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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Investigating the evolution of major Northern Hemisphere ice sheets during the last glacial-interglacial cycle" *by* S. Bonelli et al.

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We would like to thank Dr. Calov for fruitful comments and attentive corrections. We agree with his suggestions, and we have modified the manuscript accordingly. For the specific comments and concerns, please see the list below.

Answers to major concerns:

1. In section 2.1, "The CLIMBER climate model", the authors describe the CLIMBER-2 model which is used in their simulations. Overall, this is well done. But concerning their inclusion of a parameterisation of dust impact and snow aging on snow albedo there is problem with the method and with proper citation. The authors implemented these

parameterisations on the coarse CLIMBER grid. Therefore, the parameterisations cannot directly affect the surface mass balance of the ice sheets and might have a minor impact only. The authors of the reviewed paper should demonstrate how much their dust and snow aging parameterisations influence the results. How strong do these parameterisations affect mass balance and ice volume? Further, a similar dust parameterisation following Warren and Wiscombe (1980) was already described and applied to last glacial inception by Calov et al. (2005). Calov et al. (2005) introduced the dust parameterisation in their high resolution surface energy balance module. Their paper should be cited at the place where the dust parameterisation is introduced. The albedo is computed on several vertical layers; therefore, the impact of dust parameterization on the albedo is accounted for at the topography corresponding to that of the ISM. As a consequence, in our model, contrary to the Dr. Calov's statement, this parameterization directly affects snow mass balance. To better assess its effect, we have performed a test where no dust is accounted for. In this simulation, the NH ice sheets do not completely retreat during the deglaciation period and an ice sheet is produced over Siberia, reaching a volume of 35 x 1015 m3 at the LGM. This effect has been discussed in the revised version of the manuscript (see section 5, discussion). Compared to the previous study by Calov et al. (2005b), the impact of dust on the first phase of the glacial cycle is minor, probably because in the early phase of the glacial inception the atmospheric CO2 concentration is close to its pre-industrial level and dust weight is therefore negligible. For further discussion, please see point 7, answers to comments by reviewer #1. The paper by Calov et al. (2005) is now cited where dust parameterization is described.

2. Section 2.3. An inversion is predominantly a winter phenomenon. Therefore, the parameterisation of inversion may affect the ice extent through the glacial cycle presumably only minor (via less snow fall in winter, last formula on page 1022). If a parameterisation is introduced, the reader of the paper is certainly interested how far this parameterisation does effect the results. The authors should write some sentences about that. Best would be to discuss shortly a simulation without the inversion param-

eterisation in comparison with a simulation which includes the inversion parameterisation. We agree. As suggested, we have now performed a simulation without the inversion parameterization. This produces a smaller ice volume (12.5 x 1015 m3 at 110 ka, versus 14.5 x 1015 m3 for the standard simulation). A brief discussion of this parameterization effect on the ice extent has been added to the revised version of the paper, section 5.

3. In Figure 6, the northern Hemisphere ice volume curves nearly all show a rather sharp bend at simulated last glacial maximum (LGM). This sharp bend appears also for the simulations with constant atmospheric CO2 content. Therefore, my first guess would by that orbital forcing causes the sharp bend, which is followed by a strong decrease in ice volume (termination). Considering the characteristics of the ice volume curves during times earlier than LGM, one observes, as the authors state too, that the amplitudes are rather small: the response of the model to the precession in orbital forcing is rather small. Considering the summer insolation (red curve in Figure 5), one can observe a rather strong precessional cycle showing strong up and down in summer insolation. But simulated ice volume response is weak. On the other hand, the model strongly response to the rather weak increase in summer insolation after about 20 kyrs BP. My questions are: Which mechanism causes termination in the model? Is there a marine instability parameterisation in the model? The coupled model does not include marine instability parameterization. Therefore, the only major changes amplifying the CO2 and insolation forcings for deglaciation are dust and diurnal variability. In the next step we would also like to account for glacial ocean variability. The implementation of the dust parameterization represents a first mechanism favoring glacial termination. Indeed, when no dust is accounted for, glacial termination is not completed. In all the sensitivity studies performed here, the dust weight is based on the CO2 concentration inferred from Petit et al. (1999), so that dust is the same for all the experiments. The dust weight is hence maximum close to the LGM, as described in section 2.1. Indeed, the response of the model to the precession signal is rather small. We have therefore decided to examine the role of the PDD parameterization on ice

