

Interactive comment on “Estimating Greenland ice sheet surface mass balance contribution to future sea level rise using the regional atmospheric climate model MAR” by X. Fettweis et al.

Anonymous Referee #1

Received and published: 30 August 2012

The authors address the issue of the future surface mass balance of Greenland through experiments using 2 RCMs forced at the boundary by GCMs from CMIP5. They attempt to determine if the uncertainty in projected SMB can be reduced if only GCMs, which they judge to perform well for the recent past, are used.

The paper represents an incremental advance on Rae et al (2012).

However, the paper is poorly presented both in the objective of the study and the internal logic towards a conclusion. It does not maintain a consistent storyline and so becomes very difficult to read. It would appear that the authors have produced a brain-dump from their RCM simulations and hoped that the mass of information would pro-

C1305

duce a viable paper.

A major rewrite and rethink of the intended message and its presentation is required. I have made a number of suggestions as to changing the information content and style, but not wanting to rewrite the paper myself, I hope the authors can apply similar modifications throughout.

Detailed comments

Abstract:

The abstract is too complex – it is not necessary to cover every aspect of the paper here. A bad idea to have a list embedded in the abstract – a list which adds nothing to the headline conclusions. Suggest rewriting to express the motivation (missing), method (first sentence), and headline conclusion (lines 21-25) with implications.

Introduction:

3103-12:14. What about the impact of increased liquid precipitation?

3103-22:23. Redundant. Reduce this sentence to “Mass loss from ice calving is estimated to be roughly that from SMB van den Broeke et al., 2009; Rignot et al., 2011).”

3103-22:24 Suggest changing this to “The dynamical response of the ice sheet to surface melt reaching the bedrock (Zwally et al., 2002) is still uncertain. However, recent observations suggest it is not a significant component of the total uncertainty in future sea level rise (Sundal et al., 2011; Rignot et al., 2011). The acceleration in flow of tidewater glaciers, due to large melting at the calving front, is expected to decline in future as the glaciers retreat above sea level (ref).”

3103-27. Reword this confusing sentence, perhaps “A preliminary objective, addressed in this study, is to provide the best possible estimate of future surface mass balance and associated surface freshwater runoff. Such runoff contributes both to sea level rise and may eventually affect the North Atlantic thermohaline circulation (Swinge-

C1306

douw et al., 2009).”

3104-13:15. Although it is true that GCMs do not include snow-firn-ice physics, this is not to computational load. The snow scheme has to work for seasonal snow cover (important for regional water resources) and all vegetation types, it does not follow that the snow schemes are inferior. In addition the ice sheets are often fudged to avoid ocean salinity drift, so providing better understanding of large-scale changes to the global hydrological cycle.

3104-16:17. Rephrase this. RCMs contain neither feedbacks on ice dynamics nor impacts on THC, so they are not ‘ideal’ tools. Be more specific about the purpose of RCM’s in solving overall mass balance ‘issues’. As GCM resolution increases (examples in AR5) RCM’s will become redundant. You mention in 3106-13:17 that there is qualitatively no difference between 15 & 50 km resolution, so a more consistent storyline is required. . . RCMs can assist the development of GCMs specifically that of snow-firn-ice schemes (actually in CESM for AR5 but now being incorporated in many others).

3104-26. But MAR was used in Rae et al (2012), so this study is not uniquely different.

3105-19:22. Rearrange this sentence to avoid repeated use of the word ‘module’. What is meant by ‘integrated surface albedo’ is it an emergent characteristic of the proceeding components (for example using a delta-eddington scattering model) or is it merely an empirically tuned expression of grain size and wetness?

3105-22:23. Remove reference to blowing snow model. That it exists is not relevant and in any case is negligible (see 3103-9).

3105-23:26. Simplify. Suggest: “SISVAT does not include a 3D ice sheet model and consequently Greenland maintains a fixed height and extent through the simulations”

3106-7: Either explain here why orographic smoothing is needed, or don’t mention it at all (it is irrelevant to the processes described in the paper).

C1307

3106-15:17. Only one resolution is used throughout the paper, why mention that the parameterisations are resolution independent. There is no reason to suppose that any of the surface components outlined on 3105 should be scale dependent.. All this statement tells us is that you don’t need high resolution to model the ice sheet SMB – perhaps a subject to be presented in the conclusions.

3107-5-15. Rearrange this section to start with verification of MAR-era interim for the various processes which then supports its use as a reference simulation throughout the analysis. A caveat that the relatively short ERA-interim timeseries could contain significant decadal variability with cannot be temporally matched to GCM forced simulations (which will all have different phases of the decadal variability over this period.). One way to reduce this ‘observational bias’ is to look at the smoothed NAO index for ERA and GCM to determine if one would expect a difference from a circulation perspective. Analysing a GCM just over the very small area of Greenland for just 20 years is no indication at all that the GCM will perform well in the future. The GCM needs to verify well globally to have any confidence that patterns of change – teleconnections – are valid. The models which ‘perform well’ over Greenland are actually rather poor globally. For example, CanESM2 depicts that we have already lost the Arctic summer sea ice.

