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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 #2**

Received and published: 8 September 2020

### General comments

The earth system model FAMOUS-ice, which now includes an updated multi-layer snow scheme and coupled downscaling of the surface mass balance using elevation tiles, is presented in the manuscript. Synchronous coupled ice sheet-climate simulations is an active area of research, and this manuscript is a timely contribution. The computational efficiency makes FAMOUS-ice well-suited and capable for long climate simulations, where the evolution of the ice sheets play a crucial role.

FAMOUS-ice is not capable of accurately resolving orographic precipitation over the ice sheet, and is biased warm at high altitudes. The resulting SMB has obvious spatial



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differences with the regional climate model MAR simulated SMB. In particular, surface melt occurs at too high altitudes due to a low albedo. Due to this, the manuscript could be improved by adding a more detailed analysis of the surface energy balance.

Before publication I recommend minor revisions.

**Scientific comments**

You argue that Helsen et al., 2012 developed an empirical parameterization to translate global climate fields to usable boundary conditions. However, they state that their method is not usable for models where ablation areas are not resolved.

How does FAMOUS-ice prevent snow from growing infinitely thick? Does it include a firn-to-ice densification scheme, or do you cap the snow above a certain thickness?

For downscaling of the temperature and (incoming?) longwave radiation from the atmosphere to the elevation classes you use an empirically tuned method, where you assume that the near-surface climate gradients are similar to the climate gradients in the lower atmosphere. Is this a valid assumption? For example, the temperature decreases with elevation along the GrIS near-surface, while the lower atmosphere has a temperature increase with height due to inversion.

I am not convinced you improve on the elevation class implementation analysed by Sellevold et al., 2019, as you are not comparing the same metric - that is, you do not subtract the grid-cell average from the same grid-box elevation class simulated value.

Below 1000 m, the latent heat flux is positive in FAMOUS (Fig. 2), while negative in MAR and RACMO. First, it should be indicated whether the positive values means energy loss or gain at the surface. I am assuming the latter, as MAR and RACMO simulate climatological mean sublimation at lower elevations. Can you explain why FAMOUS simulates climatological mean deposition at these elevations?

You argue that the shortwave down is too low because of a high biased cloud cover. I am surprised to see that this is not compensated for by an overestimation of longwave down. Is this due to the atmosphere being warmer in FAMOUS?

The simulation of albedo shows clear differences, particularly at higher elevations, when compared to MAR. You argue that the trigger for the lowered albedo at the higher elevations is a warm temperature bias at higher altitudes. Including a (supplementary) figure of April/May near-surface temperature or snow grain size compared to MAR/observations would likely strengthen this argument. Further, is it possible to assess whether the albedo is triggered too much and/or too early in the melt season?

Also, if the atmospheric temperatures are too high at high altitudes, the melt energy contribution from the sensible heat flux should also be higher. As you don't show the sensible heat flux, I am left to wonder how the simulation of the sensible heat flux is.

How well is rain simulated over the ice sheet? As the ice sheet is warm at higher altitudes, is there an overestimation of rainfall at higher altitudes? Could this also contribute to a lower albedo?

The presented surface mass balance shows a realistic distribution with ablation at lower elevations and accumulation at higher elevations. However, the ablation areas are biased large and the high accumulation areas in the Southeast and Northwest show too little accumulation. As you don't present a transient climate simulation, I think the possible effect of these biases in transient simulations deserves a paragraph in the discussion section.

It would be easier to understand some of the thickness anomalies in Fig. 8 if you showed the ice velocities. Consider adding it to supplementary materials.

**Technical comments**

I found many occurrences of double parentheses with citations. If latex is used, use (text before citation ) or  to avoid.

P. 11, l. 12: replace fig:4 with figure 4.

P. 19, l. 2: “a model”

P. 19, l 2: “allow for coupled”

Fig. 7, caption: replace x in: 10x km3

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