Clim. Past Discuss., 8, C40–C44, 2012
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Climate of the Past Discussions

CPD

8, C40–C44, 2012

Interactive Comment

Interactive comment on "Modelling large-scale ice-sheet–climate interactions following glacial inception" *by* J. M. Gregory et al.

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Received and published: 13 February 2012

The manuscript by Gregory et al. for the first time presents results of simulations of the last glacial inception using a fully coupled AOGCM-ice sheet model. This is an important step towards the study of glacial cycles with comprehensive Earth system models and I would recommend publication of the manuscript in CP after some improvements in discussion of results and limitations of the modeling approach. However, before discussing more or less technical issues, I want to share with the authors my serious concern about the methodology used in their study. As it follows from the last paragraph of the manuscript, the authors do have an intention to improve their modeling approach in the future and I would strongly support this intention because serious flaws in the methodology undermine potential importance of this pioneering work.

General comments

1. Surface mass balance scheme. To couple the Glimmer ice sheet model with the FAMOUS AOGCM the authors use the so-called Positive Degree Day (PDD) scheme which they characterized as “convenient and well-established empirical approach”. To the contrary, I would argue that using of PDD scheme in the coupled AOGCM- ice sheet model is an anachronism. The applicability of the PDD scheme to different climate and geographical conditions is questionable at best. Recent study by van de Wal et al. (Nature Geosci., 2011) convincingly demonstrated that the PDD scheme considerably underestimates surface mass balance response to the insolation changes even for the Greenland ice sheet for which this scheme was tuned. I strongly believe that further advances in understanding mechanisms of glacial cycles require abandoning of the “good old” PDD scheme once and forever.

2. Ice sheet model. The ice sheet model used in the study does not allow ice to advance beyond the present-day coastline. In reality, of course, ice sheets can easily cross the shallow channels and seas, like the Baltic Sea. This limitation imposes significant restriction on spreading of the ice sheet and leads to underestimations of the total ice sheet area and albedo feedback. Since many ice sheets models do allow ice to spread over shallow water, the choice of the ice sheet model for this study is hard to justify.

3. Ice-albedo feedback. If I understand the methodology correctly, even a single “pixel” in the Glimmer model covered by several centimeters of ice makes the entire FAMOUS grid cell 100% ice covered. Since the area of FAMOUS grid cell is 50,000 times larger than Glimmer grid cell, it means that, at least theoretically, the ice-albedo feedback can be overestimated by factor 50,000! I believe, the commonly used “fractional approach” is absolutely crucial for the realistic coupling between a high resolution ice sheet model and a coarse-resolution AOGCM.

4. Ice sheet domains. The ice sheet model is only applied to two, relatively small domains (north Europe and Laurentia). If the computational cost is not a problem, then what is the reason for such restrictive application of the ice sheet model? This provokes

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a suspicion that the model tends to grow ice sheets in the "wrong places".

Specific comments

Page 171, line 12. "It is generally accepted that the timing of glacial cycles is linked . . ." Do the authors mean here that only the timing of glacial cycles is related to the orbital forcing? I would assume that it is (almost) generally accepted that the orbital forcing is the driver of glacial cycles.

Section 4.3. The authors try to explain here why the FAMOUS-Glimmer model simulates too much ice for preindustrial ("recent") climate conditions. They first attribute this problem to cold biases in FAMOUS but then they state that when Glimmer is coupled to HadSM3, which has smaller biases, even more ice is simulated. Eventually the authors conclude that "the excessive ice volume simulated by FAMOUS–Glimmer for the recent climate is due to biases both from the SMB scheme and from the simulated climate". While I do have serious concerns about SMB scheme, I cannot see the reason why it should necessarily overestimate surface mass balance. I would rather suspect that overestimation of "recent" ice volume results from overestimation of the ice-albedo feedback strength discussed above. Whether this hypothesis is correct can be tested by comparing coupled and uncoupled Glimmer simulations for the "recent" climate state.

Page 182, line 18. "the area of accumulation have positive SMB". This sounds rather trivial.

Page 182, line 24. "as found by Born et al. (2010)." Born et al. did not employ an ice sheet model.

Page 183, lines 5-10. The authors try to explain here why simulated rate of ice volume growth is much smaller than the reconstructed one. Their first idea is that paleoclimate reconstructions can be biased because of "using a fixed conversion factor" between d18O and ice volume. However, it is not true that ice volume reconstructions are based

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on “fixed conversion” of d18O to ice volume because such reconstructions explicitly account for the temperature effect through the independent reconstructions of the deep ocean temperature. Moreover, apart from benthic d18O, there are several other independent methods (corrals, Red Sea records) which all give quite consistent estimates. Although uncertainties in reconstructed global ice volumes during the last glacial inception (stage 5d) remain, there is no doubt that in reality ice volume increased much faster than simulated by the model. And there are many reasons for that. Firstly, applicability of PDD scheme for this type of studies is questionable at best. I guess, the authors admit this fact by mentioning “large systematic uncertainties”. Secondly, the ice-albedo feedback is strongly overestimated by using of unrealistic coupling procedure (see above). On the other hand, the ice sheets growth is strongly restricted by their inability to bridge even narrow straights and seas. In a view of all these problems, it is surprising that the model is able to simulate at least the right order of magnitude of ice volume change.

Page 184, line 11-12. “high surface albedo of ice and snow tends to reduces . . . surface melting”. Since there is nothing to melt in the absence of snow and ice, I would suggest to skip the last two words.

Page 184, lines 13-15. When discussing elevation feedback, the authors mention only sensible heat and write that this “is less important than absorption of solar radiation”. However, the role of the elevation feedback is quite comparable with the ice-albedo feedback for the growth of ice sheets. This is because elevation feedback results not only from sensible heat flux but also from a strong decrease of downward long-wave radiation with elevation.

Page 185, line 1-3. “substantial areas are converted to ice-sheet surface conditions in FAMOUS within the first two decades . . . because of positive SMB in Glimmer”. This sounds like Glimmer simulates positive mass balance over the entire FAMOUS grid cell. In fact, the whole FAMOUS grid cell can be converted into ice-covered state by a single Glimmer’s “pixel” with positive mass balance. This potentially serious caveat requires more substantial discussion.

Page 185, lines 17-20. A small warming over North Atlantic is explained by an enhancement of the AMOC which, in turn, is explained by a cooling over North Atlantic. This is not logical.

Section “5.2 Ice sheet mass balance”. In this section the authors use without distinction two types of mass balance characteristics: (i) accumulation and ablation integrated over the fixed domain and (ii) SMB and calving simulated for the real ice sheets. This causes confusion and leads to incorrect conclusion. On the page 187 (second para.) the authors write that “SMB increases are dominated by the effect of cooling in reducing ablation, which in general outweighs reduction in precipitation”. However in the next paragraph, the authors write that “the area-integral precipitation increases by about 35% in Laurentia and 25% in Fennoscandia”, i.e., contrary to the previous statement, precipitation does not decrease. Moreover, in fact, real ablation also does not decrease. Indeed, the area-integral ablation is completely irrelevant for the mass balance of the ice sheets because it is dominated by the potential ablation in the areas where there is no ice sheet. As shown in Fig. 14, the real ablation increases with area and, therefore, with time. Hence the ice sheet ablation has the same temporal dynamics as the calving and also contributes to cessation of the ice sheets growth.

Page 188, line 16, 17. Discussion in the text does not agree with Fig. 14. Apparently, names of the ice sheets are wrong either in the text or in the figure.

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