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Climate of the Past Discussions

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

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Thank you very much for the constructive reviewer comments. The main points that you and also the other reviewer, Didier Roche, criticised are well taken: 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, most importantly (but not only) the lack of hydrological feedbacks in response to ice sheet changes, the lack of Aeolian dust feedbacks, and the effect of the applied acceleration technique. Please find our responses to your individual comments below.

General comments

1. Surface mass balance scheme (PDD).

We agree that the surface mass balance scheme is crucially important for the simulations, and that a more physical approach that directly accounts for shortwave radiation changes is preferable to PDD schemes. In the present model setup, shortwave radiation changes due to precession only affect the surface melting via air temperature changes due to the atmosphere/ocean/vegetation feedbacks to the shortwave changes. We plan to test our results with an energy-balance-based surface mass balance model in the future, replacing the PDD scheme, but this is beyond the scope of the paper at hand. Thus, at this point, we cannot test whether the inception-problem during the first half of the glacial cycle, and the weak influence of precession during the second half of the glacial cycle are artifacts of the PDD-approach. We also agree that the PDD-factors, which are tuned to reproduce a realistic present-day Greenland mass balance, may be incorrect for past times and other ice sheets. We will discuss these possible caveats in the revised manuscript.

The PDD scheme could indirectly account for the effect of Aeolian dust, if Aeolian dust was included in our model setup. The shortwave effect would not be directly captured by the PDD scheme, but 2m air temperature in LOVECLIM would respond to a darker/lighter ice or snow surface. The warming/cooling would lead to more/less melting even when a PDD scheme is used; the magnitude of the melting may of course be different compared to the melting in a model that applies an energy-balance-based surface mass balance scheme. We will discuss possible effects of Aeolian dust in the revised paper.

2. Missing mechanisms that may affect the deglaciation.

We agree that including ice shelves, Aeolian dust deposition changes, or basal hydrology and fast ice streams may affect and potentially accelerate the deglaciation.

While we find that, for a constant CO°2 concentration of about 190ppm, orbital parameter changes alone only lead to a deglacial ice loss of about 20m sea level equivalent (sle), Ganopolski and Calov (2011) found that prescribing a constant CO°2 concentration of 200ppm only slightly reduces the amplitude of the inception and deglaciation compared to prescribing the reconstructed variable greenhouse gas forcing. However, they find that reducing the effect of Aeolian dust deposition changes in their model leads to a significantly weaker, incomplete deglaciation. This indicates that, if we also prescribed a similar effect of Aeolian dust, the deglacial ice loss would be larger, both in the "only greenhouse gas forcing" and "only orbital forcing" cases.

We expect that including ice shelves may enhance the variability in the ice sheet-climate system, in particular when ocean interactions can be taken into account, once runs without accelerated orbital and GHG forcing are more feasible (e.g., Alvarez-Solas et al. 2013).

The larger effect of the orbital forcing alone during the last deglaciation in Abe-Ouchi et al. (2013) is probably due to the fact that the IcIES forcing fields, including the temperature field that drives the PDD scheme, are computed differently. In iLove, the atmosphere responds interactively to the local and temporal variations of insolation due to orbital parameter changes. Abe-Ouchi et al. (2013) used equilibrium simulations with MIROC for different orbital parameters to derive a parameterisation of the orbital forcing effects on temperature. Their parameterisation of the orbital forcing prescribes a spatially uniform, annual mean temperature anomaly as a function of the July insolation at 65°N. During the deglaciation, with increasing precessional index, when Northern Hemisphere summers occur closer and closer to the sun, the parameterisation by Abe-Ouchi et al. (2007, 2013) leads to a warming due to the associated intensification of July insolation at 65°N. In our model, increasing precession also causes the Northern Hemisphere winters to become longer. The longer winters counteract the summer insolation changes, probably leading to a smaller ice loss during the deglaciation in our model.

