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Interactive comment on "Deglacial ice–sheet meltdown: orbital pacemaking and CO_2 effects" *by* M. Heinemann et al.

M. Heinemann et al.

malteh@hawaii.edu

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Thank you very much for your constructive comments.

1 General comments

As mentioned in the response to Andrey Ganopolski's comments, your main points are well taken.

Below, and in the revised version of the paper, we will describe in more detail how the coupling between the ice sheet and climate model is achieved to allow reproduction of the results. In particular, we will describe the PDD scheme that was used, and also discuss the possible caveats of using the PDD scheme, rather than using a more physical surface mass balance approach. We will also discuss other limitations and

possible caveats, such as the lack of hydrological feedbacks in response to ice sheet changes, and the effect of the applied acceleration technique.

2 Major comments

2.1 Reproducibility

a) Downscaling, fields exchanged. Why forest fraction? Coupling steps? Albedo exchange? Ice-sheet mask for ECBilt?

Precipitation from ECBilt on the T21 grid is corrected for the present-day temperature bias after each LOVECLIM "chunk", and then bi-linearly interpolated onto the 1x1 ice sheet model grid. Temperature is bi-linearly interpolated onto ice sheet model grid (without bias-correction), and subsequently corrected for deviations of the ice sheet surface height from the low-resolution / T21 ECBilt surface height, assuming a spatially uniform lapse rate of 0.005°C/m. I.e., the fields passed from LOVECLIM to IcIES are the 2m air temperature (bias-corrected), precipitation, and the reference T21 orography.

After each IcIES chunk, the surface albedo, forest fraction, and orography in LOVECLIM are updated according to the new ice sheet surface height and thickness. The height and thickness fields are computed from the last timestep of each IcIES "chunk". The T21 grid points in LOVECLIM are either ice covered, or not. We do not apply fractional ice coverage. A cell is defined as ice covered, if more than 50% of the cell are covered by at least 10m thick ice (again using bi-linear interpolation). This is our ice mask. If a LOVECLIM grid cell is ice-covered, all the trees are removed (forest fraction set to 0), and the surface "background" albedo is set to an ice albedo of 0.4. The relatively low albedo is more typical for melting ice. But the low value should not affect the surface albedo during the glacial or inception very much, because most of the ice-covered LOVECLIM grid cells are covered by snow, which overlays the surface "background" albedo of the ice. The forest fraction in LOVECLIM is updated according to the ice coverage, because there are no trees on an ice sheet. Otherwise the forest fraction would affect the computation of the surface albedo over ice.

For the runs presented here, each LOVECLIM "chunk" is 50 years long. Within those 50 years, the orbital and greenhouse gas forcing varies as if those 50 years were 1000 years. The precipitation and temperature climatologies that are passed to IcIES thereafter are based on the entire 50-year-period. Then, IcIES is run for 1000 years with that climatological forcing. In other words, the coupling timestep is 1000 years, with an acceleration factor of 20.

b) PDD scheme, values of PDD parameters.

The PDD scheme used here is based on Reeh (1991), the "standard formulation" in the comparison by Charbit et al. (2013). The PDD factor for snow melting is set to 3 mm day^{-1}K^{-1} water equivalent, and that for ice melting is set 8 mm day^{-1}K^{-1}. It is assumed that 60% of the melted snow re-freezes onto the ice. The standard deviation of the daily temperature is assumed to be 5.5 K, which is slightly larger than the "standard" value of 5 K used in Charbit et al. (2013).

c) Climate model setup. Glacial, present-day, or transient land-sea mask? Bering Strait?

We are using the LGM setup of LOVECLIM, based on Roche et al. (2007). The land-sea mask in ECBilt, and the CLIO bathymetry are fixed according to LGM sea level. The Bering Strait is closed. And the CLIO parameterisation of the Bering Strait transport (Goosse et al. 1997) is not used.

d) What is the performance of the model for the obtained pre-industrial at the end of the simulation? It would be very interesting to add the Surface Mass Balance on the ice-sheet grid for the end of the run (and for the LGM for future intercomparison with different groups). How that SMB for the end of the run compare to Present-Day?

The simulated present-day Greenland ice sheet does not look that bad, considering the low-resolution atmosphere model above. The Greenland ice sheet is too thick

compared to Bamber et al. (2001) (see Fig. 1d of this author response). The surface mass balance in iLove, especially over northern central Greenland is overestimated compared to higher-resolution model results (Ettema et al. 2009, Fig. 1b-c). Only some of the ablation zones in the coastal areas are captured in iLove.

For comparison, the LGM surface mass balance is shown in Fig. R2a.

e) How are bias correction done? Anomaly approach? North American temperature specifically corrected?

