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The Cryosphere Discussions

Interactive comment on "Large sensitivity of a Greenland ice sheet model to atmospheric forcing fields" *by* A. Quiquet et al.

Anonymous Referee #2

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Quiquet et al. describe ice sheet model experiments using a set of different climate model forcing fields (temperature and precipitation). They show that the outcome of ice sheet model experiment is strongly influenced by the climate forcing. Next to that, they study the reasons behind regionally different ice sheet model responses, and manage to attribute these regional differences to either temperature or precipitation anomalies, thereby differentiating the ice sheet sensitivity to these forcing parameters.

The model set-up, initialization procedure, input fields are all well described, just as the experiments. This is a comprehensive assessment of the large influence of climate forcing fields on ice sheet model results, which is an important result.

I do have some points of criticism, which are listed below. In my view, this work is a valuable contribution within the theme of ice sheet – climate model integration, and

therefore should be published, but the paper can be improved if the below-mentioned issues are addressed.

Comments:

Paragraph 2.1 How is ice discharge (by calving) described? Presumably this uses some floatation criterion. Later in the manuscript, when the different responses between the North and South of the ice sheet are discussed, this may be important, since the Southern ice sheet margin is for a large part in contact with the ocean, thereby allowing a significant mass loss term by ice discharge. Does this explain the low sensitivity for temperature of the southern part of the ice sheet?

Paragraph 2.3 The approach to calculate SMB from the input fields (precipitation and near surface temperature): a degree day approach is followed to calculate melt. The authors seem to justify this choice by saying that since the downscaling procedures used for temperature and precipitation is physically based, and SMB downscaling is not, computing SMB from a PDD method is a logical choice. However, SMB computations from regional climate models is also physically based, and it has been shown that a PDD approach cannot exactly reproduce such field (Van den Broeke et al. 2010, GRL 37, L18501; Van de Berg et al, 2011, Nature Geoscience 4 ; Helsen et al, 2012, The Cryosphere 6). It should be better emphasized that the choice for a PDD approach is merely practical, since most climate models do not produce SMB, only precipitation and temperature. Next to that, an effect of the use of a PDD approach is that it overestimates the climate sensitivity (Van de Wal, 1996). How does the choice of the PDD approach influence the results? Would an alternative SMB calculation, such as explicitly taking into account both temperature and insolation (Pollard, 1980; Oerlemans, 2001; Van den Berg, 2008), energy-moisture balance (Robinson, 2010) or SMB gradients (Helsen et al, 2012) lead to significant different results? This subject deserves more discussion.

Paragraph 2.4.1 The dynamic calibration of the ice sheet model is well-described, and

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reasonable choices are made in this process. However, it should be noted that the dynamic calibration is carried out using the FE09 forcing fields. As such, any different forcing field will result in an immediate response of a changing ice sheet, which is also noted at the end of this paragraph. After performing the experiments with different forcing fields, conclusions are drawn that using those different forcings results in different ice sheet volumes compared to the FE09 reference forcing. It should be noted however that in practice a coupled ice sheet – climate experiment would normally involve a dynamic tuning of the ice sheet model using that particular climate model data. As such, it may well be possible to arrive with a comparable ice sheet (as in this study with the FE09 forcing) with different settings of e.g. the ice rheology parameters. It is not feasible to perform every experiment using different (tuned) dynamical parameters, but this issue deserves some attention in the text.

Paragraph 3.4 The conclusion that temperature is the major driver of ISM behavior may also be the consequence of the very low precipitation in all climatologies in the north (which is also reality). Perhaps the (too?) strong response to temperature perturbations also has to do with the choice for a PDD approach to calculate melt.

Paragraph 3.5 aims to describe the sensitivity of the results to the topographic lapse rate. However, only experiments are carried out without any adjustment of precipitation and temperature as a response of topographic changes. This obviously leads to drastic differences. It would be more valuable to show results obtained with different values of the lapse rates.

In general, the language needs improvements.

Minor suggestions:

Page 1038, line 24: ice sheet models (plural)

Page 1040, line 4-5: rewrite, this is not clear what is meant.

Page 1043: the model's surface topography is not really an input field, it evolves

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through time (and as such is actually an output of the ice sheet model), but it is used to calculate near-surface temperature and precipitation. Rewrite.

Page 1045 line 9: by construction? Unclear. Do you mean by definition?

Page 1049, line 11-15: Do you use a spatially uniform temperature perturbation during the glacial-interglacial spin up ? Please be more specific on this.

Interactive comment on The Cryosphere Discuss., 6, 1037, 2012.