Clim. Past Discuss., 5, C189–C194, 2009
www.clim-past-discuss.net/5/C189/2009/

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.

R. Calov (Referee)

calov@pik-potsdam.de

Received and published: 11 May 2009

In the paper, the climate model CLIMBER-2 coupled to the ice-sheet model GREMLINS is utilized for simulations of last glacial cycle. A simulation with varying orbital forcing and prescribed atmospheric CO2 content from the Vostok ice core is performed. Additionally, there are sensitivity studies with fixed interglacial orbital forcing and with different values of constant atmospheric CO2 content. All these model experiments are well explained.

The major conclusions of the paper are (i) that their model is capable to simulate the last glacial cycle, (ii) the drop in orbital forcing is the main factor for glacial inception, (iii)

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

the northern Hemisphere ice volume responds strongly if CO2 drops below a certain value and (iv) the Fennoscandian ice sheet has a stronger sensitivity to CO2 than the Laurentide ice sheet.

Overall, the paper is clear and concise, although I have to list below a number of comments which might improve the paper. Given the authors will follow my advices, the paper deserves publication in Climate of the Past.

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.

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 affect the results. The authors should write some sentences about that. Best would be to discuss shortly a simulation without the inversion parameterisation in comparison with a simulation which includes the inversion parameterisa-

CPD

5, C189–C194, 2009

tion.

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?

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.

2.Page 1015, line 27: “. . .and do not have intrinsic glaciological self-consistency“. What does this sentence mean?

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. GREMLINS includes the full 3D temperatures and not only vertical profiles. Ice flow is always induced by ice dynamics. Please, fix the sentence.

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

CPD
5, C189–C194, 2009

function of surface mass balance, velocity fields, and bedrock position."

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.

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?

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.

8. Page 1030, line 9: replace "stretches" with "advances".

9.Page 1030, line 15-17: Maybe, the authors consider to cite Calov et al. (2002), who modelled such a two-dome structure by including fast sediment sliding.

10.Page 1031, the formula in line 19: Please, use the Greek rho for density. The same in the corresponding text.

11.Page 1032, line 22: the number in the parenthesis is not sea level drop. It is sea level stand.

12.Page 1033, line 4: Now indeed a drop in sea level of 24 m is meant. Please, remove the sign of the 24 m.

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.

CPD
5, C189–C194, 2009

14. Page 1034, line 6: Please, replace “sea level lowering” with “sea level stand”.

15. Page 1034, line 23: The comma should be erased.

16. Page 1035, line 16: Replace “on Fig.” with “in Fig.”

17.Page 1036, lines 19-20: Please, erase the words “as soon as the early phase of glaciation”.

18. Page 1036, line 20: 49% is meant here.

19. Page 1036, line 21: 100% is meant here.

20. Page 1036, line 26: 157% is meant here.

21. Page 1036, line 27: 28% is meant here.

22. Page 1036, lines 24-28: Please, mention that LGM values are compared here.

23. Page 1037, line 2: “ice advance” instead of “ice expansion?

24. Pages 1039-1045: The entire reference list should be checked whether it is according to the Copernicus Publications Reference Type. For example, the full authors list has to appear also in case if there are more than three authors. Very often or always there is only a “et al.” in the paper if there are more than three authors, e,g., page 1040, lines 7-17.

25. Page 1040, line 13: “Clausse” should spell “Claussen”.

26. Page 1041, line 24: “Gallee” is misspelled. Please check the last "e".

27. Page 1049, Fig. 2: One can hardly see the ice cover in relevant regions which are mentioned in the manuscript. The figure should enlarged

28. Page 1051, Fig. 4: As for Fig.2.

29. Page 1052, Fig. 5: What does the grey shading bars denote in the figure. Either explain them or erase them.

CPD

5, C189–C194, 2009

References

Calov R, Ganopolski A, Claussen M, Petoukhov V, Greve R (2005) Transient simulation of the last glacial inception. Part I: glacial inception as a bifurcation of the climate system. Clim Dyn 24 (6): 545-561, doi:10.1007/s00382-005-0007-6

Calov R, Ganopolski A, Petoukhov V, Claussen M, Greve R (2002) Large-scale instabilities of the Laurentide ice sheet simulated in a fully coupled climate-system model. Geophys Res Lett 29 (24): 2216, DOI: 10.1029/2002GL016078

Hutter K (1993) Theoretical Glaciology. D Reidel Publishing Company. Dordrecht etc.

Warren SG, Wiscombe WJ (1980) A model for the spectral albedo of snow. II: Snow containing atmospheric aerosol. J Atmos Sci 37:2734-2745

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