Only the temperature is bias corrected, not the precipitation. The annual mean temperature bias is computed from the years 1961-1990 of a transient LOVECLIM simulation with prescribed greenhouse gas concentrations, compared to 2m air temperature and SST observations for the same period (Jones and Moberg 2003). This present-day annual mean temperature bias is subtracted from the ECBilt temperature field after every LOVECLIM chunk, before it is passed on to IcIES. This means that the ice sheet model is driven with the temperature anomalies relative to Jones and Moberg (2003) with respect to the annual mean temperatures. The annual cycle of the simulated temperature is not altered by the bias correction. The bias correction is applied over the entire ice sheet domain, North America is no exception. No other correction was applied in North America, the formulation in the manuscript was misleading, and we will modify the paragraph to clarify this issue.

f) LOVECLIM was coupled to at least one other ice-sheet model before. How is your setup comparable to the one from Huybrechts' group? How are your results also comparable or not to theirs? (cf. Huybrechts, Quaternary Sci. Rev., 21, 203-231, 2002.).

To our knowledge, LOVECLIM has been coupled to a model for the Antarctic and Greenland ice sheets named AGISM (Goosse et al. 2010, Huybrechts et al. 2011, Goelzer et al. 2012), to the ice sheet model GRISLI (Roche et al. 2013), and by O. Elison Timm to Glimmer.

Huybrechts (2002) reconstructed Greenland and Antarctic ice sheet variations during the LGM and during the last four glacial cycles using AGISM. However, in that study, AGISM was not coupled to LOVECLIM. Instead, the ice sheets were forced by temperature and precipitation fields, which were based on present-day observations, plus anomalies estimated from GRIP and Vostoc ice core records. Since our setup only includes interactive Northern Hemisphere ice sheets, we cannot compare our results to Huybrechts' results for the Antarctic ice sheet. In our model, the Greenland ice sheet contributes to the deglacial sea level rise by about 3.5 m (the Greenland ice volume drops from about 14.5 to 11 m sle between 15 and 7 ka BP), which is only slightly more than in Huybrechts (2002). The North American and Eurasian ice sheets were not simulated in Huybrechts (2002).

g) on albedo again. Do you account for snow and ice albedo? If yes how different are they and how is the computation made?

Yes, we do account for snow and ice albedo. The albedo of the bare ice sheet is set to 0.4. For example during the LGM, the land-ice-covered areas in ECBilt are almost entirely covered by 10m thick snow (water equivalent), which is the maximum allowed snow depth in LOVECLIM (see response to Andrey Ganopolski's comment 3.3). The snow albedo is set to 0.85 poleward of 72°N/S, and 0.8 elsewhere. The surface albedo in the ice-covered areas is set to the ice albedo, if there is no snow. It is set to the snow albedo, if the surface is covered by at least 0.05 m of snow, and the albedo is linearly interpolated between the ice- and snow-albedo for snow thicknesses between 0 m and 0.05 m (all in water equivalent).

h) On the α coefficient. Could you indicate in more details how the value was chosen with respect to the present-day climate? (if done as such ...).

We used the α coefficient as a tuning parameter to find a good fit between the evolution of the ice volume, and the reconstructed sea-level change. The α that yields the best fit might of course not be unique. If we, for example, included a parameterisation for

Aeolian dust deposition changes, a perhaps lower value of α could yield similar results.

2.2 Additional Discussions

Two aspects due to the setup of the model needs to be discussed in much more details

a) Discussion on the lack of freshwater exchange.

If the freshwater forcing was accounted for, it would affect the thermohaline ocean circulation, and hence the meridional ocean heat transport.

In the runs presented here, the deep ocean circulation remains qualitatively similar to the present-day circulation, with deep water formation in the North Atlantic and Southern Ocean, and an Atlantic Meridional Overturning Circulation (AMOC) of about 20-30 Sv. The AMOC is slightly weakened in the control run during the deglaciation, which might in part be due to the melting of snow in LOVECLIM. This AMOC weakening lasts for about 10,000 years in the accelerated control run. However, the 10,000 years are equivalent to 500 years for the ocean model. Hence, if no acceleration technique was applied, and if CLIO would see the same amount of freshwater within 10,000 years, the effect on the AMOC would likely be much smaller. However, the amount of freshwater due to melting ice during the deglaciation is of course much larger than the amount of melting snow (by a factor on the order of 100; given that in most ice-covered areas the maximum snow depth in LOVECLIM of 10m water equivalent is reached during the LGM, compared to ice thicknesses on the order of 1 km). Hence, we would expect a large effect on the ocean circulation in not-accelerated runs with the complete freshwater forcing.