evolution; being based on an empiric formulation, this method becomes less accurate for climate states largely different from present-day ones (i.e. glacial state). Among the different empiric parameters affecting the PDD calculation, we have focused our attention on the role of diurnal variability. To do so, we have examined the results of the HadCM3 and of the MIROC 3.2 models for the LGM and present-day (PMIP2 database: http://pmip2.lsce.ipsl.fr/database/), which are the only ones among the PMIP2 GCMs including diurnal variability (i.e. the IPSL model does not include it), and we have observed that the amplitude of the diurnal cycle at the LGM is increased by ~ 150 % in both models compared to interglacial periods over Fennoscandia and North America. We have included these results in our simulations via the standard deviation of the daily temperature parameter in the PDD formulation, which differs for glacial and interglacial conditions by this factor. Furthermore, based on available literature, we have also assessed the effect of different degree-day factors for snow and ice, necessary to convert PDDs into melting, to finally let them fixed to the most commonly used values of respectively 3 and 8 mm day-1C-1.

Answers to minor comments:

1. Page 1015, line 23: "They are also useful to study the internal viscoelastic structure of the solid Earth." Please, erase the word "solid". Only parts of the Earth are solid. Done.

2. Page 1015, line 27: ": : :and do not have intrinsic glaciological self-consistency". What does this sentence mean? We agree that this sentence is not clear and misleading. It has been deleted in the revised manuscript. In our mind, glaciological self-consistency means that the past history of the ice sheets is accounted for, which is not the case in models such as ICE 5G (Peltier, 2004).

3.Page 1017, lines 10-12: "they also explicitly account for key features such as the vertical temperature profile in the ice sheet, the basal melting and the ice flow induced by ice dynamics (Ritz et al., 1997)." There is something wrong with this sentence.

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GREMLINS includes the full 3D temperatures and not only vertical profiles. Ice flow is always induced by ice dynamics. Please, fix the sentence. This sentence has now been corrected in the revised version of the paper.

4.Page 1020, lines 14-16 and 20-22: Repetition. Please, erase one of the sentences. Best erase the sentence The evolution of the ice sheet surface and geometry is a function of surface mass balance, velocity fields, and bedrock position.“ We disagree on this point.

5.Page 1020, lines 23-24: It is calculated with the zero-order shallow ice approximation (Ritz et al., 1997).“ The zero-order shallow ice approximation is certainly not by Ritz et al. (1997). If you explicitly write zero-order shallow ice approximation“ at least Hutter (1983) deserves to be cited. We agree; we have now added the citation in the revised version of the paper.

6. Page 1023, lines 7-13: Please, give more details about the coupling procedure. In the paper, it is written “In our simulations, the ISM is called every 20 years : : :”. What do you mean? Is the time step of the ice sheet model 20 years? Or is there an asynchronous coupling and the ice sheet model is called more often than the climate model? In our simulations, the ISM is called every 20 years and is run also during 20 years with the same climate forcing; in turn, the new ice-sheet geometry (altitude and surface), as well as the new land and/or ocean fraction computed by GREMLINS are then averaged on the CLIMBER grids through an aggregation procedure and provide new boundary conditions for the climate model. This description has now been included in the revised version of the paper, section 2.3.

7. Page 1026, lines 10-13: Yokoyama et al. (2001) is cited when modelled sea level during glacial onset is compared with proxy sea level. But Yokoyama et al. (2001) does not seem to contain data for that time. We agree, it is a mistake. This citation has now been erased.

Points 8 – 12. We agree. We have now included all suggestions in the revised version of the paper.

13. Page 1033, line 25-26: I am not able to detect mentioned “slight decrease” in the Waelbroeck data. If this was about one or two meter sea level change I would not be concerned about such variation, because it is beyond data accuracy and model ability. We agree. It is indeed a variation of about 2 m in 8 ka (from 50 ka BP to 42 ka BP). We have now changed this sentence.

Points 14 – 26. We agree. We have now included all suggestions in the revised version of the paper.

Points 27 – 28. We have now provided the figures to “Climate of the Past” in a format that can be zoomed without significant image quality loss.

29. Page 1052, Fig. 5: What does the grey shading bars denote in the figure. Either explain them or erase them. The grey bars denote relevant time intervals. Their purpose is to highlight the synchronicity of curves evolution. This is now explained in the figure caption.

Interactive comment on Clim. Past Discuss., 5, 1013, 2009.