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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 #3

We greatly appreciate the detailed suggestions put forward by the third referee

Section 2

1) Concerning the sensitivity to a doubled atmospheric CO_2 concentration: indeed, this sensitivity was tested with a coupled model. ECBilt-CLIO-VECODE. Furthermore, a weakening in maximum overturning stream function in the North Atlantic ocean of 15% and 30% at the time of CO_2 doubling and CO_2 quadrupling, respectively, in an experiment in which the CO_2 concentration is increased by 1 % per year (see Gregory

et al, 2005 for details). This will be clarified in the text of the revised manuscript (Section 2).

2) With regard to the results of Weber et al. (2004), it should be noted that we used a different model. Our model only has the atmospheric component ECBilt in common with the model version utilised by Weber et al. (2004), as they used a version with a flat-bottom ocean model without sea-ice dynamics at lower resolution (T21) and no dynamic vegetation component. An important difference is that in our model, the sea ice cover controls the deep convection in the Nordic Seas. As discussed in several papers (Goosse et al., 2003, Goosse and Renssen, 2004, Renssen et al. 2005c), a sea-ice expansion in the Nordic Seas associated with cooling leads to reduced deep convection in the Nordic Seas, whereas the opposite occurs in case of warming. This is fully consistent with the events described in the present paper. However, it is important to separate this regional effect in the Nordic Seas from the impact of warming/cooling on the overall overturning strength in the North Atlantic, as this is also affected by the response of deep convection in the Labrador Sea (i.e. the other major site of deep convection in our model) that could compensate for the decrease in deepwater formation in the Nordic Seas. As discussed by for instance Renssen et al. (2005c, Geophys. Res. Lett. 32, L08711), the deep convection in the Labrador Sea is much less influenced by sea ice and may show an opposite response to cooling/warming compared to the Nordic Seas. Indeed, in a global warming experiments performed with ECBilt-CLIO-VECODE (see Gregory et al., 2005) deep convection in the Nordic Seas weakens but the overall overturning rate decreases in agreement with Weber et al. (2004). We will include this discussion in the paper in Section 3.

3) The 3rd referee requests information about the calculation of the surface air temperature, specific humidity and drag coefficient. The value of the drag coefficient is prescribed. The surface air temperature is calculated using an extrapolation from the computed temperatures at 650 and 350 hPa (using a similar method but a more complex profile than in Opsteegh et al. 1998). Changes in specific humidity are calculated

following equation (8) of Opsteegh et al. (1998), which involves the total precipitable water content between the surface and 500 hPa. Above 500 hPa the atmosphere is assumed to be completely dry.

4) As suggested, we will include in Section 2 (last paragraph) other estimates for the TSI reduction during the Maunder Minimum as published by Lean et al. (2002) and Fröhlich and Lean (2004).

Section 3.1.

5) In the first paragraph in Section 3.1, we will provide a discussion of how orbital forcing affects temperature.

6) Using the present model version (ECBilt-CLIO-VECODE version 3), there is no warming trend over time with constant forcings as reported by Opsteegh et al. (1998). Goosse & Renssen (2004) have run version 2 of ECBilt-CLIO with constant forcings for 15,000 years and found no warming trend. A similar result has been obtained in multi-millennia runs with version 3 of the model.

7) As requested by the third referee, we will provide more quantitative detail about the "local temporary shutdowns of deep convection in the Nordic Seas". Several new figures are to be included with information about this subject (Fig. 5 and Fig 6a-d). See also our answer to the first point raised by referee #1.

8) We will include a figure (Fig. 6c) showing the sea-ice expansion during a cold event, together with a figure depicting the accompanying temperature anomaly.

9) In the last paragraph of Section 3.1, we will quantify how much warmer the early Holocene climate was in the Arctic as requested. In addition we will provide information on the reduction in sea-ice area.

Section 3.2

10) As suggested, we will provide in the revised manuscript more quantitative infor-

mation about the changes during the 3000-2000 BP period in Section 3.2. See 2nd paragraph and discussion of new Figures 5 and 6a-d.

