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The Cryosphere Discussions

Interactive comment on "Snow on Arctic sea ice: model representation and last decade changes" *by* K. Castro-Morales et al.

Anonymous Referee #3

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The manuscript proposed by Castro-Morales et al. discusses simulations of large-scale snow characteristics in a sea ice & snow modelling system.

— General statement —

My perception of the manuscript is that it lacks fundamental rigor to be published.

I'm not going to detail all issues but concentrate on the main ones.

1) Most conclusions are not supported by the results

2) The focus of the paper is not well-defined

3) The methods are not well explained / wrong to be confident in the conclusions.

4) The snow budget analysis lets the reader with poor confidence. It is far from being closed and the given explanations of why it is the case cannot be true.

— More details on evaluation

Let's start with some physics. At first-order, the snow mass budget in the Arctic can be written as: dms / dt = Precipitation - Melting.

Other terms are of small importance (let us say <20%)

Intense snow fall occurs in fall (Sep-Nov), then nothing in Dec-Jan, then steady accumulation until May (as reviewed by Warren et al.). Snow is almost completely melting in summer. Precisely measuring / reanalyzing snowfall is complicated. Gauges miss a significant part of falling snow. Reanalysis systems have a hard time to simulating water vapour and hence precipitation.

Based on these simple ascertainments, (i) a forced model will have winter snow depth mostly driven by the quality of precipitation fields; (ii) trends in snow depth could arise from either decreasing snow fall or decreasing sea ice when snow fall occurs.

These simple physical lines of thoughts should have guided the writing of the paper: (i) how are Era-interim precipitation fields consistent with icebridge snow depths; do we need a complex snow model ?; (ii) are there possible trends in snow depth and what would drive them (snow fall or ice retreat) ?

By contrast, the proposed paper vaguely turns around these issues without pointing the key aspects that should have been covered.

To further circumstanciate my general statement, here are now more elements:

1) The conclusions are mainly unrelated / unsupported to the contents of the paper.

"The model snow depth is comparable to radar measurements at regional scale." -> Yes, but that must mostly be due to the quality of precipitation fields

"Parameterizing snow with multiple categories ... leads to a realistic representation of Arctic snow."

-> There is no trace in the paper why the multiple snow categories contribute to a realistic representation of snowfall. -> Why the snow simulation is realistic is mostly likely because of precipitation forcing, which is missing from the paper.

"We encourage to improve snow physics" -> Why would one do so ? You do not give any argument.

"We should use data assimilation, as they would improve model snow distribution" -> In which context ? And how is that related to the contents of the paper ?

"We should measure snow better" -> Is not that obvious ? How is that related to the contents of the paper ?

2) The stated "motivation" of the work is to (i) evaluate snow depth, (ii) trends, and (iii) analyze the contributions to the snow mass balance.

I think just (i) is correctly done. (ii) Regarding trends, the authors seem to mean that there is less snow in 2013 than in 2000. Even if they acknowledge large variability, there is no statistical quantification of the trends and of their signficiant. Besides, there is no attribution of trends in wrt to changes in precipitation in Era-Interim or sea ice retreat. This could have easily been done. (iii) I have no reason to believe in the snow budget as presented in the paper (see below).

3) Methods and physical understanding are problematic throughout the paper. I'm not saying the model is wrong. But the analysis lacks fundamental rigor.

- There are two simulations ran, but the second one is barely used.

- The snow model has prescribed ITD (have no idea how that works, the paper Castro-Morales et al 2014 is missing from the reference list). There is snowfall, melting and flooding. Fair enough, but then your snow budget should consistently reflect these

processes.

- Section 3 - results. How can you expect that snow conductivity would affect snow depth ?

- Why did you interpolate model on data grid and not the contrary (Section 3.1).

- Why would you show the dependence of model wrt latitude if this proves meaningless. I would have looked at the PDF of error for different snow depths. This would have brought much more understanding

- Snow accumulation looks like snow depth in your figure 4. Your definition in the text seems like a time derivative of snow depth. What is "snow accumulation" in the end ?

4) The snow budget presented is problematic. (i) Snow depth is not precisely defined. As such, snow depth is not an extensive variable, hence it cannot be conserved, because ice concentration is changing. If snow depth refers to an equivalent to snow mass (snow depth times concentration), this is fine.

(ii) The snow budget is not closed, and the closing ÂńÂăresidualÂăÂż term makes 78% of the budget. This is not acceptable. The authors should have diagnosed the snow budget from their model much more precisely. What is more, the residual term is attributed to the missing processes in the model (snow formed by rainfall, wind redistribution, blowing snow sublimation). How can that be if these processes are missing from the model ? My belief is that there is a leak either in your budget or in your model and this is what this residual term represents.

(iii) Why is snowfall evaluation based on climatology ?

(iv) How can the ocean heat affect the snow ?

(v) How can snow depth increase due to rain ?

Interactive comment on The Cryosphere Discuss., 9, 5681, 2015.