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Climate of the Past Discussions



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

**H. Renssen et al.**

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Referee #2 (Crucifix)

We gratefully acknowledge the constructive suggestions made by Michel Crucifix

2.1 Comments on solar reconstruction

1. The referee asks us to clarify the statement that "on the decadal to centennial time scales the potential for carbon cycle induced change in delta 14C is not very large". In particular, he wonders if we have accounted for the effect of NADW reduction on the 10Be concentration. In fact we don't use 10Be to reconstruct the irradiance. Therefore, we don't have to include this potential climate influence on 10Be. In addition, as shown



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in Muscheler et al. (2004b), it is very improbable that changes in ocean ventilation caused the short-term changes in the 14C record. This argument and the agreement between 10Be and 14C make it very likely that our record is dominated by production rate changes.

2. The 2nd referee asks us why a 5th-order polynomial fit is applied instead of a band-pass filter. There is no special reason for this choice. We tried to apply a simple method to remove the long-term trend. We agree that we could also have used a band-pass filter. It is important to evaluate our TSI reconstruction with the involved uncertainties in mind. There are different methods to remove the trend, but the results are usually within the errors of the geomagnetic field uncertainties.

3. As suggested by the referee, we will mention in the revised version the 0.08% estimate of Lean et al. (2002 and Fröhlich and Lean (2004) in the last paragraph of Section 2.

2.2 Comments on climate model

4. Referee #2 asks how well our model captures the location and position of present-day convection sites. As will be shown in a new Figure 6a in the revised manuscript, deep convection in the North Atlantic occurs at two locations in our model: in the Nordic Seas south of Svalbard and in the Labrador Sea (see Figure 6a). These locations agree well with observations.

2.3 comments on discussion/presentation

5. We acknowledge the comment of referee #2 about the model not being appropriate to study tropical dynamics. We will state in the revised manuscript that it is unclear if the model and data are consistent over Northern Africa, thereby referring to the paper by Hoelzmann et al. (2004) that was mentioned by the referee.

6. Referee 2 mentions that the likelihood is calculated with respect to the average over 9000 yrs. Actually, we calculated the probability per 50-year periods using detrended



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data. The long-term cooling due to orbital forcing is already removed as discussed in the caption of Figure 2 and our method is thus very close to the one suggested by the referee (using a 1000-yr running mean to get rid of long-term cooling trend). The analysis is thus performed on a temperature record without a long-term cooling trend, but with increasing variability with progressing time. As explained by Goosse et al. (2003, Clim Dyn 20, 523-536), it can be expected to have an increase in variability in our model when the climate cools, as there is also an increase in the interaction between sea ice and deep convection (i.e. an important source for variability at these time-scales). So it is consistent with earlier work that the probability to have an extremely cold year increases with increasing variability. We prefer not to suppress this increase as in our view there is relevant information contained within it, i.e. that the probability to get events is higher in the cooler 2nd half of the Holocene.

2.4 minor comments

a) p 211. As suggested, we will add references to Shindell et al. (2001) and Palmer et al. (2004).

b) p 212. We will replace “T21 horizontal” by “T21 horizontal resolution (about 5.6 latitude x 5.6 longitude)”.

c) p 214. We will replace “suppressed” by “strongly reduced”.

d) p 215 As suggested, “hampers” will be replaced by “prevents”

e) p 215. In the revised manuscript, we will try to improve the quantitative analysis of the relation between TSI anomalies and local convection failures by including new Table 1 and a new paragraph in Section 3.1. See also our reply to comment 2 of referee #1.

f) p 215, line 24. We will insert “surface”. The sentence now reads: “In general, the global atmospheric surface temperature closely follows the TSI anomaly.”



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