

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

### **Anonymous Referee #3**

Received and published: 30 May 2006

**GENERAL COMMENTS:** The authors use an ensemble of medium-resolution coupled model runs to demonstrate that TSI variations have the potential to trigger climate changes by altering the location/magnitude of NADW production. In light of the articles referenced in the paper, the topics presented here come across as an elaboration on and corroboration of findings discussed in earlier papers on related modeling studies. In this respect, Renssen et al.’s most recent results are generally interesting and useful as they describe another context for calibrating potential solar forcing during the Holocene. However I often felt that throughout the paper, results could have been discussed in greater depth and would benefit from a more rigorous quantitative treatment; many of the following comments address this issue. Indeed, the uncertainties

(acknowledged by the authors) pertaining to the reconstructed TSI record – arguably the central feature of the experiment – prescribe as specific and objective an analysis as possible. A more thorough discussion of the potential mechanisms that link the forcings with the observed climate changes would significantly strengthen the conclusions overall, and will particularly help readers to evaluate the comparisons the authors make towards the end of the article between the model results and proxy records.

**SPECIFIC COMMENTS:** Section 2. Model and Experimental Design first paragraph: the text refers to the model's sensitivity to doubled CO<sub>2</sub>, and suggests that the 2xCO<sub>2</sub> experiment was done with a coupled atmosphere-ocean model. If this is actually the case, were there any changes in NADW production in the doubled CO<sub>2</sub> experiment?

Also, in the same paragraph: Weber et al, 2004, is mentioned as another case in which the model was used to assess the impact of solar forcing on climate change. I looked briefly at this article and my impression is that Weber et al found the opposite: that increased TSI resulted in decreased meridional overturning circulation. How do you reconcile their findings with the results discussed in this paper?

Also: regarding the ECBilt model in general: it is my understanding that quasi-geostrophic models are more frequently used to look at changes in the free troposphere, and are less often used to drive a coupled ocean model. Clearly, because of computing time, it is expedient to use a simpler model in order to be able to do the long simulations needed for the experiments described here. However I would like to know a little more about some particulars relating to the model surface fluxes, since these are so important in driving ocean circulation/convection. In Opsteegh et al, 1998, section 2.2.3 includes equations that involve the surface air temperature, the specific humidity and the drag coefficient. How are these values determined?

Also concerning the TSI reconstruction: in the last paragraph of section 3.3, you mention that your estimates are probably at the upper limit of realistic estimates. It would be helpful if you could describe the current range of estimates so as to give the reader

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a better sense of context; also, do you have any plans to do similar experiments in the future to compare the a range of potential TSI forcings? This could potentially help in determining the degree of any possible nonlinear response in the model.

3.1 Global climate response to TSI variations first paragraph: please include a discussion of the mechanism for how the orbital forcing affects global temperature, in light of the results alluded to in Renssen et al 2005a. For instance, why (for each hemisphere) does temperature seem to be affected most strongly during the spring/summer for that hemisphere?

Also, is there any tendency in the model for global mean temperature to drift over time, independent of any forcing (orbital or otherwise)? Opsteegh et al, 1998, suggests that a warming trend is possible (section 5 of that paper). What is (was) the magnitude of the warming trend? Is there any evidence that this warm drift still exists in ECBilt, and if so were any efforts made to minimize it for these experiments?

second paragraph: " ... a local temporary shutdown of deep convection in the Nordic Seas": please describe in more quantitative detail the nature of these convection shutdowns: what is the percent change in streamfunction and/or the change in Sverdrups? Where, specifically, does the change take place, or to what regions is the convection change limited? How long do the shutdowns last for? etc.

Also: "These events are linked to sea-ice expansion during relatively cold phases..." Please describe more specifically how cold a relatively cold phase is, the magnitude of (and, if applicable, the mechanisms for) the sea-ice expansion, and any other ideas that would help the reader understand the character of these changes in a more quantitative manner.

last paragraph: "The early Holocene climate in the Arctic is relatively warm as a consequence of the high orbitally-forced summer insolation values..." Again, please quantify how much warmer the early Holocene Arctic (for instance, between 9000-5000 BP) is compared to later Holocene temps (4000-0 BP). I understand that these topics are

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covered in greater detail in Renssen et al, 2005a, but I think a somewhat expanded discussion here would benefit the paper. Also, as mentioned earlier, it would be helpful to provide the reader with an idea of the changes in magnitude and location of sea ice/convection changes.

3.2 The simulated climate response during 3000-2000 BP first paragraph: "In the Nordic Seas region a characteristic temperature anomaly pattern is visible ... associated with a shift in deep water formation ... " Again, please provide more quantitative information about the magnitude and character of the change in deep water formation; is there any way to show that the locations of the changes in deep water formation are the same as the areas of the surface temperature anomalies?

"After the lowest TSI values at 2700 BP, the global surface temperature and TSI diverge... This could be partly attributed to the thermal inertia of the oceans..." If this is the case, then why is there no visible lag at the start of the cooling?

