Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-97-RC1, 2019

Geoscientific Model Development Discussions

Interactive comment on "Version 1 of a sea ice module for the physics based, detailed, multi-layer SNOWPACK model" *by* Nander Wever et al.

Anonymous Referee #1

Received and published: 11 July 2019

In the manuscript: "Version 1 of a sea ice module for the physics based, detailed, multi-layer SNOWPACK model", an extension of the physical based SNOWPACK model to sea ice is presented. Parts of the model equations are adapted to account for salinity. In addition, other relevant processes like sea ice growth and melt, flooding and brine dynamics are included. The authors discuss the performance of the model comparing it to snow buoy and IMB measurements over Antarctic sea ice. Overall, this manuscript is well written and has a clear structure. Modeling the evolution of snow on sea ice is a challenging task and the here presented model extensions provide important progress for the snow on sea ice modeling community. I believe that this model will be widely used in future research, especially if it will be coupled to SMRT like the authors state in their conclusions. However, the presentation and the discussion of the results are partly

too short and important details are missing. Therefore I recommend the manuscript to be published after major revisions. I have three general comments that I would like the authors to address.

General comments (GC):

GC1: In the description of the experiments, important details about the model initialization are missing. In SNOWPACK, the snow is described by grain parameters like dendricity, sphericity and bond radius. These parameters are hard to obtain from snow pit or snow buoy measurements and I wonder what the authors assumed in the model initialization (so far, it is not mentioned in the manuscript). A discussion about these parameters and their influence on the results would be very helpful for further SNOWPACK users. In addition, the authors should provide the full initialization profiles and SNOWPACK settings (I guess this can be provided in the appendix).

GC2: In general, the motivation and discussion of the different experiments (section 4.1 to 4.5) are too short and need to be extended. Some features, which are visible in the figures are not or only poorly discussed. I encourage the authors to carefully read my specific comments for the single chapters.

GC3: It seems like the authors used the same color-scale range for all figures. In some figures, it is impossible to distinguish between different values since the color-scale goes far beyond the actual values in the figure. I recommend to use individual, representative color-scales for each figure.

Specific comments:

Section 3.1: Please improve figure 1.

Section 4.1: From figure 2, it looks like it takes less than two hours until the 1.58 m thick sea ice is saturated. Is this realistic? It seems very fast to me. The caption for figure 2 (a) is confusing. I guess you mean "dry sea ice" instead of "dry snow"?. In figure 2 (a) to 2 (d) the color-scale range need to be improved.

Section 4.2: Please increase the y-axis in figure 3 (b). Please mention what the blue line in figure 3 (c) is (I guess the ice/ocean interface?). In figure 3 (d), a difference of 6-10°C is found close to the snow/ice interface. Do you expect the differences to be even higher at the top of the snow? As I understood, you use ERA5 incoming longwave radiation as a forcing data. So in principle, the differences shown in figure 3 (d) could also be caused by errors in the forcing data. It might be helpful to add a timeseries of the ERA forcing data to figure 3. I'm surprised that even at the ice/ocean interface, the difference can be up to 2°C. To my understanding, the temperature should always be around the freezing temperature, or not? The caption for figure 4 is wrong (there is no temperature plot). Please adapt the color-scale for figure 4 (b).

Section 4.3: It is not clear to me, what the authors want to demonstrate in this section. I therefore encourage the authors to write a short motivation at the start of the experiments (This also applies for the subsequent sections). In Figure 5 (a) to 5 (c), vertical stripes are visible at the end of the simulation, which look like numerical instabilities. Are these related to the bucked scheme or to a too big timestep?

Section 4.4 In figure 6 (b) and (d), horizontal stripes are visible in the ice in the middle and at the end of the simulation, which look like numerical instabilities. To my understanding, these patterns should not appear since you are using the Crank-Nicolson scheme and the CFL criteria. Do you have an explanation for this phenomena? Please also discuss if these instabilities could have influenced the results of the simulations. In figure 6 (d), high brine salinity is simulated at the top of the snowpack. This must be wrong as I can't find any explanation how this could have happened? (especially since the snow was dry according to figure 6 (b)). Figure 7 (a) , (c) and (d) are not discussed in the text.

Section 4.5. Please extend the discussion of the experiments. The color-scale for figure 8 (a) needs to be adjusted.

Section 4.6. Please extend the discussion and describe in more detail what is seen in

figure 11.

Minor comments and typos: (P refers to the page and L to the line)

P1L3: cut "strongly"

P1L18: "high salinity water" → "saline water"

P2L4: Snow limits also the summer sea ice melt

P2L8-9: Please consider to rewrite this abstract since it is hard to understand

P2L10: "snow melt" → "melted snow"

P2L12: "Uncertainty in knowledge" reads strange, please rephrase

P2L13: "Assessing snow amounts on sea ice is not straight-forward from atmospheric forcing alone" → "Assessing snow amounts on sea ice from atmospheric forcing alone is not straight-forward"

P2L22-23: I think all of these effects can also be observed during winter season.

P2L23-25: Please specify "shallow snow". According to figure 1 the snow depth can be up to 1 m which I would not consider as shallow.

P2L26: "...impact on snow microstructure..." → "impact on the snow microstructure"

P3L2: You could cite a paper dealing with this problem. E.g. Markus et al (2006)

P3L17: Please explain the grain parameters.

P4L15-20: This paragraph is difficult to read. I recommend to rewrite it

P4L26: "in the ocean from below" → "in the sea ice from the ocean below"

P6L13-14: "A critical assumption is. . .. That it assumes..." please rewrite.

P7L13: What does MPFD mean in equation 14?

P7L22: please explain all coefficients in equation 15.

P8L18: "w“ → "we“

P8L18: please mention what CFL stands for

P8L25: "and a Dirichlet boundary condition by...“ → "and a Dirichlet boundary condition at the bottom by...“

P9L3: Equation 18 needs to be explained in more detailed

P9L20: cut out "for snow“

P10F1: "two sea ice buoys“ → "two snow buoys“. Please improve the notation of the dates in this figure.

P10L1: In general, the buoys to not measure the snow depth but the hight of the snow above the initial snow/ice interface. In case of the formation of superimposed, this can make a significant difference

P11L17: "after which“ → "and“

P11L18: "up to“ → "until“

P11L25: to me, a volumetric ice content of 0.9 seems rather low.

P12L7-8: "ERA5. . . to provide“ → ERA5. . . data to provide“

P14L6-8: This reads strange, please rewrite

P14L7: Please specify "spring“ (which months?)

P15L11: Please specify "slightly“

P16L6: "depending“ → "depending on“

P17F5: I don‘t see grey colored snow in figure 5 (b)

P17L9: "For warming“ → "For the warming experiment“

P20: Figure 8 is shown before mentioning it in the text

P22L1: Please rewrite the first sentence

P22L25: "simulate sea ice“ → "simulated snow on sea ice“

P24: I don’t see grey snow in figure 11 (b).

P24F11: "melting conditions is enforced“ → "melting conditions are enforced“

P25L3-4: please rewrite

P25L7: "limited forcing data“ → "limited measurement data“

P25L7-9: please mention the other limited measurement data

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