Clim. Past Discuss., 8, C3329–C3341, 2013
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Climate of the Past Discussions

CPD

8, C3329–C3341, 2013

Interactive Comment

Interactive comment on "Modelling Northern Hemisphere ice sheets distribution during MIS5 and MIS7 glacial inceptions" *by* F. Colleoni et al.

Anonymous Referee #2

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The paper *Modelling Northern Hemisphere ice sheets distribution during MIS5 and MIS7 glacial inceptions* by Colleoni et al. addresses the challenge of modeling a glacial inception and compares the last two glacial inceptions in stand-alone simulations with a low-resolution setup of the AOGCM CESM1.0 and with the ice sheet model (ISM) GRISLI. As the authors correctly state, modeling the glacial inception is a very interesting problem and a good test for models.

There are coupled AOGCM-ISM studies, e. g. by Herrington and Poulsen (2012) or Gregory et al. (2012) that address the glacial inception, while in this study time slice experiments from the AOGCM are used to force the ISM, but the ice sheets are not fed back into the AOGCM. A novelty in this study compared to the two studies listed above is the comparison of the last two glacial inceptions (MIS5 and MIS7). Comparing the

MIS5 and MIS7 inceptions in ice sheet and climate model simulations definitely is scientifically very interesting, and clearly matches the scope of Climate of the Past. For some of the simulations, a surface air temperature anomaly is used to drive the model. This temperature anomaly is constructed from a reconstructed Greenland temperature anomaly by Quiquet (2012) and a split of the glacial climate changes into different components by Köhler et al. (2010). Using the split of the temperature anomaly into different components and applying the components seperately as climate forcing is a promising approach. Hovever, I have serious doubts about the way this forcing is applied to the ISM. These doubts might be solved with a proper description of the methodology. So far, I am this remains somewhat unclear because of an inaccurate description and errors in the relevant equations. The basic sectioning of the text is largely appropriate, within the sections, it is less clear in which order the different aspects are discussed and I had to invest substantial effort following the comparison of the different experiments. The figures need some modifications but display most of the relevant variables. All in all, the paper is a valuable contribution to studying glacial inceptions but it needs **major revisions** before being accepted for publication.

For each of the two glacial inceptions, the authors analyze two time slices in AOGCM experiments. They use the climate states obtained in these four simulations to drive steady-state simulations of the ISM. In addition, within each of the inceptions, they connect the two time slices with a transient ISM simulation, that is forced by a combination of the two climate states and a temperature anomaly. They compare the modeled ice sheets at the end of the steady-state experiments and at the end of the transient experiments and compare the evolution of the ice sheet volumes during the transient experiments with sea level reconstructions.

The main conclusions from the experiments are as follows:

1. The pre-inception ice sheets are represented reasonably well in the steady-state experiments, while the inception ice volumes are not captured well in the steady-

CPD

8, C3329–C3341, 2013

Interactive Comment

state simulations.

2. With additional cooling (to represent the cooling effect of growing ice sheets) prescribed from a Greenland temperature reconstruction, glacial inception is reached in the ice sheet model in one of two experiments.

3. In the colder MIS7 interglacial, the climate model cold bias over the northern hemisphere does prevent the ice sheets from reaching their desired volume while for the MIS5 inception, this does not matter too much.

I see conclusion (3) as only partly supported by the model results. The main measure for the distinction between success and failure of the MIS5 and MIS7 inceptions is the ice volume. In the *successfull* MIS5 inception, a substantial fraction of the ice grows in the Bering Strait area, where no ice sheet should grow. It is therefore not obvious why this ice volume should be considered for a comparison with the sea level reconstructions. For MIS7, the sea level reconstructions differ greatly (at some times by 60 m). Therefore comparing to these reconstructions is of limited use.

