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Climate of the Past Discussions

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 reviewer #1 for his/her constructive comments. We agree in particular with the fact that a more detailed comparison with Calov et al. (2005a, 2005b) is needed. This has been implemented in the revised version of the paper. For the specific comments, please see the list below.

1. "Page 1014: The abstract should mention the name of the climate model and of the ice sheet model used, as that will make it easier for interested readers to find the article when searching for articles using CLIMBER or GREMLINS" We agree. This is now done.

2. Page 1015, line 4: Calov et al. (2005b) also showed the important effect of the ice-albedo effect on glaciations. Please add a reference to this article in the list of references cited here. 3. Page 1016, line 24: Please add a reference to Loutre and Berger (2000) to this list of references. They used the LLN 2-D NH model to simulate the last 200 ky, looking especially at the effect of CO2 on the glacial-interglacial cycle. We have responded to comments 2 and 3; the suggested references have now been added to the manuscript.

4. Page 1019, line 3-6: In earlier versions of CLIMBER, the snow aging process might not have been accounted for. However, in later versions of the model (i.e. CLIMBER 2.3, which is used here, as well as in many other more recent studies, e.g. Calov et al. (2005a, b), Claussen et al. (2003, 2006), Jahn et al. (2005), Kubatzki et al. (2006)) the snow albedo depends on the surface temperature and snow age (see section 2.3 in svat.f of the CLIMBER 2.3 source code), and, if coupled to the ice sheet model SICOPOLIS, contamination by dust is also accounted for (see Calov et al. 2005a). Some important processes affecting the ice sheet mass balance are still missing, but the ice age and temperature effect on the snow albedo is accounted for. Please adapt this statement accordingly. Some slight differences between the versions of CLIMBER 2.3 used at PIK and at the LSCE exist. On our version, the snow aging process is not accounted for over glacier areas. Therefore, we did not change our statement in section 2.1.

5. Page 1019, line 12 to page 1020, line 7: A very similar parametrization of the effect of dust on the snow albedo was already used in Calov et al. (2005a, b), and needs to be cited here. The only difference between the parametrization used here and the one used by Calov et al. (2005a, b) appears to be that Calov et al. used a weight based on the ice volume at a given time compared to the LGM ice volume, whereas here the weight is a function of the atmospheric CO2 variations. In order to be able to use this parametrization in other models, it would be useful to give the details of how the weight is calculated here. The reasons for choosing a different weight than

Calov et al. (2005a, b) should also be explained. Calov et al. (2005b) also show in detail how the inclusion of the dust improved the simulation during glacial times, which should be mentioned here. We agree. In the revised version of the paper (section 2.1) we have now included a description of how dust weight is accounted for. We decided to index the dust weight on the CO2 concentration, rather than on the ice volume as done by Calov et al. (2005a, b), to keep the dust concentration independent from the ice evolution itself. It is therefore assumed as external to the system (since dependent to the prescribed pCO2), and its dependency to CO2 is justified by the fact that dust seems larger for lower CO2 concentrations, and vice versa. We recognize that this may be linked to ice evolution, but we have decided to use a simple modeling approach, in which ice does not feedback on dust, since this process is poorly known. The effect of this parameterization is now discussed in the revised version of the paper (sections 2.1 and 5).

6. Page 1019-1020: Since Calov et al. (2005a, b) used the CLIMBER-SICOPOLIS model for studies of the glacial inception, with some difference in the results compared to the results presented here, it would be helpful for the reader to briefly mention how the GREMLINS model differs from SICOPOLIS. We agree. We have now included a brief discussion based on information available in Calov et al. (2005a,b) (sections 2.1 and 3.2.1 of the revised paper). Please see also points 7 and 8 for further discussion.

