 13 : note that the uncertainty on the probability is (assuming that this is a Poisson process)  $\sqrt{n}/N$ , where  $n$  is the number of events, and  $N$  the number of years on which the probability is calculated. This may be helpful in determining the

significance of the probability lows and highs given by the author (although this is a first-order indication because the Poisson process assumes that events are independent, which is obviously not the case).

page 215, line 24 : atmospheric temperature -> surface air temperature (I assume)

## References

Fröhlich, C. and J. Lean, 2004: Solar radiative output and its variability: evidence and mechanisms. *Astron. Astroph. review*, **12**, 273.

LeGrande, A. N., G. A. Schmidt, D. T. Shindell, C. V. Field, R. L. Miller, D. M. Koch, G. Faluvegi, and G. Hoffman, 2006: Consistent simulations of multiple proxy responses to an abrupt climate change event. *Proc. Natl. Acad. Sci. USA*, **103**, 837–842.

Palmer, M. A., L. J. Gray, M. R. Allen, and W. A. Norton, 2004: Solar forcing of climate: model results. *Adv. Space Res.*, **34**, 343–348.

Shindell, D. T., G. A. Schmidt, M. E. Mann, D. Rind, and A. Waple, 2001: Solar forcing of regional climate change during the Maunder minimum. *Science*, **294**, 2149–2152.

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Discussion Paper