In the following I will detail on my doubts about the way the climate forcing for the ice sheet model is constructed. **Since this is essential for the model results, clarification is unavoidable here.** For the steady-state experiments, the forcing seems to be rather standard, while for the transient experiment different methods are combined. In section 2.2.1 (page 6230) the authors provide equations for the application of temperature and precipitation anomalies. These equations are inconsistent. Equation 1 states

$$T_{rec} = T_{S1} + \frac{T_{S2} - T_{S1}}{dt} - \lambda(S - S_{0K})$$

Line 6 states that dt is the integration timestep of GRISLI. This implies the authors are adding a temperature (T_{S1}, first term after the "$=$") and a rate of change of temperature $\frac{T_{S2}-T_{S1}}{dt}$. In the following I will assume this was meant to be a linear interpolation between the two temperatures and is a mishap (well beyond the level of a typo, though) in writing the equation. Similarly, I will read $\frac{\Delta T_{index}}{dt}$ in equation (2) as $\Delta T_{index}(t)$. Adding the temperature index and the linear interpolation between both climate states results in a double correction of the temperatures, especially close to the S2 time slice. Could you please comment on this? Similarly, the precipitation seems to be double-corrected in equation (3). Staying with the precipitation, but jumping into the results chapter, on page 6240 the authors write “If the temperature field becomes more negative during the experiment, the precipitation is increased to compensate for the fact that the climate might become dryer during the transient experiment and to allow for ice accumulation in region initially very dry.”. Please include a detailed description of this treatment in the section on the experiment setup. What are the consequences of this treatment for the model results? How does it affect the comparability of the steady-state and the transient simulations?

General comments

The paper needs work on the use of the English language and the structure should be made more transparent throughout the text, especially a separation between Discussion and Conclusion would help to display the main results of this work more clearly. I will not list the spelling and grammar errors in detail throughout the entire document, but just list the errors I found on 6240 as a sample:
line 2: “not the case -of- *for*”
line 11: “region*s that* initially *are* very dry”
line 13: “base don”
lines 24, 25: “either (...) and” – *either* goes with *or*, not with *and*
lines 25, 26: “as**,** for example***,*** *the* vegetation feedback”
I suggest to also change the following wordings:
line 7: “ice simulations” → “ice *sheet model* simulations”
line 18: “the matching is good” → “matches the sea level reconstructions well”

CPD
8, C3329–C3341, 2013

One major problem is the weak AMOC in the low-resolution setup of CESM and the associated temperature and precipitation bias over most of the (sub-)polar regions (fig. 6). It remains open how well the inception over Eurasia can actually be studied in these experiments, and over northern America there are serious problems, too. It might be a necessary to largely exclude Eurasia from further analysis because of this bias. While the temperature as well as the precipitation bias are serious, they do, to some degree, counteract.

A verification run of the of the ice sheet model with the pre-industrial forcing would be crucial for model validation. In the case that you cannot perform a full steady-state run, could you please comment on the response of the ISM to pre-industrial forcing over something like 10 kyrs and compare it to the first 10 kyrs under the MIS5 and MIS7 conditions?

Considering that the authors aim at analyzing the role of feedbacks for inception, it is surprising that in the introduction (page 6226, lines 21–25) they state that "CESM 1.0 provides the possibility of computing dynamical vegetation and dynamical aeolian dust distribution, but we turned off those options and prescribed the pre-industrial vegetation cover and dust distribution over continents in all our experiments since in this work we focus only on the impact of external forcing on the Earth's climate.". Why are feedbacks that control the response of the climate system to changes in orbital forcings deactivated?

For each of the previous two glacial inceptions (MIS5 and MIS7), the authors analyze two time slices. For MIS7, they choose two time slices in the cooling phase with falling sea levels, while for MIS5 they choose one time slice rather early in the interglacial (judging by the development of the sea level and by the Greenland temperature index used later in the work, that shows a strong warming between 125 kyrs BP and 120 kyrs BP, figures 1 and 3), and one later during the inception. The difference between the

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Interactive Discussion
Discussion Paper

choices of the time slices obstructs a comparison of the model results.