"In all ensemble members a temporary local convection shutdown is simulated ..." Again, please provide some quantification of how large the change in convection is and how long it lasts for.

third paragraph: While figure 5 shows a strong drop in precipitation over Africa, it's not clear why this is the case since figure 4a shows similar temperature changes over Asia but no similar change in precip. Is there some other mechanism that could explain this apparent discrepancy in precipitation changes?

3.3 Comparison with proxy evidence for the 2800-2600 BP event in general: I have strong reservations about the authors' preference to compare observations with results from a particular ensemble member rather than consistently using the results from the whole ensemble. It is entirely possible that the colder conditions shown in figure 4c are the result of internal noise in the model; these conditions are not necessarily representative of variability in the real world. Therefore, in this section, I urge the authors to limit their comparison of the observations to the ensemble mean and refer

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to the coldest ensemble member only in passing or parenthetical comments.

second paragraph: "The increases in precipitation... in our simulation results are very small and not statistically significant." Could you also provide some information on the statistical significance of the simulated temperature changes, in light of the proxy data discussed earlier in this paragraph? Also, how do you explain the relatively small simulated precip. changes? Could they somehow be a result of the fact that model cooling is not as large as the cooling suggested by the proxy data? I think some more detailed discussion here about the reasons for the discrepancies between the temp/precip proxy data and the model results would strengthen the paper.

fourth paragraph: The article refers to proxy evidence for wet conditions over N. Hem. mid-latitude continents, however it is not clear from figure 5 that conditions over these areas are generally getting wetter (although there appear to be moderate increases in precip over China and Siberia). Similarly, while figure 5 shows precipitation decreases over Africa, it seems possible that in a cooler climate, changes in relative humidity could mean that conditions over northern Africa might not necessarily be drier even if there was less precipitation (i.e., reductions in evaporation might be comparable to reductions in precip.). In both cases, it would be helpful to complement the discussion of precipitation changes with a discussion of soil moisture changes; this will provide a more comprehensive picture of how moisture changes in the model match up with the proxy archives (particularly since proxies for wetness, such as lake levels or pollen/tree-related data, are often closely tied to changes in soil moisture and the combined effects of precip. and evap. in general).

last paragraph: I would recommend adding a reference to Shindell et al, 1999 (Science, vol 284, pp. 305-308), another paper which comprehensively addressed the topic of the dynamic effects of stratospheric ozone changes relating to TSI changes.

4. Conclusions first paragraph: " ... while in the tropics the climate becomes drier." I may be mistaken but this seems to be the first time tropical drying is mentioned; it would

be important to discuss this feature in greater detail earlier in the paper. However, looking at figure 5, it's not clear to me that any notable tropical drying is taking place (in fact the eastern tropical Pacific is wetter). Please clarify.

**TECHNICAL COMMENTS:** In general, I think the paper would benefit by acknowledging more explicitly the uncertainties inherent in the TSI reconstruction and the implications those uncertainties have for the interpretation of the model results. Most of the following comments address this concern.

1.Introduction: last paragraph: "... recent analysis of the 14C production rate rate and ice core..." Delete second "rate"

also: "This has enabled us ... to study the impact of TSI variations on the Holocene climate." I would recommend rewording this to say "study the potential impact of TSI variations" in order to acknowledge the uncertainties that relate to both the model's simulation of Holocene climate and to the TSI reconstruction.

2.Model and Experimental Design: last paragraph: change " ... a first and reasonable step to quantitatively study the solar influence on climate..." Insert "potential" before "solar influence."

3.1 Global climate response to TSI variations first paragraph: "The simulates ensemble-mean long-term cooling trend in annual global surface temperature ... is associated with the orbital forcing." Insert "probably" between "is" and "associated."

Similarly, later in the same paragraph: "Decadal-to-centennial scale variations, on the other hand, are primarily controlled ..." Replace "are primarily" with "appear to be primarily." Also, it might be better to move this sentence to the start of the next paragraph, since this sentence relates to the effects of the TSI variations and how they affect climate (which is the focus of the second paragraph).

3.2 The simulated climate response during 3000-2000 BP first paragraph: the cooling over the N. Hem. mid-latitude continents does appear to be larger than the cooling over

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the ocean, but figure 4a indicates that the strongest cooling takes place in the Arctic. Please clarify.

second paragraph: "This is related to the shorter duration of this TSI anomaly ..." Insert "probably" between "is" and "related."

3.3 Comparison with proxy evidence for the 2800-2600 BP event second paragraph: "North Atlantic marine records show a marked surface cooling ... where the anomaly reached -1.5 C ..." What is the uncertainty associated with this temp. change estimate?

third paragraph: seems like you are referring to the warming near Iceland shown in figure 4b; please clarify in the text.

4. Conclusions second paragraph: "This is related to the relatively warm early Holocene..." Insert "probably" between "is" and "related."

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