The acceleration of the orbital and greenhouse gas forcing, and the choice of the coupling interval that is used to compute the climatological fields driving the ice sheet model, can also affect the magnitude of the ice loss during the deglaciation and the inception. These model parameters not only affect the comparison with Abe-Ouchi et al. (2013), but also with Berger et al. (1999) and Ganopolski and Calov (2011). We compute climatologies to drive the ice sheet model from each complete 50-year LOVECLIM "chunk". If the climatologies were instead computed from, say, only the last 30 years of the 50-year periods, the initial atmospheric adjustment to the melting ice sheets during the deglaciation would be cut off. Since the atmospheric adjustment during the deglaciation usually involves a surface warming, the simulated deglaciation could occur faster, and maybe the effect of just the greenhouse gas increase, or just the orbital forcing would be enhanced. However, we chose to use the full 50 years to account for all the orbital and greenhouse gas changes that are prescribed over each of the 50 year periods.

We will discuss these issues in the revised manuscript.

3. More detailed model description.

Yes, this is the first description of iLove. We will describe the model setup in more detail in the revised manuscript.

(1) Do the authors apply the same annual mean present-day temperature correction to all seasons?

Yes.

(2) How are temperature and precipitation computed on the coarse grid of LOVECLIM applied to the high-resolution ice sheet model grid?

Precipitation is bi-linearly interpolated onto the high-resolution ice sheet model grid. Temperature is bi-linearly interpolated onto the high-resolution ice sheet model grid, and subsequently corrected for deviations of the ice sheet surface height from the low-resolution LOVECLIM surface height, assuming a spatially uniform lapse rate of 0.005°C/m.

(3) What happens with the snow which accumulated over the ice sheet in LOVECLIM?

In this model setup, the hydrological cycle in LOVECLIM does not know that there can be ice sheets. Any snow that exceeds a thickness of 10m water equivalent first fills the soil water buckets, and once those buckets are full, the snow turns into ocean runoff.

(4) How simulated ice sheet affects surface properties of LOVECLIM?

If more than 50% of a LOVECLIM grid cell are covered by at least 10m thick ice, the LOVECLIM grid cell is defined as ice-covered. 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.

(5) What the author did with the river routing scheme?

The river routing scheme in this model setup is not affected by ice sheet changes.

(6) How forest fraction affects ice sheets (p. 513, line 14)?

The forest fraction does not affect the ice sheets directly. The formulation of the sentence in the manuscript is misleading. The 2m air temperature and precipitation fields are passed from LOVECLIM to IcIES; then, after the IcIES iteration, the surface albedo, forest fraction, and orography in LOVECLIM are updated according to the new ice sheet surface height and thickness. The forest fraction affects the ice sheet only indirectly via its effect on climate in non-ice-covered regions (for example, a larger forest fraction may lead to a darker surface albedo in snow-covered areas, because the snow can fall through the canopy).

(7) Where one can find description of parameterisation of "active calving into proglacial lakes"? And if it was not described before, would be useful to do it in the manuscript under consideration

The parameterisation was previously described in Abe-Ouchi et al. 2013 (see their Methods section).

"Active calving", or "calving into proglacial lakes" is parameterised as a surface melting at a rate of 10m per year, if a grid point fulfills the following conditions: i) the bedrock elevation at the grid point is below sea level; ii) the surface mass balance at the grid point is negative, corresponding to an ablation area; and iii) at least one of the neighbouring grid points fulfills the floating condition: ice thickness $<$ water density/ice density*(sea level - bedrock height). Here, the sea level does not vary with the ice volume, it is assumed to be at 0m.

(8) How proglacial lakes were simulated/diagnosed?

The terminology in the paper is confusing. The "proglacial lakes parameterisation" affects all the grid points that fulfill the conditions above. These conditions may also be fulfilled in areas that were not necessarily close to a proglacial lake, for example in coastal areas. Our setup does not include a hydrological model that could explicitly simulate actual proglacial lakes.

(9) Do you scale up by factor 3 only CO2 or effect of all three GHGs and if the later is correct, please, change the text accordingly.

Only the effect of CO_2 is scaled up by a factor of 3, not the effects of N_2O or CH_4.

Specific comments

Abstract. Duration of inception, and reconstructed deglacial warming.

Good point, we will adjust the abstract accordingly. The statement that the SAT rose by about 4°C during the deglaciation is based on the estimates by Shakun et al. (2012)

and Marcott et al. (2013). The uncertainty will be updated based on the IPCC results.

