

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

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We thank the referees for their detailed and careful comments, which we believe have helped us to improve the paper. We are pleased that they see value in this work and think that it could be suitable for publication in “Climate of the Past”. A revised manuscript showing the changes with respect to the original submission is attached as a supplement to this comment.

Both referees express concerns about aspects of the experimental design which make the simulations less realistic. We agree with the referees, being aware of these limitations ourselves, and it appears from their comments that we did not describe the implications sufficiently thoroughly in the manuscript. In the revised version, therefore, we have tried to be clearer from the outset that this study does not aim at a com-

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pletely realistic simulation of the growth of ice sheets throughout the glacial cycle. This version of the model is not adequate for that purpose, owing to (among other things) our restriction to two domains within the Northern Hemisphere, the fixed sea-level and coastline, and the omission of a treatment of ice-shelves and marine-based ice-sheets. Coupling an AOGCM interactively to an ice-sheet model presents new technical and scientific challenges, and we would like to learn to walk before we try to run.

Consequently we set ourselves the limited objective of studying the interaction between the ice-sheets and regional climate, which is an aspect in which the use of an AOGCM is particularly relevant. We fixed other boundary conditions, including the atmospheric composition, orbital forcing and bedrock altitude, in order to simplify the problem and focus on this limited objective. We agree of course that they must all be included in a complete solution. The present work takes a step towards that objective. We are working on improving the model in order to remove some of the limitations of this work, as advocated by Referee 1.

In order to make our intentions clear, we have added a new paragraph to the introduction and a sentence in the conclusions, along the lines of the above comments. Following the recommendation of referee 2 that we should explain the strengths of FAMOUS–Glimmer for this work, we have moved a paragraph describing the relevant phenomena from a later section into the introduction. Throughout the paper, we have made other changes and additions in response to the comments, as follows.

Referee 1, General comments

1. We agree with the referee that to make progress in understanding glacial cycles we need a better SMB model than the PDD scheme. In the final paragraph of the conclusions, we describe our plans in this direction, citing various recent works. Nonetheless it is true that the PDD scheme is “convenient and well-established”, and it remains in use. At the point in the manuscript where we introduce the PDD scheme, we have

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changed the text to draw attention to the shortcomings of this method.

2. The formulation of the model does allow the ice sheet to spread into shallow water; it applies either a bathymetric or a flotation criterion to determine which Glimmer gridboxes contain grounded ice. In either case, however, the advance of the margin is really determined by the fall in sea level, which would have to be imposed as a time-dependent boundary condition. Our brief statement of this in the manuscript was evidently misleading, so we have expanded it. We agree that it is an unrealistic restriction, and have added remarks about it in section 4.4 and in the conclusions.

3. Yes, we have maximised the albedo feedback, as stated at the top of p179. As we say there, we made this choice in order to promote ice-sheet growth. (Note that FAMOUS gridboxes, of about 450 km spacing, are 500 times the area of Glimmer gridboxes, with 20 km spacing.) In one of the sensitivity tests of section 5.3, we minimise the albedo feedback, by suppressing it; this contrast is pointed out explicitly in section 3.3 of the revised MS and in the conclusions, where we note also that one aim of our current development work is to allow FAMOUS gridboxes to have partial ice-sheet cover so that we can treat this more realistically.

4. We chose those two domains for the ice-sheet because they were likely to be areas showing ice-sheet growth, in order to focus the analysis; we have added a remark on this in section 3.1. We have not run the model for other areas, and therefore do not know where else ice would grow. In that respect, as in several others, the work described is not an attempt to simulate the last glacial cycle comprehensively. In the project in which we are currently engaged, we will run Glimmer for the whole Northern Hemisphere.