The global application of a Greenland temperature index seems somewhat troublesome. Climate models are used exactly because the climate does not change uniformly. During the LGM, Greenland shows extremely strong changes in temperature (e. g. Kim et al., 2008) when compared with other regions, especially considering that the surface elevation in Greenland remains largely unchanged. The concern that this temperature index might lead to too extreme results is mentioned by the authors in the results chapter (page 6240 lines 12–13) and is supported by the complete destruction of the Greenland Ice Sheet during the transient simulations of MIS5. In reality, parts of the ice sheet did survive this time period.

Specific comments

Introduction

Please to include the inception studies by Herrington and Poulsen (2012) and Gregory et al. (2012) here and detail on the additional value of your work. You might also want to mention Ganopolski et al. (2010); Ganopolski and Calov (2011) as examples for 3D ISM-EMIC simulations. Please clearly state that Born et al. (2010) also force an ISM with GCM output for 115 kyrs BP and study the glacial inception.

Methods

On page 6226 lines 6–8 the authors correctly refer to Calov et al. (2009) for a study with a comparison of asynchronous and synchronous coupling. However, the context with this study that uses neither of the two methods remains unclear.

Page 6226, lines 10–14: The use of Greenland ice cores for modulating temperatures goes back beyond Charbit et al. (2002). See Abe-Ouchi et al. (2007) for some references.

CPD

8, C3329–C3341, 2013

Interactive Comment

CESM

On Page 6227, lines 21–24 the authors state that the climate snapshots for the early MIS setups were chosen at the sea level highstands. For MIS5, this clearly conflicts with figure 1 and the model results.

GRISLI

Could you please describe how the ice sheets interact with the ocean? A few words on calving and shelf basal melt would be nice, since the parametrizations and thresholds used here can vary strongly between different setups and have a large influence on the results.

On page 6229 line 23 the authors state "The climate memory of ice sheet is about 5 kyr to 10 kyr". This is a very low estimate considering the 150 kyrs the authors invested into the steady-state simulations and the results of Rogozhina et al. (2011).

Results - CESM

On page 6233 lines 23–24 the authors state that "In all the experiments, the Northern Hemisphere mean annual precipitation is reduced by more than 20% with respect to CTR1850" Please distinguish more clearly between local changes and a large-scale mean. Similarly, the "thickening of the sea-ice cover (of about 2 m)" needs a specification of the region affected, especially since here no plots are provided. Please provide more detailed information on the sea ice extent and concentration. Sea ice is crucial for the climate around the Arctic Ocean. The way it is written in the paper, it appears as if the higher albedo of the thicker sea-ice would directly decrease the precipitation. Please rephrase.

page 6235 lines 4–9: The authors conclude from a comparison of the central Greenland temperature change with the Greenland temperature reconstruction by Quiquet (2012) that their model captures the desired effects on NH scale reasonably well and that the temperature and precipitation anomalies do not affect this. This is a far-reaching conclusion from comparing one temperature point and a comparison with other climate simulations for 115 ka (e. g. the already cited simulations by Jochum et al. (2012) with a high-resolution version of the same climate model) would help adding credibility to this statement.

Steady-state ice-sheets experiments

As stated above, the steady-state results would greatly benefit from a comparison with a steady-state experiment with pre-industrial forcing. This would also help sorting out errors from the climate model bias.

page 6236–6237: In the comparison of the steady-state sea level equivalents with reconstructions for the transient evolution during the interglacial and especially during the inception, it should be clearly stated that one should not necessarily expect these to match, since the ice sheets have much more time to grow/melt during a steady-state simulation, than in reality.

While the cold bias in CESM results in less precipitation, the low temperatures should foster ice growth, especially in steady-state simulations, where the ice sheets have time to grow. Please comment on (and plot) the time evolution of the modeled ice sheet volumes in the steady-state experiments.

page 6237 lines 20–21: why do the simulations only account "to some extent, to the effect of the sea-ice albedo"?

