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

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The manuscript by Heinemann et al. presents one of the first attempts to simulate the last glacial cycle with a geographically explicit bi-directionally coupled climate-ice sheet model of intermediate complexity. Using a set of experiments with different forcings the authors came to the conclusion that both orbital forcing and GHGs play important role during glacial termination. This is interesting and well-written paper and I would recommend it for publication in the CP after moderate revision. I believe that the manuscript will benefit from more detailed disruption on modeling approach and more critical discussion of model limitations and potential caveats.

General comments

1. The authors mentioned their surface mass balance scheme in just one short sentence: "The surface mass balance is approximated by a positive-degree-day (PDD) scheme". However, surface mass balance scheme is very important for simulation of glacial cycle and for simulation of glacial termination, arguably, it is crucially important. The PDD scheme is not physically based and it was shown in a number of recent publications (see e.g. Robinson et al. (2010), van de Wall et al., (2011)) that under variable orbital forcing its applicability even for the Greenland ice sheet, for which it was developed, is questionable at best. For the rest of the world PDD scheme is not even semi-empirical because there is no "empirical" data for the Laurentide ice sheet. The problem is not that PDD is simple – there is no prove that complex is always better than simple. The problem with PDD is that it does not explicitly account for shortwave radiation which is the major component of ice sheet mass balance. This is why it is likely that PDD–based models significantly underestimates the effect of precession on surface mass balance and this could be one of the reasons for the problem with simulation of the first half of the last glacial cycle. This is also probably the reason why in this model precession plays so minute role during deglaciation. In addition, PDD scheme cannot account for the effect of dust on snow and ice albedo, the effect which some workers consider to be very important during deglaciation. Therefore I believe that a more thorough discussion of model limitations and potential caveats is required.

2. All model results are model-dependent but in some cases they are more model-dependent than in other. Clearly, glacial termination belongs to this case. Since glacial termination is the manifestation of strong nonlinearity of the climate-cryosphere system, any factor analysis and computed synergy are very sensitive to model formulation. The model used in the study is the mdoe intermediate complexity (both in respect of climate and ice sheet components) and it is lacking a number of processes which can be especially important during glacial termination, in particular, dynamical destabilization of the grounded ice via collapse of the shelf ice and acceleration of the ice streams; effect of basal hydrology on the ice sheet dynamics; effect of aeolian dust deposition on surface albedo and, as I argued above, the model likely underestimates the effect

of presessional component of the orbital forcing because of PDD method. All these limitations should be discusses and results compared with similar modeling studies. For example Berger et al. (1999) and Ganopolski and Calov (2011) found that up to 80 msl of the NH ice sheet can be melted by the orbital forcing alone with the fixed glacial CO2 level. Interestingly that even in Abe-Ouchi et al. (2013), where similar ice sheet model and PDD approach were used, orbital forcing is more efficient in melting of ice sheets than in the manuscript under consideration.

3. My understanding that this is the first description of iLove. This would justify a more detailed model description. I personally have a number of questions to the authors: (1) do the authors apply the same annual mean present-day temperature correction to all seasons; (2) how temperature and precipitation computed on the coarse grid of LOVECLIM are applied to the high-resolution ice sheet model grid; (3) what happens with the snow which accumulated over the ice sheet in LOVECLIM; (4) how simulated ice sheet affects surface properties of LOVECLIM; (5) what the author did with the river routing scheme; (6) how forest fraction affects ice sheets (p. 513, line 14); (7) where one can find description of parameterization of "active calving into proglacial lakes" and if it was not described before, would be useful to do it in the manuscript under consideration; (8) how proglacial lakes were simulated/diagnosed; (9) do you scaled up by factor 3 only CO2 or effect of all three GHGs and if the later is correct, please, change the text accordingly. Some of these issues may be too technical but they can be described in appendixes.

Specific comments

Abstract. My understanding is that the first sentence of the abstract is based on the general knowledge, not on specific modeling results. Then it is not clear why "ice-sheet buildup came to a rapid end at ca. 20-10 kaBP" since it is known that this happened prior to 20 ka. Moreover, if the beginning of buildup was the last glacial inception at ca. 120 ka, then the buildup lasted almost 100 kyr, not 80 kyr as written in the manuscript. It is also written that global SAT rose during glacial termination by about 4C. At the

same time, in our IPCC chapter WE wrote that LGM was 3-8C colder than present. I wonder why the authors reduced this uncertainty so drastically.

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

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.

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

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

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?

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

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

519. 25 The meaning of "simulated 3ka lag of the CO2 increase" is not clear to me. Pleas clarify

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.

521. 10. What is the meaning of “dynamically very active”?

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

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

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

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