

Interactive
Comment

Interactive comment on “Future projections of the Greenland ice sheet energy balance driving the surface melt, developed using the regional climate MAR model” by B. Franco et al.

Anonymous Referee #2

Received and published: 3 September 2012

Review of

Future projections of the Greenland ice sheet energy balance driving the surface melt, developed using the regional climate MAR model

by Franco and others

Major comments 1) This was not an easy paper to review. The English at places is rambling, the formulations at times incomplete. As a result, it is sometimes unclear what message the authors try to convey. I have highlighted the most significant ones under detailed comments, but stopped after a few pages. A thorough scientific and linguistic editing of the paper is necessary before it can be properly reviewed and finally

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



published.

2) Apart from this technical issue, my biggest concern is the model evaluation. Note that models are by definition an approximation of reality, so cannot be 'validated', rather evaluated. It is notoriously difficult for GCMs to correctly partition the surface energy balance over (seasonally) snow-covered surfaces, especially during/after melt conditions.

On page 2274, section 3, the authors state that "Given that the ERA-INTERIM-forced MAR run has already been successfully validated (see Sect. 2) . . ."

However, from section 2 it does not become clear what this successful validation entails. In the framework of this paper a successful validation would mean that the partitioning of energy balance components during melt was accurately simulated, yet I do not find a reference to such a comparison. Note that this is very different from comparing a model to observed wind speed, 2 m air temperatures or satellite melt extent, as has been done recently for a suite of regional climate models in Rae and others (TCD, 2012), including the model used in this paper.

The authors continue ". . .and given the lack of direct measurements of melt on the scale of the whole ice sheet. . ."

This motivation is not strong: it is rather evident that there are no energy balance measurements (which I assume is what the authors mean by 'melt measurements') on the scale of the whole ice sheet: if that were the case, this modeling exercise would not be necessary. Modeling by definition is intended to fill the gaps between widely spaced observations in a physically meaningful way, and model evaluation should involve comparison with and tuning to those same observations.

The authors continue ". . .the melt outputs from MAR forced by the GCMs under current climate (1980–1999) are validated by comparison with the results from MAR-ERAINT (see Fig. 1b–d)."

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



This is not sufficient: it merely tests for consistency in lateral/surface forcing fields from GCMs which does not replace an independent evaluation.

So before the scientific value of the results in the remainder of the paper can be assessed, a more in-depth model evaluation is necessary. This is especially important as the remainder of the paper assumes this partitioning to be correct! Numerous energy balance studies from Greenland have been published in literature, and those results must be used to see whether MAR-ERAINT is capable of providing the right partitioning of the energy balance during melt conditions.

3) The chosen threshold of 'melt' is 1 mm WE day⁻¹. I wonder how sensitive the ice sheet integrated results are to the choice of this threshold. If this value was chosen to be e.g. 0.1 mm WE day⁻¹, a much larger part of the higher ice sheet would be involved in the calculations, and the energy balance partitioning of that region would start to dominate the ice sheet averages. I invite the authors to comment on this and demonstrate that the results are robust with respect to the melt threshold chosen.

4) I am somewhat uncomfortable with the multitude of figures using 2 m temperature as independent or 'predicting' variable (Figs. 2, 3, 5, 6). The correlation between melt, energy balance components and 2 m temperature follows from the simple fact that all respond in first order to the surface energy balance and changes therein. This does not necessarily mean that 2 m temperature changes have good predictive skills for future melting, and that is the way in which many readers will interpret these results, e.g. you take a temperature perturbation and you get the perturbation in ice sheet mass balance; please discuss.

5) How was the model snowpack initialized? Was it in balance with climate before the melt started increasing, i.e. did temperature, liquid water content and density equilibrate before the scenario runs were started?

Subsequently, does this lead to a trend in subsurface heat flux, as the snowpack warms up in response to enhanced refreezing? I know that Polar MM5 had initialization issues

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

over Greenland, and that the model had to be restarted every now and then, a problem that was likely associated with the drifting snowpack.

Technical comments

Title: shorten considerably

p. 2266, l. 2: What are '25 km simulations'?

p. 2266, l. 6: What does 'TAS' stand for? A more commonly used abbreviation is NSAT but I prefer 2 m temperature.

p. 2266, l. 12: When does the increase in melt 'surpasses' the effect of enhanced snowfall?

p. 2266, l. 17: Opposite trends in cloudiness: do you mean over the same period? If so, does this mean that the current melt trends are part of natural variability?

p. 2266, l. 21: What do you mean by 'timing'?

p. 2267, l. 2: What do you mean by 'direct consequence'?

p. 2267, l. 5: Please be accurate when describing the state of the art. Enhanced meltwater supply sometimes leads to a decrease, not an increase of basal sliding of land-terminating glaciers; anyhow, this effect has not led to measurable mass loss from land-terminating parts of the ice sheet, and this remark therefore is out of place in this context. Please adjust formulation to reflect this.

p. 2267, l. 7: Please make clear that increased discharge only occurs for marine-terminating glaciers.

p. 2267, l. 10: What do you mean by 'concerns'? Concerns about the accuracy of the projections, or about their outcome?

p. 2267, l. 16: The transition to the discussion of surface albedo is abrupt.

p. 2267, l. 21: Albedo is not an SEB component.

C1364

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

p. 2267, l. 23-26: This sentence is unclear, remove or make more specific.

p. 2268, l. 10: Please explain K-transect or show map.

p. 2268, l. 22: infrared is inaccurate, use longwave/terrestrial.

p. 2269, l. 22: What was reduced by a factor of two, and compared to what?

p. 2270, l. 6: Does this mean that the snow model allows a layer thickness of 1 mm? How small must the model timestep be for a layer with such small heat capacity (and hence very fast temperature changes) to be numerically stable? Does this comply with the Courant Friedrichs Levy condition for numerical stability?

p. 2270, l. 9: 'posits'? Do you mean 'assumes'?

Etc. . . .

Interactive comment on The Cryosphere Discuss., 6, 2265, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)