Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-207-RC1, 2020

Geoscientific Model Development Discussions

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Interactive comment on "FAMOUS version xotzb (FAMOUS-ice): a GCM capable of energy- and water- conserving coupling to an ice sheet model" *by* Robin S. Smith et al.

Anonymous Referee #1

Received and published: 1 September 2020

1 General comments

This manuscript presents the Earth System Model (ESM) FAMOUS-ice, which is designed for coupled ice sheet-climate simulations on multi-millennial time scales. Branching off from FAMOUS-xfhcu, this model configuraion has been extended by a multi-layer representation of the snow-pack. Furthermore the model is now capable to consider glaciated surfaces on multiple subgridscale-tiles of different elevation for each model grid-box.

The manuscript consists of a description of these modifications to FAMOUS and eval-

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uates simulated components of the surface mass balance (SMB) and the resulting long-term coupled response of the Greeenland Ice Sheet (GrIS) based on a simulation forced by sea surface and sea ice conditions from a CMIP5 simulation.

The inclusion of realistic interactions between ice sheets and the climate system in ESMs is timely and an important step towards a more comprehensive view on long-term climate change. The hard-wired implementation of a snow pack model which is resolving sub-gridscale topography is a promising approach, which is capable to resolve essential small scale processes in the context of long-term simulations. FAMOUS is a paricularly fast model and is one of the few general circulation models which can cover glacial-interglacial timescales. As such the presented model is a particularly valuable contribution but the presented results also indicate considerable shortcomings in the representation of the spatial distribution of the surface mass balance, which, in my opinion, should be addressed in more depth. Seeing these problems with spatial characteristics it is even more important to analyse the model's SMB response to temporal climate variations — this is missing in the manuscript.

Furthermore the manuscript could benefit from introducing the general concept of the snow-pack scheme, which is here only described where it differs from a referenced published version.

Before publication I recommend major revisons (see comments below).

2 Major comments

The presented analysis demonstrates that FAMOUS-ice in combination with an ice sheet model is capable to simulate a reasonably realistic GrIS under present day climate conditions. The surface mass balance, however, appears to be extended to too high elevations and albedo seems to be overestimated in the ice sheet's interior. Given that other ESMs of similar design and resolution exhibit a qualitatively better skill (e.g.

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Kapsch et al., 2020; Fettweis et al., 2020), the analysis of the model should answer the question, whether these shortcomings are the result of a biased climate or of a misrepresentation of specific processes at the ice sheet's surface.

Fig. 2 indicates that the downward shortwave radiation is strongly biased towards lower values while downward longwave radiation does not exhibit any clear bias. As argued by the authors, this may indicate an overestimated cloud cover, which should be substantiated by decomposing long wave radiation into atmospheric emissivity and near surface temperature. A potentially biased cloud cover should be reflected in atmospheric emissivity. Near surface temperature influences both, downward longwave radiation and turbulent heat flux and is an important driver of the SMB, which should be analysed aswell.

Given that the spatial distribution of surface mass balance and albedo from FAMOUS-ice differ strongly from respective fields simulated by MAR, I would propose to additionally show selected surface exchange characteristics (Fig. 2) as 2-D fields in comparison to MAR.

It is important to not only analyse the spatial characteristics of the simulated SMB but to also evaluate the model's response to climate variations. The model is explicitly designed to simulate fundamental climate change and consequently this paper should allow to assess the model's skill to respond to climate change. This could be accomplished by using a transient ocean forcing from a 21st century climate projection.

Furthermore, I found the manuscript at times hard to read, as essential information with respect to the snow pack schemes are only found in referenced literature:

Page 10: It would be helpful to briefly summarize which processes influence the grain-size of snow and to generally characterize the relationship between snow albedo and climate forcing, and to give typical grain sizes of new snow and old/wet snow. (Alternatively, a figure illustrating albedo as a function of influencing factors could be beneficial.)

This is particularly desirable as Marshall (1989) is not easily available and as Gardner and Sharp (2010) indicates a sensitivity to grain size ≈ 0.05 / mm towards greater grain sizes, which seems to be considerably smaller than the values used here.

I also missed a separate short paragraph on how the snow pack evolvels over time: How does density change if snow accumulates on top, what happens if the first layer melts, how and when are densities of the snow column prognostically calculated, what is the typical depth of compacted snow?

I also recommend to improve the structure of the paper by a further break down of subsections. Separate subsections might cover initial conditions, upper and lower boundary conditions of the snow pack as seen by the land surface model, and boundary conditions at the ice surface as seen from the atmosphere. Finally the analyzed experiments, together with the MIROC forcing and the MAR and RACMO simulation should be introduced in a separate section, possibly before section 4.

3 Some specific comments

p. 5, l. 8: Is it possible to quantify the computational cost of the new model version in comparison to the old version?
p. 5, l. 17: "We will describe later..." Please refer to the respective section.
p. 7, l. 9: high-latitude -> high-altitude?
p. 7, l. 11: Is the sub-surface lapse rate in line with the coarse resolution subsurface temperature distribution?
p. 11, l. 12: (fig:4) -> (Fig. 2) ?
Fig. 3, 5,6, : As the colorbar does not caver the full range of values, maybe inlude the range of values in the figure caption.
p. 12-14: does the spatial representation of SMB, albedo improve qualitatively if the number of elevation classes is increased?

p. 12, l. 9: It should be stressed that FAMOUS and MAR are consistently forced with the climate model output while RACMO is forced by reanalysis data. Also please specify the ERA forcing used and the period which was analysed.
p. 14, Tab. 1: Please specify which ERA forcing is used here, Fettweis 2013 only has RACMO(ECMWF), and the RACMO(ERA) experiment that was used in this paper should be added to this table.

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