11) We agree with the referee that there no visible lag between TSI and global temperature at the start of the 2700BP cooling and that it is thus unlikely that the thermal inertia of the oceans play an important role. We will therefore modify the first paragraph of Section 3.2 accordingly.

12) We will include in Section 3.2 quantitative information on a typical change in convection and how long it lasts for.

13) In Section 3.2 we will explain that a positive feedback involving vegetation is playing an important role in the precipitation decrease over Northern Africa. Over Asia, the model suggests no vegetation changes, explaining why the precipitation changes remain small here.

Section 3.3

14) The referee has strong reservations about comparison of coldest ensemble member instead of ensemble mean, as the coldest member could represent internal noise. We should point out that the climatic signal in response to the forcing (i.e. the ensemble mean) is strongly smoothed, as is for instance clearly seen in Figure 3a. The signal registered in proxy records (i.e. “reality”) can be expected to show a stronger magnitude than this ensemble mean. There might be other solar events that do not show a strong climate reaction in the proxy data. In such a case “reality” was probably in a state where it was less susceptible to solar variations. However, if there is clear evidence for a solar influence on climate (e.g. around 2700 yr BP) we should compare it with a model result with a strong climate reaction since this probably describes best what happened in the real world. Therefore, we argue in the paper that the model-data fit can be considered reasonable if the proxy-based reconstruction lies within the range suggested by the individual ensemble members. In the revised manuscript, we have chosen to use the “best-fitting” ensemble member in the discussion of Section 3.3. In

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the previous version of the paper we showed the "coldest decade" which shows the best agreement with regard to temperature. We will now use an ensemble member that shows a clear reduction in MOC in the Nordic Seas, as this member shows the best overall fit (see new Figure 5 and 6).

15) We will provide information about the statistical significance of temperature changes as suggested by the referee (in caption of new Fig 6).

16) Concerning changes in precipitation: as suggested by referee #1, we will omit the figure showing precipitation, as only the precipitation changes in Northern Africa were statistically significant. In the revised paper, we will only discuss in some detail these significant changes. Indeed, here also a clear decrease in soil moisture content is simulated as suggested by the referee.

17) As suggested, we will add a reference to Shindell et al. (1999).

Section 4

18) In the Conclusions, we will remove the sentence "while in the tropics the climate becomes drier".

Technical comments

a) Introduction, last paragraph. Delete second "rate". Acknowledged.

b) Introduction, last paragraph. As requested we will insert "potential". The sentence now reads "This has enabled us to perform transient simulations of the last 9,000 years with a coupled climate model to study the potential impact of TSI variations on the Holocene climate"

c) Section 2, last paragraph. As requested we will insert "potential". The sentence now reads "However, due to lack of precise knowledge of long-term TSI changes we think that this record is a first and reasonable step to quantitatively study the potential solar influence on climate on longer time scales."

d) Section 3.1. After comparison of different simulations with different set-ups we are certain that in our model the long-term cooling trend is caused by the orbital forcing. We therefore argue that it is not necessary to insert “probably” in this case.

e) Section 3.1, first paragraph. As suggested we will replace “are primarily” by “appear to be primarily”. The sentence is now as follows: “Decadal-to-centennial scale variations, on the other hand, appear to be primarily controlled by TSI anomalies at this time-scale.”

f) Section 3.2, first paragraph. We will clarify the temperature reduction in Figure 4a by discussing briefly the effect of the sea-ice cover over the Arctic Ocean.

g) Section 3.2, second paragraph. We will insert “probably” as requested. The sentence now reads “This is probably related to the shorter duration of this TSI anomaly and to the relatively high TSI values before and after the negative excursion.”

h) Section 3.3. The referee asks what the uncertainty is associated with temperature change in North Atlantic marine records. We understand that this uncertainty is typically in the order of 0.5-1°C, although it is not our field of expertise.

i) Section 3.3, third paragraph. We will include a reference to Figure 4b as suggested.

j) Section 4, conclusion 2. We will insert “probably” as suggested. The sentence now reads: “This is probably related to the relatively warm early Holocene climate at high northern latitudes, which is due to the relatively high orbitally-forced summer insolation at that time.”

Interactive comment on Clim. Past Discuss., 2, 209, 2006.