3107-13:16. The pseudo – endorsement of MAR from Rae et al., against just 2 other RCMs, is unnecessary and should be deleted. No mention at all is required of RACMO2 here. Reference it when relevant later. I suggest removing all text between ‘In addition at a resolution of 11km” to prevent drift in the storyline of the paper.

3107-25:26. Not clear why ECMWF-forced simulations are mentioned in a paragraph about future projections. Rearrange.

3108-2. A radiative forcing of 4.5 Wm² at 2100 is of course the definition of the RCP.

3108-5. Replace ‘pessimistic’ with the less judgemental ‘high-end’. These are “repre-

C1308

sentative pathways” not “predicted pathways”.

3108-9:11. It would have been more interesting to include RCP 2.6 as this is a strong mitigation pathway and might indicate that such a policy trajectory would prevent a –ve SMB.

3108-17. What about the 60's & 70's is comparable to 1980-1999?

3108-20:21. Why is this a problem? Would this not help verify the icesheet behaviour with warming (partially related to a change in NAO).

3108-22:26. A good representation of climate over Greenland is no guarantee at all that the large-scale circulation changes with global warming will be any better than a random guess for the future. As mentioned before, regional patterns will change depending on ocean heat uptake, over turning circulation, cloud phase change etc. which means that present day climate of a small region like Greenland will sufferer from large multi-decadal variability. This paragraph needs to qualify statements within this context.

3109-2. Replace 'well-known' with a reference.

3109-5. ‘..are obviously.’ only if they lie outside the multi-decadal variability – either inferred from the GCM control simulation or an analysis of multiple reanalysis from 1950's.

3109-15. Remove 'and 600 hPa' as this is not relevant here.

3109-20:29. The separation of windspeed and direction is strange as advection of warm air depends on the source of the air and so wind direction is important as well. The circulation over Greenland is dependent in the position and strength of the polar vortex in the model. It also depends on the Froude number of its interaction over the orography which is RCM dependent.

3110-8. Remove 'indeed', its use is misplaced here.

C1309

3110-11:29. The formulation of the argument is muddled. I suggest restructuring such that you are assessing a number of CMIP5 models. It should not be relevant that they are meteorologically the 'best'. As detailed below, this is no guarantee that they will perform well in the future scenarios.

I am unable to assess the methodology used to evaluate the circulation regime (paper submitted). I presume it uses some form of self-organised mapping technique (cluster approach). To restrict this to Greenland itself assumes that Greenland exists without teleconnections to the rest of the global system. That some models cluster well against a short term reanalysis, only indicates that the driving GCMs happen to have an NAO and PDO aligned with the present day. Since the climate models are free running, this is unlikely to be the case for most of them. This does not indicate that they would perform better in the future simulations. The models selected here are particularly bad on the global scale which suggests that there may be serious issues with their parameterisations or process physics.

3111-3:8. Restructure such that the purpose of the table/figure is defined before it is referenced in the text. E.g. 'The SBM describes the integral response of the ice sheet to the climatic forcing. To identify differences in the forcing we break the SMB down into its components (Table 2).' Table 2. Remove reference to blowing snow in caption.

3111-3:8. Describe how the errors on Table 2 are derived and their significance (2 sigma?). I assume these are not just interannual variability, but rather standard errors of the means.

3111-10:16. What is the purpose of showing MARv1, if the difference between v1 and v2 is just the ice mask? The less irrelevant data you present, the simpler your argument, and the easier the paper is to read.

3111-17:21. Why? Is this due to a misplaced gulf-stream or smoothed orography, ill positioned centre of the NAO, spectral model? I know you do not want to get into adetailed analyses of each GCM, but what is the significance of this description?

C1310

3111-27. Suggest changing to 'In addition to its impact on SMB, a low winter snowfall results in an earlier exposure of bare ice during summer melt, and since ice has a lower albedo results in a higher ablation (Mote, 2003; Tedesco et al., 2011).

3112-4:'south-westerly flow'

3111-3113. Much of this descriptive reasoning for GCM 'biases' is by inference. Showing GCM moisture and heat convergence may be a more convincing approach. Also the annual mean circulation is depicted in fig 2, but you refer to winter precipitation, the circulation patterns for which cannot be inferred from an annual mean. Reference to 'common' circulation features in each section of the description is repetitive. Start the start of this section, a description of the climatology (from ERA-Interim) is needed (perhaps move the section on this from earlier in the paper). This description would be sufficient to set the scene when describing deviations in the GCMs from the reanalysis.

3113-23. Why is the mean annual cycle important? What does it tell us about the system? You are just throwing a lot of data at the reader and providing no context.

3114-15. Poor structure + needs context. I suggest .. 'An adequate depiction of the model observed interannual variability is an indicator that it will perform well under future simulations. The variability arises due to changes in the storm tracks, the NAO, the westerly winds, and sea ice conditions, which may combine to generate decadal variability. However, alignment of these components in GCMs is not likely to be the same as those in the real world. Consequently, we cannot expect temporal coincidence of any observed changes to those in the GCMs. The summer air temperature anomaly at 600 hPa may be considered a proxy for the evolution of surface melt (Fettweis et al., 2012).

