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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.

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We appreciated your valuable comments which helped us to provide an improved version of the manuscript. In the following we will answer to each individual of the referee's comments. Referee's comments are identified by RC and authors' by AC.

RC: 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.

RC: 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.

RC: 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:

RC: 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?

AC: Calving is parameterized with a simple cut-off based on a threshold on the ice thickness. This threshold is spatially uniform but is time dependent. Its value varies with the surface temperature anomaly used in the spin up experiment. For present climate the threshold is 250 m and, in most places, this criterium does not allow any floating tongues at the scale of the grid box (15 kmx15 km), due to warm conditions and flow divergence. It means that when ice automatically calves when arrives to the ocean (flotation criteria indeed). We think that the absence of ice shelves under present day climate is realistic, because ice tongues of this dimension (15 km squared) are not observed in present day GIS. We can mention however that we do generate ice shelves during the glacial spin up. On the other hand, in our model, present day simulated ice sheet is insensitive to calving parameterization (sensitivity tests were performed during spin up experiments). We agree that for realistic future projections a good parameterization of the calving is important, but a finer grid resolution is then required.

To address your comment we estimate that calving is probably not responsible for the low sensitivity of the southern part of the ice sheet. This sensitivity is more likely due to high precipitation rate in this area associated with the fact that the major glaciers flow in narrow fjords and are not perfectly taken into account by the model leading to a thickening trend that make the region less sensitive to temperature. In the new version of the manuscript we added more information about the calving parameterization.

RC: 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.

AC: Without considering any other variables (insolation, long-wave radiation etc.), the only method to downscale the SMB is to use the gradient method used by Helsen et al. (2012). However you make the point, this method is only feasible with high resolution RCMs, which use a good snow scheme, and not for GCMs. PDD approach may overestimate the climate sensitivity (Van de Wal, 1996) but it cannot be considered as a systematic deficiency. Indeed, the PDD method uses 2 m temperature from climate models. Over any melting snow or ice surface, including the ablation area, surface temperature is limited to 0°C when the GCM grid cell is completely snow or ice covered. Therefore, all climate models which have a resolution which is fine enough to have several grid cells completely over the ice-sheet, should simulate 2 m temperatures very close to the melting point during surface melting conditions, even when very warm conditions prevail above the atmospheric boundary layer. The higher the resolution of the model, the more efficient is this process because the ice surface over the Plateau is flatter, allowing the surface boundary layer to fully develop in the GCM. This effect may conduct to an underestimation of melting by PDD, in contrast with the study of Van de Wal (1996). Considering that we can not conclude firmly about the effect of taking more sophisticated SMB model, further systematic studies aiming at reducing these uncertainties would be really valuable for the community. We can think about steady state temperature scenarios as well as cooling and warming, applied to different SMB computation methods.

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

AC: Your comment was also addressed by Referee1 and following is our previous response: The calibration/initialization of an ISM is a difficult problem that would require assimilation methods to be rigorously solved (Arthern and Gudmansson, Journal of Glaciology, 2010). However, this problem can be split into two parts depending on the considered variables. Ice velocity is a diagnostic variable which depends on surface and bedrock topography, 3D temperature field, basal drag and ice deformation properties but does not depend directly on surface mass balance. It is thus possible to calibrate ice flow parameters using only observed ice velocities. It is the approach we chose in this article and indeed it made our calibration almost independent on the atmospheric forcing fields. "Almost" stands for the impact of the temperature field that is a prognostic variable and thus depends on the past ice sheet evolution, past surface temperature and geothermal heat flux. However, we estimate that this effect is of second order compared to the atmospheric field impact (although it may change the value of the calibrating parameters to fit observed velocities). To answer more directly the question, we believe it would indeed be possible to tune the parameters (e.g. basal drag) for each forcing field to try to produce the present ice sheet geometry and observed trend in surface elevation but i) there are many atmospheric models for which it will not work because they have too strong bias ii) there is the risk to produce a velocity field very different from the observed one iii) it will be very difficult to compare atmospheric models iv) it is far beyond the aim of this paper. We improve the text in order to give more information on this point.

RC: 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.

AC: You are right, North is a very dry region (relatively well reproduced in all climatologies used), and that is maybe why this region is such sensitive to temperature. However models have biases in temperature as well as in precipitation and it is interesting to see that the temperature bias seems to play a dominant role in this region. Your comment about the PDD method points to an important issue. As we mentioned in the text, different ISM model experiments in the literature suggest than the South could be subject to more drastic changes than the North is. But others suggest the contrary. The method of SMB calculation could be an explanation but there is no agreement within the scientific community to address this issue. Specific studies are again highly needed here.

RC: 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.

AC: The section dealing with the lapse rate is now entitled “Importance of the feedback from surface elevation changes”. The aim of this section is to assess the importance of taking into account the elevation changes feedback. Indeed, in a case where ISMs are not included in future projections of sea level rise for example, how large is the error? We realized these experiments to answer this issue. We added the following statement at the very beginning of this section: “Sea level rise projections generally use complex climate models with fine resolution and/or sophisticated physics. ISMs are not yet included in these models and in this section we want to assess the importance of including the elevation changes feedback on temperature and precipitation for the ISM response.” Once again, sensitivity to parameters is not the focus of this study even if it is an important issue of course.

RC: In general, the language needs improvements.

AC: The text has been revised by a native english speaker.

Minor suggestions:

TCD

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

AC: Done.

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

AC: We hope that we fixed it now.

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

AC: We have tried to be clearer in the new version.

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

AC: Text has been completed with the following: "The AOGCMs simulate atmospheric surface conditions in interaction with their ocean and sea ice components and with no or little external sources of variability. Hence, the simulated time series can not be expected to correlate with observed variables as in the atmosphere-only models with observation-derived lower boundary conditions."

RC: 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.

AC: Yes, we do. The perturbation is deduced from the d18O of the GRIP record, converted to temperature with a paleo thermometer assumption, using a constant slope of 0.42 ‰°C-1 (Huybrechts, 2002). The temperature perturbation is indeed applied uniformly. We added information in the text.

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