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page 171 line 12. We have changed this to, “It is generally accepted that glacial cycles arise from variations in insolation . . .”, and hope that the referee agrees. To say they are “driven” by insolation might be misleading, given that feedbacks are much larger than the forcing, especially for the ~ 100 -ka cycle.

section 4.3. The uncoupled Glimmer does not have the albedo feedback, as the referee says, and it does produce less ice with the FAMOUS climate, but only by a factor of about 2, and therefore still too large by a factor of 5 in Laurentia and by orders of magnitude in Fennoscandia. Unfortunately the comparison between coupled and uncoupled Glimmer is not a “clean” one, because we used a different σ_{dd} when using a climatology to force the uncoupled model. With the data that we have available, we therefore cannot be conclusive, but we think this evidence suggests that the albedo feedback is not the main reason for the excessive ice volume in the present-day climate, although it may exaggerate it, as we have noted at the end of the section. The SMB scheme does not *necessarily* exaggerate a positive SMB, but it can do so in various ways, one of which is described in this section: it assumes a sinusoidal shape for the annual cycle, which is not a perfect fit, and in this case apparently tends to bias the summer temperatures on the cool side.

page 182 line 18. The sentence could be misunderstood because of the bracket interrupting it—sorry. We have changed it to, “The areas of positive SMB (Fig. 7a) are relatively cold.” It contrasts with the later point that the area of positive SMB in southern Scandinavia is due to high precipitation.

page 182 line 24. Born et al. (2010) ran simulations with SICOPOLIS, a three-dimensional thermomechanical ice-sheet model.

page 183 lines 5–10. We have removed the sentence about $\delta^{18}\text{O}$; this was based on a remark by Bintanja et al., but we may have misunderstood the matter and therefore ac-

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cept the referee's view instead. Yes, the uncertainty in SMB refers to the PDD scheme, and this is clarified in the revised manuscript. We have also inserted a sentence about the fixed coastline (following general comment 2).

page 184 lines 11–12. Quite right, thanks.

page 184 lines 13–15. We have changed the text accordingly, thanks.

page 185 lines 1–3. We have reworded this point and inserted a reference to the earlier discussion of section 3.3. Please see also general comment 3.

page 185 lines 17–20. This may sound surprising but we do not think it is illogical. The point is that the air temperature change and the SST change are not the same; the air blowing over is colder and the SST warmer.

section 5.2. We intended to make a clear distinction between these two views of mass balance, namely the integrals over fixed area and over ice sheet area. The former shows more obviously how the sums add up, and which terms reach a steady state first. We agree that accumulation and ablation integrated over fixed areas include fluxes over ice-free regions which cancel out and do not affect the area-integral SMB or the ice-sheet mass. However, this discussion was evidently confusing so for the sake of simplicity we removed accumulation and ablation from Figure 13 and deleted a paragraph of this discussion.

page 188 lines 16–17. The plot was incorrect—apologies and thanks for noticing. The black lines in the two panels had been exchanged!

Referee 2, general comments

Asynchronous coupling. There are advantages of technical convenience and efficiency in coupling the models if information is to be exchanged frequently i.e. climate information from FAMOUS to Glimmer, and topography and land surface information

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from Glimmer to FAMOUS. This coupling could be done as a manually initiated separate step whenever required, but clearly this is tedious and time-wasting if it is frequent, which is the case with FAMOUS. We couple asynchronously by running 1 FAMOUS year as input for n Glimmer years. Hence asynchronous coupling does not greatly reduce the frequency of coupling in wall-clock time, since most of the CPU time is taken by FAMOUS. Even at $n = 100$, the information exchange occurs dozens of times per wall-clock day, and automating it is therefore necessary. We have inserted this remark in section 4.2 of the revised version.

Although we think that $n = 100$ does not significantly distort the time-mean evolution since the climate and ice-sheets are evolving relatively slowly at inception, there are periods when it would not be acceptable, such as deglaciation. For inception, Calov et al. (2009) show that an acceleration of 1000 produces a serious distortion in the rate of ice-sheet change, and Herrington and Poulsen (2012) show a substantial dependence on the interval between coupling in ice-sheet years for intervals they use, which are of 500 years and longer. We have added a comment about this in section 4.2 of the revised version. We would therefore not like to use a greater acceleration than 100 since it could make a significant difference to the ice-sheet development. With $n \leq 100$ it is unavoidable that coupling will have to be carried out hundreds of times during an integration of tens of millennia.