3115-18 '..temperature sensitivity to GHG increase...'

3115-19 "...are amongst the GCMs with the highest climate sensitivities'

3115-16:26. As noted above, the observed acceleration of melt is not necessarily

C1311

a direct response to global warming. There are a number of 'natural' multi-decadal oscillations that are combining in addition to global warming. It is thus not unexpected that the models will have a mixed response over a 30 year period.

3117-6:11. This section should be moved to the introduction of Section 3. It is a characteristic of climate models – that they will not produce a temporally equivalent change in a natural (or even forced) oscillation. However, they will still simulate natural oscillations. Thus the conclusion that one should use the 1980-1999 section against GCMs is invalid, as the GCM is just as likely to have entered the 2000-2011 part of its equivalent oscillation earlier as later. The GCM can still be good yet disagree with 1980-1999 due to natural variability. The conclusion should be that 20 years is not a sufficient time to access the climatology of a GCM!

3118-10:15 I don't understand this paragraph – rephrase. Surely any bias in 1990 mass balance is small compared with the differences by 2100 (as per your comments on 3117-20:26)?

3118-14. Replace "most pessimistic simulations" with "high CO2 emission scenarios".

3118-17:18. The argument is messy since you are not doing attribution studies (e.g. keeping sea ice fixed). Thus it is better to state what you would expect to see then determine what the models indicate. For example, I would expect to see a shift in the zonal winds (which can be tested in the forcing fields) and consequently the pattern of precip. Try to tie the changes to the boundary conditions as you did on 3109. This will tie the elements of the paper together and make a tighter storyline.

3119-18:22. These suggestions are testable. Do the GCMs with the highest sea ice reduction result in the largest winter warming? Do the simulations with the greatest change in the ELA result in the largest August T anomaly?

3119-23:28. Rephrase. "However, in these scenarios we see little seasonal change in the components of the SMB...".

C1312

3120-1. ‘...most of the simulations...’ Why don’t ALL the simulations have snowfall converted to rainfall?

3120-4:6. Poor sentence structure – suggest “An increase in summer snowfall in the percolation zone would result in a negative albedo feedback (Box et al., 2012. ...), but no such snowfall change occurs in the MAR simulations.

3120. What about the variability in summer temperature? If in some models the temperature extremes become greater, with regional warming, then the daily melt events are stronger even if the mean temperature rise is the same. Are there non-linear characteristics such as this which could cause the models to produce different SMB by 2100?

3121-11:16. Suggest rearranging this to ‘However, rainfall does not contribute significantly to the SMB, occurring mostly over bare ice or saturated surfaces. Since the rain is not considered to transfer sensible heat, it simply runs off in both MAR and RACMO2. Consequently, we only consider meltwater (run-off minus rainfall) hereafter.’ Check that you are consistent in the use of the term ‘meltwater’

3121-17. ..warming climate...

3121-17:27. This section need to be rearranged and simplified (avoiding repetition). That on general increase in precip with temperature (sensitivity) should be at the start of 5.1. It should then lead into paragraphs on rain and snowfall changes. Note that if RACMO2 has a larger domain than MAR then the RCM physics will in part alter the pick-up of water vapour from the ocean within the domain.

3122-1:14. The rationale for doing this is poorly explained. GCM circulation changes in the Arctic and North Atlantic during global warming will alter precipitation (water vapour convergence) over Greenland. You seem to be calling this ‘inter-annual variability’. Why do you need to do this? What does it add to the paper?

3122-16:3123-7.. This is a confused section and needs rearrangement/compression.

C1313

If you have the evidence here then you do not need to refer to the work of others otherwise there is no need to mention it in this paper – it is not new. You have already discussed the bare-ice albedo effect so no need to go over it again. These 27 lines can be compressed to ... ‘The GrIS meltwater flux can be approximated by the JJA TAS (Figure 7c). However, the meltwater run-off rate increases with JJA TAS, which may be interpreted as the extension of the summer ablation zone (Franco et al., 2012b). Refreezing within the snowpack is anticipated to decrease due to the formation of ice lenses which reduce the downward percolation of meltwater’

3123-8:18. This needs simplification. Suggest rather than naming simulations, make a generic statement and refer the reader to the table.

3128-23:29. These highlights are structured poorly. E.g. #1. The key finding is that seasonality does not change, so 1. No increase in the duration of the summer melt season with warming, due to enhanced winter snowfall.

3129-9:11. This is quite surprising given the GCMs depict changes to the storm tracks, sea ice and AMOC. It is possible that the standing Rossby wave maintains a fixed local climate over Greenland. (given the small domain used this will result in little change at the boundaries.

3129-20:27. I disagree that RCMs are useful for such studies unless they feed back on the general circulation models (where elevation changes have a significant impact on storm tracks, Föhn effect, Froude number etc and meltwater affects overturning circulation). The effort should be in full ocean-ice-atmosphere coupling in high res GCMs. This is supported by your earlier statements that RCM resolution had little impact. The study here is supportive of such in that it identifies processes that the SMB of GCMs needs to include.

Interactive comment on The Cryosphere Discuss., 6, 3101, 2012.

C1314