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Geoscientific Model Development Discussions

Interactive comment on "Ice sheet dynamics within an Earth system model: coupling and first results on ice stability and ocean circulation" *by* D. Barbi et al.

Anonymous Referee #2

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The manuscript presented concentrate on the important issue of the coupling of a somewhat low resolution model with a high resolution ice-sheet model. This type of manuscript is important since all further work involving the use of the coupled climate ice-sheet model depends on the quality of the presentation, on the justification of the method and on the verification and testing of the model results.
As it currently stands, I consider the manuscript to be far from being in a stage acceptable for publication in Geoscientific Model Development. The model coupling procedure is not described in details sufficient to re-implement it as required by GMD (for example the PDD formulation used is not given, how is meltwater treated with respect to the ocean, how is the water closure dealt with within the complete COSMOS

+ RIMBAY model), the initial states of the models are not described in sufficient details and the validation against present-day measurments is not really convincing. Before advancing into the effect of the ice-sheet melting on the oceanic circulation (an important subject that would need much more than one page (p. 15) of description of the figures provided but a much more in depth analysis of the mechanisms, the authors should concentrate on improving the description and testing of the control runs with and without ice-sheet.
An example to summarize the disappointing impression given by the manuscript: I still wonder after reading it carefully whether an interactive Antarctic ice-sheet is included or not? If yes, why no figures whatsoever are given? Why no validation is given for that part of the model as well? If it is not included, then some parts of the description are rather misleading.
I therefore recommend major revision of the manuscript before the publication be considered in GMD.

Detailed comments

Page 2,line 17-18: no ice-sheet are not always represented by rule-of-thumb parametrizations. Some experiments make use of different ice-sheet geometries prescribed (e.g.: PMIP coordinated experiments), some already have coupled ice-sheet components (cf. Ganopolski Calov, 2011), some use parametrizations for ice-sheet melting (Swingedouw et al., 2009).
Page 4, line 15: "was operated" -> "is operated"
Page 4, line 15: "in the case of paleo-climate simulations" -> this is not the case here, what are you trying to say?
Page 4, line 16: "ice-sheet model should not be run on grids coarser than" -> Why not? Reference for such a bold statement?
Page 4, line 18-20: "Our work shows ..." I do not think that you do show that. What

you show is that you obtain a stable Greenland ice-sheet with your parametrization, not that it is needed to be stable.
Page 5, line 13: "nuntaks" -> "Nunataks"
Page 5, line 20 and elsewhere when you discuss the starting up of the simulations: what are the temperature profile used in the ice-sheet? It is one of the most important boundary conditions for the ice-sheet. If not correctly set, it might need 15,000 years to re-equilibrate in my experience.
Page 5, line 19: "close inspection" : this is a standard procedure in all GCMs to my knowledge. Why do you need close inspection for that?
Page 5, line 20-25: you should detail the alterations to the HD model at length. What are the mass balances you are talking about on the two models? On the two grids?
Page 5, line 24-26: why don't you need changes in the ocean model? How is meltwater dealt with there?
Page 6, line 6: "initial ice-sheet distribution" -> what is the initial vertical temperature profile used?
Page 7 line 24-29: I do not understand what is done there.
Page 8, line 5-6: "the difference between net accumulation ... of the ice thickness" -> changes in the flow of ice also alter ice thickness locally.
Page 8, line 14-16: I do not understand how the changes in topography are only feedback to the geopotential height? I assume that there is an orography in ECHAM? Please detail what is done.
Page 9: in your downscaling procedure, why don't you downscale the limit between liquid and solid precipitation? If you change the vertical temperature profile, this should change as well for consistency.
Page 9, line 17: "both models agree on this value" -> not both models (you do not model twice the same thing in different grids) but the two fields on the different grids.
Page 9, line 21-24: I have no clue about what you are doing here between northern and southern Greenland. Do you include specific localized spatial limits? If yes why and how?

Page 9, line 25 : Surely Antarctica can't remain stable under the 1000 ppm scenario. Please show all results for Antarctica to support those assertions.
Page 10: PDD: Please give the PDD formulation, there are several PDD possible (cf. Charbit et al., 2012). Why are you using a sine monthly seasonnal cycle and not ECHAM's? This would surely be much better in terms of consistency between the two models?
Page 11, line 1-11: what is the surface gradient you are talking about? Surface slope? Surface humidity gradient? Page 11, line 10: "The result ... the total accumulation is conserved" -> Surely this is not done over all Greenland but locally on the T31 grid cell? Same question for temperature (page 11, line 20-22). Page 12, line 12: "distribitions"
Page 12, line 13: What is a "stable" (between quotes) GCM-state?
Page 12, line 16-17: I have no idea about what you are doing there!! Are you taking the mass balance from one run and using them in another run?
Page 13 - all figures: please show differences to the reference states you chose to compare your simulation to (i.e. climatology or regional model result), only keep absolute values for the reference chosen.
Page 13, line 13-15: surely winds transport is computed in ECHAM? What are you trying to explain?
Page 13, line 19: Please give global ice volume values and the time series of it to give a precise icea of the state of ice-sheet you are obtaining.
Page 4, Antarctica: Please include all results for the reader (figures).
Page 15: you only describe the figures with your result, but there is no analysis of the mechanisms. For example, how much freshwater is added to the ocean and where? What is the time evolution of both ice-sheet under the 1000 ppm scenario?
Page 15: are you runnning with Antarctica activated? If yes how much freshwater is added to the ocean there as well? Page 15 bottom and page 16 top: what do you conclude from the analysis of the sea-ice changes at the beginning of your simulation? It is quite odd to compare that with the final product of your simulation for the other

fields.
Page 16, line 5-6: This is wrong. The anomaly in your figure depict changes in AAIW circulation, not in the upper branch of the AMOC. There is no changes in the North Atlantic, which is at odd with other melting scenarios (cf. CMIP5).
Page 17: your discussion should be centred on the quality and weaknesses of your model with respect to the data, as well as on the interpretation of results if any.
Page 17, line 3: "ansatz" ? The manuscript should be in English language.
Page 17-18 : Greenland / Antarctica : update these sections when considering all other comments.
Page 18, line 7: how can Antarctica be insensitive to 1000 ppm? Where is the changes in AAIW depicted come from then?
Page 35, figure 10: what is depicted: 3b - 3a or the opposite?

References cited

A. Ganopolski and R. Calov, The role of orbital forcing, carbon dioxide and regolith in 100 kyr glacial cycles, Clim. Past, 7, 1415-1425, 2011
Swingedouw, D. et al., Journal of Climate, 2009, vol. 22
S. Charbit et al., Influence of ablation-related processes in the built-up of simulated Northern Hemisphere ice sheets during the last glacial cycle, The Cryosphere Discuss., 6, 4897-4938, 2012

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