On page 6238, lines 9–11 the authors write "However, the already cold climate context minimizes the impact of the land-albedo feedbacks that would more quickly cool the climate over the inception areas." Please explain.

Transient ice-sheets experiments

As stated above, I have serious concerns regarding the method used to construct these simulations. I will nonetheless provide comments on the presented results.

CPD
8, C3329–C3341, 2013

Please provide more information on the fate of the Greenland Ice Sheet. It should definitely be included in the ice volume plots in figure 10. The final shape of the Greenland Ice Sheet in the MIS5 experiments is very peculiar. Please provide more information on the evolution of the northern part. Why are the rims of the ice sheet so much thicker than the interior? Could you please plot the ice sheets at their minimum in the MIS5 experiments?

On page 6239 lines 25–29, the authors write that they would assume the ice sheets at the end of the transient run to match those of the steady-state run. This could be expected if, and only if, the ice sheets were in constant equilibrium with the climate and had no hysteresis behavior. I do not see why either of the two conditions would be fulfilled. Furthermore, as stated above, it seems as if the forcing were different because of the addition of the time series.

What do the error bars in the modeled ice sheet volume time series in figure 8 indicate? Why is this time series not continuous?

The authors force the ice sheet models with different temperature anomaly time series. A comparison of the modeled evolution of ice area with the forcing might help explaining the differences in the evolution of the ice covered area between the different experiments.

Discussion and Conclusion
As stated above, please separate this more clearly into a discussion and a conclusion.

Technical corrections

The Barent**s** Sea has an s at the end of the first word. 3 out of 4 occurrences are misspelled.

Köhler is spelled with an ö instead of an o.

Title

It should probably be “ice sheet distribution” instead of “ice sheet*s* distribution”. Please check with a native speaker, if you have not already done so.

CESM experiments setup

On page 6227 line 25 “five experiments” should be replaced by “six experiments”.

Ice-sheet experiments results
The equation for sea level change (7) should be a sum over the whole model grid.

Figures and tables:

Please use a shared color scale for plots that have the same color scale (e.g. the top row in figure 4). Also make sure, all levels are clearly labeled on non-linear color scales.

On the temperature scales in fig 4 and 6, the degree-sign is broken.

Figure 3: Please flip the time axis. Time running from left to right is more conventional in modeling.

Table 3: Please add units as in tables 1 and 2.

Figure 5: Please use a wider color scale. One suggestion would be: < 1 day ice-free, < 1 week ice free, < 1 month ice free, < 1 season ice free, < 2 seasons ice-free, > 3 seasons ice-free, or so (please experiment with different values) This would allow to distinguish between regions, where practical perennial ice cover is reached and those that are far away from this state. Presently, the plot is practically binary. Please also make sure, that you also plot snow cover on sea ice. Can you explain the snow cover zone reaching south from Greenland to Baffin Island at about 65 to 70°W? Please also add figures for your control runs.

Figure 6: The label should read “Bias in temperature an**d** precipitation” (a “d” is missing), substitute “as evidenced by the NCEP reanalysis” by “compared to the NCEP reanalysis” (or something similar). Please label the left end of your color scale. As it is

exceeded, the value is of interest.

Figure 7: “each of the steady-state experiment**s**” needs an “s” at the end.

Figure 8: please flip the time axis. Why do the ice sheet volumes have error bars? Could you please plot them as continuous time series?

Figure 10: Please flip the time axis. Please add lines for the Greenland ice sheet. Please add an extra line for the Bering Strait ice sheet.

Citations:

Charbit et al. (2002) is QSR vol. 21, not QSR vol. 23

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CPD

8, C3329–C3341, 2013

Interactive Comment

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CPD
8, C3329–C3341, 2013