GHGs. According to the Vostok record, the CO₂ concentration at 115 ka BP was about 270 ppm. We actually use the standard FAMOUS pre-industrial value of 290 ppm because we wanted to simplify the experimental design by changing as few boundary conditions as possible, and the difference between these two would produce only a small radiative forcing (0.4 W m⁻²), which would have a much less important effect on regional climate than the orbital forcing does. We kept the GHG concentrations constant throughout the experiments because our focus was on ice-sheet–climate interactions, and we therefore wished to fix other boundary conditions. We agree of course that in a realistic simulation the evolution of GHGs would affect the climate experienced

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by the ice-sheets. We have not experimented with lowered GHGs in combination with incipient glacial orbital forcing. However, we know from other studies that FAMOUS has a climate sensitivity somewhat higher than that of HadCM3 (in FAMOUS-actan, used in this study, the equilibrium climate sensitivity is 4.2 K) and that the cooling due to GHG forcing with respect to the last interglacial was small at 115 ka BP (Smith and Gregory, 2012).

Choice of regions. Please see also reply to Referee 1, general comment 4. Referee 2 is right that we should not imply that the Canadian Arctic was necessarily the only site of inception of the Laurentide ice-sheet; that was due to our use of “Laurentide” for the restricted model domain as well, and we have amended the abstract and conclusions.

Fixed coastline. We decided not to change sea-level and the coastline because of our limited focus in this study on ice-sheet–climate interaction, and because it would have to be applied as an external boundary condition since we were not simulating all the ice-sheets. We have added remarks about this in section 4.4 and in the conclusions.

Ice-shelf dynamics. We agree that this is necessary for a realistic simulation, especially later in the glacial cycle when there is a large area of ice-sheet on the continental shelves, as the referee points out. Glimmer is currently being developed in order to address this need, because of its relevance to simulations of present and future ice-sheet dynamical change as well as past, and we hope to make use of these developments. Please see also our reply concerning page 191 lines 1–3 and to Referee 1, general comment 2.

Referee 2, major comments

page 175 lines 1–3. We have made the statement more specific, thus, “global-mean radiative forcing or SAT cannot predict glacial inception.” We accept the referee’s ex-

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planation of why global-mean SAT is higher at 115 ka BP, but we do not follow the argument. The point we are making here is that a model which related the growth of ice volume to global-mean temperature would not predict glacial inception when the global-mean temperature is warmer than the pre-industrial climate. In fact, glacial inception occurred before 115 ka BP, at a time when the greenhouse-gas concentrations were interglacial and the global-mean orbital forcing may have been positive, because “the relevant indicators are the much larger latitudinally and seasonally dependent changes in zonal-mean surface air temperature of either sign, corresponding to the changes in TOA insolation,” as the manuscript continues. Please see also our reply above to the general comment on GHGs.

page 175 lines 13–18. Yes, with the orbital forcing of 115 ka BP the global-mean SAT in HadSM3 is 0.23 K warmer than in its control.

page 175 lines 19–21. Thanks for this comment, which is useful because it indicates that the remark to which it refers was not in the right place. Yes, it is inconsistent because the FAMOUS and Glimmer simulations of SMB use different schemes and give different results. We use the Glimmer scheme for ice-sheet growth because of its higher resolution and because it is designed for this purpose, whereas the land-surface scheme of FAMOUS is not adequate to simulate ice-sheet SMB. Thus the inconsistency is a necessary aspect of the current version of the model. We have replaced the short paragraph in question with new sentences in sections 3.2 and 4.1. In the final steady-state climate, the FAMOUS gridboxes in Laurentia and Fennoscandia with the highest altitude and the highest land fraction do have perennial snow accumulation, but most of the area occupied by Glimmer ice-sheets still does not have snow accumulation in FAMOUS. We are addressing the inconsistency and the inadequacy of FAMOUS for ice-sheet SMB in our current model development, as mentioned at the end of the conclusions.

page 175–176 first paragraph. We agree that this is a drawback for a fully comprehensive and realistic simulation. Please see also our replies to the general comments

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on the fixed coastline and ice-sheet dynamics. Other simulations that we have seen show glacial inception independently on the mainland of Scandinavia and on Svalbard, but we agree that this issue has to be addressed in order to simulate the merging of ice-sheets initiated at those sites.

page 176 lines 9–19. We are aware that planetary waves can be affected by changes in topography, and indeed the effect on atmospheric circulation is described later in the manuscript. Although the two Glimmer domains are separate, they can influence each other via the atmosphere, which is global. We have not attempted Glimmer simulations in other areas where ice-sheets might grow, such as Siberia and Greenland. In the project in which we are currently engaged, we will run Glimmer for the whole Northern Hemisphere. Please see also our reply to referee 1, general comment 4.