7. Page 1027, line 8-21: In Calov et al. (2005b), it was shown that the inclusion of the dust parametrization helped to reduce the occurrence of ice over Alaska, bringing the model in better agreement with observations. It would be good to know whether this is also the case here, or which other model or forcing differences (e.g., slightly different CO2 forcing dataset and slightly different dust parameterization) might contribute to these differences in the simulation of the icesheets, since in both cases the ice models are forced by CLIMBER. Based on the outcome of this analysis, the last sentence of the paragraph should be adapted, since the glaciation of Alaska in the CLIMBER-GREMLINS model appears to not only be due to the coarse resolution of

the climate model if the simulation with CLIMBER-SICOPOLIS shows a smaller glaciation of Alaska. We agree on this point. We have now changed our sentence, and included a discussion with the previous results from Calov et al. (2005a,b) (see section 3.2.1). The differences may be due to the adopted coupling procedure, as well as to dust parameterization. Indeed, with our approach, in the first phase of glacial inception the effect of dust on ice volume evolution is smaller than in Calov et al. (2005b). At 110 ka BP, the model produces a total NH ice volume of 14.5 x 1015 m3, vs 14.7 x 1015 m3 when dust is not accounted for (whereas in Calov et al. [2005b] the simulated ice volume, at 110 ka BP, is twice the one produced in the AOVID experiment). As explained in point 5, in the present study the dust parameterization is indexed on the CO2 concentration; its impact on the simulated ice volume is minor than in Calov et al. (2005b) 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. As for the coupling procedure, Calov et al. (2005a,b) did not include a parameterization for the refreezing of melt water (included here), nor they compute the snow mass balance via the PDD method, which may largely affect the glacial onset. Furthermore, in our study we introduce a method to account for the inversion phenomenon. Regarding the differences of ice inception over Hudson Bay, neither SICOPOLIS nor GREMLINS dispose of an explicit ice shelves modeling. In Calov et al. (2005a,b), the formation of an ice cover in this region may be due to a different parameterization of the ice calving, which could crudely reproduce the ice shelves.

8. Page 1027, line 22 to page 1028, line 2: As noted above, the simulated ice cover in CLIMBER-SICOPOLIS in Calov et al. (2005b) differs significantly from the one produced by CLIMBER-GREMLINS, also in regards to the formation of ice over Hudson Bay, which occurs by 110 kyr BP in CLIMBER-SICOPOLIS (see Fig 3 and 4 in Calov et al., 2005b). A more detailed analysis should be performed to explain these differences, especially since the initial conditions (start at 126 ky BP, 5 kyr integration under 126 kyr BP forcing) and the forcing for the two experiments are almost identical. We agree. A discussion of the previous results from Calov et al. (2005a, 2005b) has been included

in the revised version of the paper, section 3.2.1. The reasons for such differences are discussed in point 7. Furthermore, the major difference between GREMLINS and SICOPOLIS is now highlighted in section 3.2.1 of the revised manuscript. Indeed, the two ice sheet models have a very similar physics. The major difference is that SICOPOLIS includes polythermal treatment in the heat equation. This gives the possibility of having temperate ice within the ice sheet which can affect ice viscosity and deformation. This method is not included in GREMLINS but we estimate that the impact of this feature is weak compared to the uncertainty on sliding parameters.

9. Page 1037, after line 10: Either in section 4.2 (or a new Discussion section 4.3), the results from the sensitivity study of the CLIMBER-GREMLINS model should be compared with the other studies mentioned in the introduction (page 1016) that also simulated the last glacial-interglacial cycle (or parts of it), to show whether the larger sensitivity of the Fennoscandinavian ice sheet to atmospheric CO2 (compared to the North American ice sheet) is a new results, in contradiction to earlier results, or in agreement with earlier results obtained by simpler models. We agree. As also suggested by reviewer #3, we have now added to the article a new section "Discussion", where our results are compared with those of previous modeling studies. The stronger sensitivity of the FIS to atmospheric CO2, compared to the LIS, is in agreement with the previous results of Tarasov and Peltier (1997) based on en energy balance model. Furthermore, they show that the radiative impact of varying atmospheric CO2 concentration is critical to achieve adequate glaciation. As highlighted by our sensitivity tests and by previous studies (Berger et al. [1998, 1999]), the atmospheric CO2 concentration affects the intensity and regional distribution of glaciation events, whereas ice inception itself is triggered by changes in summer insolation. The coupled model does not produce any glacial onset in the sensitivity test INSO-126, in agreement with Loutre and Berger (2000) and Calov et al. (2005b).

We have also answered to all minor technical corrections in the manuscript.

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