Reconstructions of the freshwater forcing during the deglaciation (e.g., Tarasov and Peltier 2006), reconstructions of deglacial AMOC changes (e.g., McManus et al. 2004) and atmosphere-ocean model studies accounting for liquid freshwater (Liu et al. 2009, Roche et al. 2009) and iceberg release (Jongma et al. 2013) indicated that the disintegration of the ice sheets caused a weakening of the AMOC, and hence a reduction of the northward ocean heat transport in the Atlantic. The cooling due to the reduced heat transport may have caused a slow-down of the deglaciation. The reduced heat transport in the Atlantic may have been compensated by an intensified northward heat transport in the Pacific (Okazaki et al. 2010). The subsequent AMOC recovery may have caused a warming (Liu et al. 2009), and therefore an acceleration of the deglaciation.

b) Implications of using acceleration technique.

As discussed in the response to Andrey Ganopolski's 2nd general comment, the ocean and atmosphere components of iLove do not have as much time to adjust to the forcing changes as they had in reality, because the orbital and greenhouse gas forcing are accelerated by a factor of 20. With respect to the temperature this implies that, when there is a cooling trend due to orbital or greenhouse gas changes, the simulated cooling is underestimated. When there is a warming trend, the warming is underestimated. This indicates that the deglaciation would occur faster, if no acceleration-technique was applied.

We think that the simulated warming-trend during the Holocene in Fig. 4a is at least in part an artifact of the acceleration technique.

A smaller acceleration factor might lead to a better fit with sea-level reconstructions at a lower α-factor / CO_2 sensitivity.

c) On the ice mask used.

Offline experiments with IcIES (not shown) with LGM forcing and the suggested LGM ice mask indicate that the ice would not expand through the Bering Strait. Still, the offline experiments suggest that ice sheets would grow over the Siberian shelves, inconsistent with reconstructions. Prohibiting the ice growth in this region with the ice mask used here was a workaround; we will point this out in the revised manuscript.

Minor comments

a) In the introduction, the citations of the box / conceptual models of Kallen only is a strange choice. Many other classical box models could be cited: Calder, Imbrie, Paillard etc.

Ok, we will do that.

b) In your conclusions, you state that the main result is that orbital alone or CO2 alone is not enough. This is not a new result and citations to previous works showing it are in order: Gallée, Ganopolski (already cited in the manuscript).

Yes, we will clarify that.

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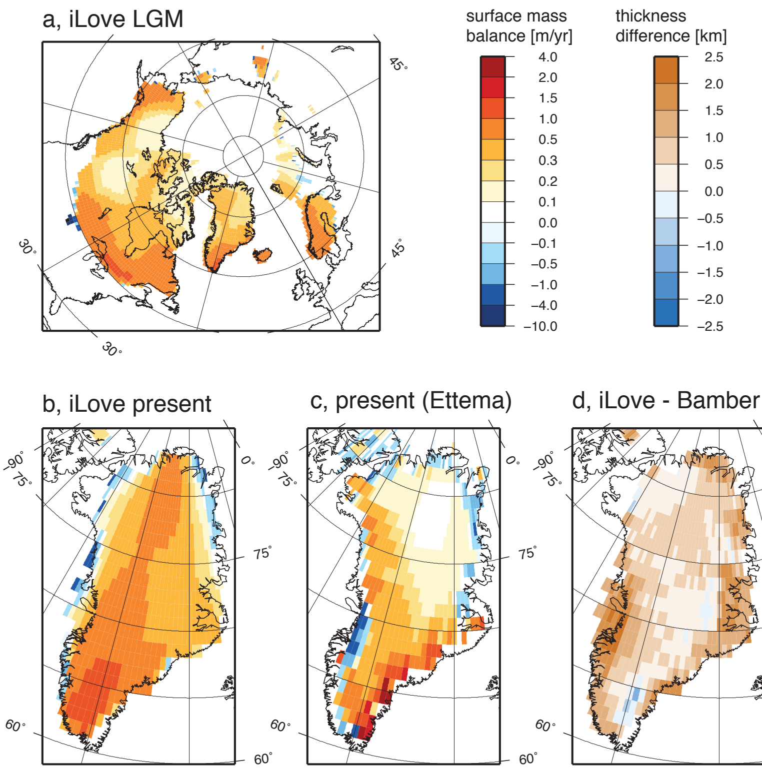

Fig. 1. a-c, Surface mass balance for LGM and present, and compared to high-resolution model results (Ettema et al. 2009, interpolated onto 1x1), and c, ice thickness in CTR relative to Bamber et al. (2001).