pages 176 line 19. Isostatic adjustment can be neglected initially, as the referee says. We switched it off even throughout these long runs because we were making deliberate simplifications. There would be a climatic consequence of isostatic subsidence, but because of the viscous timescale, the effect would depend on the rate of ice accumulation, unlike the climate feedback, which responds quickly to ice-sheet changes; the relationship between these two would be a complication in the analysis of climate feedback.

page 177 line 7. The lapse rate of 8 K km⁻¹ is the default value in Glimmer (Rutt et al., 2009). This value is used also, for instance, by Zweck and Huybrechts (2005), throughout the glacial cycle. We did not repeat the coupled experiments with other choices for the lapse rate, but we did carry out some sensitivity tests with Glimmer alone. This is the basis for the statement a couple of paragraphs later that the results vary by tens of percent for variations of the SMB parameters. For the lapse rate specifically, there is a greater sensitivity in Laurentia than in Fennoscandia. With 6 K km⁻¹ the SMB for the present-day climate is reduced by 20% in Laurentia and 60% in Fennoscandia, so we would expect the rate of ice-sheet growth to be correspondingly reduced. Of course, the use of a lapse rate which is constant in space and time is also a severe simplifi-

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cation. In future work we intend not to use the PDD scheme in order to avoid such sensitive dependence on poorly constrained parameters.

page 181 lines 9–11. Please see our reply to the general comment on asynchronous coupling.

page 181 lines 13–18. We agree that glaciers and ice-caps are not in a steady state today, and have varied throughout the Holocene. Nonetheless the reduction in ice volume in the last 150 years in these regions is by less than an order of magnitude, and hence does not explain the much larger ice volumes simulated by FAMOUS–Glimmer.

page 182 lines 10–13. This comment does not appear to be in disagreement with our statement, which concerns our ability to simulate ice-sheet–climate interactions. The referee is concerned that the climate model may be unable to simulate the climate evolution throughout the glacial cycle. We agree that there is a plenty of evidence for inadequacies in AOGCMs, even in their simulation of present-day climate. It is harder to assess how present-day biases relate to errors in simulating climate change over millennial timescales because of the limited proxy evidence available. Smith and Gregory (2012) have assessed the simulation by FAMOUS (without Glimmer) of large-scale measures of climate change during the last glacial cycle, and find that it performs well on several measures. There are some well-known problems, such as the absence of variability like Dansgaard–Oeschger events, which may be related to ice-sheet coupling.

page 183 lines 4–14. We agree and have added remarks about this in section 4.4 and in the conclusions.

page 185 lines 20–22. We agree; we do not see a need to change the text here.

page 191 lines 1–3. We have added a sentence later in the conclusions about the need for treatments of ice-sheet dynamics and ice-shelves, following other comments by both referees. We do not think that models currently available provide an ade-

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quate representation of ice-sheet–ocean coupling, which involves sub-ice-shelf ocean circulation, basal melting/freezing, and ice-stream dynamics including grounding-line migration; models of these require high resolution and detailed parametrisations, and are in active development by several groups. That was not our aim in this work, in which we concentrate on ice-sheet interaction with surface and atmospheric climate change. Please see our response to the general comment on GHGs.

Referee 2, minor comments

page 177 line 11–13. Accumulation occurs preferentially at higher altitude, but the FAMOUS gridbox has only one altitude, namely the area-average. Yes, treating all the precipitation as snowfall may give an overestimate for the accumulation areas, but using the snowfall alone would probably give an underestimate, because the rain fraction will be smaller at high altitude. To address this we could use a temperature-dependent rain fraction. We prefer instead to replace the present model with a subgrid hypsometric treatment in FAMOUS, as mentioned in the conclusions.

page 182 lines 1–3. The resolution of Glimmer is much higher than both HadSM3 and FAMOUS. Please see our reply to page 175 lines 13–18 on the climate change.

With a couple of exceptions, we have made the other editorial corrections suggested, thank you.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/8/C548/2012/cpd-8-C548-2012-supplement.pdf>

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