510.13-14 "Increasing obliquity and precession then led to accelerated ice loss due to ... calving" I do not understand how obliquity and precession affect calving

We are planning to test this hypothesis with another sensitivity experiment, keeping the obliquity constant at 28ka BP to see whether the enhanced calving persists even without the obliquity increase. An alternative hypothesis is that the increased calving stems from the geometrical constraints of the ice sheet. We will adjust the revised manuscript accordingly.

512.16 "a fixed grounding line is used" The term "grounding line" sounds strange in application to an ice sheet model which does not have floating ice. I would suggest instead to write that the domain which can be occupied by ice sheet is fixed and the ice cannot spread beyond this domain. The flux of ice through the boundary of this domain was treated as implicit calving.

Ok, we will do that.

515.13 The saddle collapse occur at around 13 ka is also simulated in CLIMBER model (see Ganopolski et al. 2010, fig 4f)

Oh, sorry, we had not noticed that. The reference will be added to the revised version.

515.16 "The Greenland and Laurentide ice sheets are thicker towards the margins" Thicker than in reconstructions?

Thicker than in ICE5G.

515.17 The model does not simulate "global" ice volume. It simulates only NH volume. Since contribution of the SH to LGM global ice volume is estimated as 10-20 m, it implies that the NH ice volume is overestimated by 30-40 m, which is a lot. Is it true?

It is true that only NH volume is simulated. But, as illustrated in Fig. 2a, the simulated NH ice volume during the LGM lies within the uncertainties of the global sea level

reconstructions, indicating that the NH volume might be overestimated by "only" 10-20m. Note that the ICE5G-curve in Fig. 2a, like the IcIES results, only represents NH ice volume changes. Also note that the ice volume is not corrected for the large Greenland ice sheet at the end of the run / at present, which amounts to about 11msle.

517.25 I cannot understand how increased calving at any time "can be regarded as a precursor to the glacial termination"

If indeed the increased calving starting at 28ka BP is caused by the obliquity increase, that means that the orbital parameter changes that led to the deglaciation already showed effect much earlier. However, we cannot show that this is the case, as stated in our response to your comment about page 510 line 13-14. We will test this hypothesis with the sensitivity experiment mentioned above. The increased calving may as well be a consequence of geometric constraints of the ice sheet, and it might not be related to the increasing obliquity at 28ka BP. We will adjust the revised manuscript accordingly.

519.10 This result is also consistent with much earlier publication by Calov et al (2005)

Ok.

519.25 The meaning of "simulated 3ka lag of the CO2 increase" is not clear to me. Pleas clarify

We mean the lag between the simulated onset of the deglaciation, and the onset of the prescribed CO_2 increase. We will rephrase this.

520.10-14 "Despite uncertainties in the order of 1 ka with respect to the dating of the CO2 reconstructions ... the qualitative result that the orbital changes had the potential to ... initiate the deglaciation holds" I do not understand what uncertainties in CO2 dating has to do with the ability of orbital forcing to initiate deglaciation.

Exactly, that is why the conclusion still holds. We will think of a different way to phrase it to avoid confusion.

521.10 What is the meaning of "dynamically very active"?

The point we want to make is that the negative terms in the mass balance, in particular the calving terms, are already quite large before the onset of the deglaciation, equivalent to a runoff of about 0.25 Sv. We used the term "dynamically active" to describe the fact that the large accumulation terms are balanced by large loss terms, even before the deglaciation. The term is confusing, and we will rephrase the sentence.

528. Fig. 2a Yellow line cannot be seen in the printed version

Ok, we will change the color to something more visible.

528. Fig. 2. Better to say "benthic foraminifera oxygen isotopic records".

Ok.

528. Fig. 2. What is the source for the uncertainty estimate for Waelbroeck's reconstructions?

The uncertainty estimate shown in Fig. 2 is the confidence interval given in Waelbroeck et al. (2002). It amounts to +/-13m sle, and originates from dating and regression uncertainties.

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Interactive comment on Clim. Past Discuss., 10, 509, 2014.