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Climate of the Past Discussions

Interactive comment on "Information on the early Holocene climate constrains the summer sea ice projections for the 21st century" *by* H. Goosse et al.

H. Goosse et al.

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We would like first to thank the reviewer for his positive remarks about the interest of the subject.

As underlined by the Referee, we have entirely focused our discussion on the summer Arctic sea ice extent. However, we do not consider that this limits the value of our work. When evaluating different models or different parameter sets, one possibility is to perform simulations over a wide range of conditions: the Last Glacial Maximum, the deglaciation, the Holocene, the last millennium, the last 150 years ... In those simulations, a large number of variables can be compared with observations and proxy-data: temperatures, ice concentration, precipitation, ... On the basis of this comparison, we

can estimate the general model quality and then choose the model version(s) or the parameter set(s) that provides the best results. The big advantage of this technique is that the result of this procedure could be used for various applications as the models have been tested on very different configurations and a lot of variables have been analysed. To our point of view, the disadvantages are the following: first, one cannot easily estimate the interest of a particular model-comparison (is the model-data comparison for the LGM interesting for my scientific question or not?) without making additional tests (for instance estimating the model quality not using this model-data comparison or only this one, ...). Secondly, one cannot ensure that a model that is good in average is also good for a particular scientific point that has to be tested with the model in a more focused study.

We have thus followed an alternative approach, which is perfectly complementary to the one described above. We have here a specific research question: what type of test or data would allow a reduction of the uncertainty in the projection of the summer sea ice cover changes in the Arctic? We propose here that this could be done by comparing model results with adequate proxy-based reconstructions of the ice extent for the early Holocene. We have then shown a clear link between the model-data comparison and the reduction of the uncertainty. Other variables could have been analysed of course. As mentioned by the Referee, analyzing some LGM simulations is clearly out of the scope of the present paper. For the winter sea ice change, in the revised version of the paper, we show that analyzing the early Holocene is not very instructive for our model. The same conclusion could be drawn for the sea ice in the Southern Ocean. The different parameter provides different simulations of the ice extent in the Southern Ocean for the early Holocene, the pre-industrial area, the last decades or the mid-21st century (figures available at http://www.astr.ucl.ac.be/users/hgs/Figures-Sud.pdf). However, the differences are not very large and no clear conclusion could be gained from the model-data comparison. In addition, there is no guarantee that a model that is behaving well in the Southern Ocean would have a reasonable behaviour in the Arctic as the processes governing the evolution of the ice cover are clearly different between

CPD
3, S691–S695, 2007

EGU

the hemispheres, in particular regarding the role of the oceanic circulation. Similarly, the surface air temperature changes in summer over the Arctic Ocean are relatively small in the Arctic (compared to the ones in winter) as the temperature remains close to the freezing point of water because of the presence of ice and cold waters. The model-data comparison of summer temperatures is thus not very instructive, while for winter the link with the summer melt appears less straightforward. Instead of showing all those relatively unsuccessful model-data comparison that could have bored the reader and dilute our conclusions, we have thus prefer to focus the paper on a interesting and useful model-data comparison (i.e. for the summer ice extent). We however include in the revised version of the paper a brief comparison for the winter ice cover to show that some data are most useful than other to constrain model results.

We agree that we only include 5 parameter sets and that additional experiments are required. This is clearly mentioned in the conclusion section of the revised paper. The Referee considerers that rejecting only one of the parameter sets on the basis of the model-data comparison for the early Holocene is not a lot. We disagree as, to our point of view, this is a clear indication that Holocene data provide a very useful constraint on the sensitivity of the model in the Arctic. The great interest is that this constraint is complementary to the one provided by the model-data comparison over the last 50 years. On the basis of recent data, experiment E5 could indeed not be rejected. It could even appear as the most realistic one. However, the available proxy-data allow us to consider that this parameter set is not realistic for the early Holocene. We admit that the model-data comparison for the early Holocene is qualitative. However, Referee 2, a well-known specialist of proxy-based reconstruction of the sea ice over the last 20 000 years, is agreeing with on our conclusion and also considers that E5 is not realistic for the early Holocene on the basis of available proxy evidence. The early Holocene data provide thus an upper limit to the simulated summer sea ice response to a radiative perturbation while the recent past provides a lower limit. More information on the early Holocene ice cover would thus help us to narrow the plausible range of response. We consider that it is a strong result of our study. We have modified the abstract and the

CPD
3, S691–S695, 2007

EGU

conclusion section of the revised version of the paper to make this point clearer.

In figures 3 and 5, the information from the trend is indeed difficult to estimate. It is the reason why we have given the corresponding numbers in table 2. If we had plotted the data since 1953, the trend would appear clearer in the figure. Unfortunately, those data were not available to us. We were thus only able to write the resulting trend in table 2, derived from the results mentioned in Stroeve et al. (2007). Simulation E1, E2 and E3 were eliminated on the basis of the table not because of the visual inspection of figure 3. The elimination of E3 might indeed appear a bit too strong in this framework. The revised version of the paper thus moderates our conclusion about E3, as proposed by the Reviewer.

We have indeed tried to build an ensemble of parameter sets that give relatively similar results for the pre-industrial period rather than for the last decades. As guessed by the Referee, this is mainly because testing different parameter sets in a transient simulation is much more difficult. Furthermore, as uncertainties exist on the forcing for the last decades, one could not guarantee that, if different parameter sets give similar results using one choice of the forcing (for instance the magnitude of the aerosol forcing), they will give the same answer for a different forcing. However, for present-day condition, one cannot state that any of the simulation is clearly better or worse than the others. A bias is possible because of this different base state but we do not believe that this has a strong impact on our results. Additional experiments using other parameters (and other base states) would be useful to test the robustness of our conclusions. This is mentioned in the revised conclusion.

The simulations for the early Holocene to preindustrial times were not particularly required (except that it provides a very luxurious set up procedure for the runs over the last 150 years). However, those simulations were available to us and we thus use them instead of launching new ones. Furthermore, those transient simulations allowed us to determine that the difference between the various parameter sets was the largest at 8ky BP (and thus the interesting signal the strongest for this period). In particular,

CPD
3, S691–S695, 2007

EGU

the signal was much stronger for 8kyr BP than for the more classical 6kyr BP. This is underlined in the conclusion of the revised version.

We have taken into account the editorial comments of the Reviewer.

We have added a data estimate of the minimum ice extent over the period 1980-2000 on table 2. The size of the figure 3 should indeed be larger in the revised version of the manuscript.

Interactive comment on Clim. Past Discuss., 3, 999, 2007.