

Review of the manuscript: « ***Comparison of a coupled snow thermodynamic and radiative transfer model with in-situ active microwave signatures of snow-covered smooth first-year sea ice*** », submitted for publication to The Cryosphere (TC).

July 29, 2015.

Comments for the authors: **minor revisions**

***General comments:***

The present paper provides the evaluation of a modelling suite, including a comprehensive 1D snow model forced by atmospheric reanalyses and a microwave backscatter model. Every component of this suite is evaluated by comparison of several simulations with in-situ observations. In particular, the study shows that the simulated surface scattering is significantly improved by applying an in-situ salinity profile to the snow profile in the model.

The paper is well written, and I believe free from major flaws (except maybe one consideration about longwave radiation – see comments below). It is interesting and very relevant to the topics of The Cryosphere. However, in its current state, I expect it to have a rather minor impact on the state of the research, because the important conclusions are not highlighted as best as possible, mainly due to problems in the paper structure. This study deserves to be published after some reorganization. My comments below, rather than criticism, involve suggestions for enhancing the message of the paper.

***Specific comments:***

Abstract

*Issue:* The main message and the novelty brought by this study get lost in the long summary of the results. The authors kept consistency with their introduction and conclusions, they address the 4 points/questions raised in the introduction. But my feeling is that there is a hierarchy in terms of the importance of the results. Among those 4 questions, 1. and 2. are mainly quality checks on the forcing data and model skills with respect to observations. This is useful and appreciated, but it is not what brings originality to the work. SNTHERM is I believe a well-established snow model that has been validated/evaluated against observations several times already in other studies. Besides, presently, this validation aspect in the abstract is addressed rather weakly, using terms as “reasonably represented” without stating any quantitative error. As for the reanalyses, checking they are consistent with observations is more a method or quality control aspect that does not require to be in the abstract where the most important must be kept.

*Suggestions for enhancement:*

- I would reduce/remove the evaluation statements on the snow model and forcing data, and emphasize instead on the results regarding radiation.
- Highlight the novelty of this work. It is said in the introduction that it is the first time such model suite evaluation is performed, say it again in the abstract.
- Rewrite last sentence and, in general, avoid such long sentences with several “and”. As such, it seems like a long list of processes thrown into the same bag without specifying which of them impacts on what. This last sentence, that conclude the abstract, must be strong and has to give the reader envy to read further.

## Introduction

- P 3295 L3-5: Instead of “governs” and “controls” I would use something like “curtails” and “exerts control”, for instance, it would be more accurate. Besides, if snow plays a very important role in the thermodynamic ice growth rate, it is not what controls everything in terms of extent and thickness, especially regarding dynamical/deformation processes (especially true for Antarctic sea ice).
- P 3295 L6-8: Statement a little vague and unclear. Maybe speak of “Turbulent sensible and latent heat fluxes”, and in terms of the importance of the snow cover for the climate system the radiative fluxes and albedo effects are just as important.
- P 3295 L9: Same, “energy exchange”, a little too vague + use plural
- P 3295 L10 : “distinctly different”, maybe just “distinct” or “different”
- P 3295 L11: “arrangement of snow mass” What do you mean by this? The fractional distribution of water phases constituting the snow?
- Note about the references: I am surprise not to find any Sturm, Massom or Perovich references when describing the importance of snow on sea ice in general. The chosen references seem appropriate, but those guys in particular (among others of course) did publish a huge amount of literature about snow on sea ice and are even the authors of related review chapter: *Sturm, M., Massom, R., 2009. Snow and sea ice. In: Thomas, D.N., Dieckmann, G. (Eds.), Sea Ice, second ed. Wiley-Blackwell, pp. 153–204 (Chapter 5).*
- P 3296 L29 - P 3297 L3: This statement is very important but the sentence is very long. It seems that it is repeated later and better formulated at L17-21. So maybe keep the latter statement only.
- P 3297 L22 - P 3298 L24: description of SNTHERM – forcing data – MSIB. In my opinion, this is a wrong place to do such a detailed description. It makes the introduction very long to read. Simply move this in the appropriate paragraphs of section 2.

## Sections 2.3 and 2.4 – description of the NARR – SNTHERM – MSIB suite

- Structure: 1. Separate those three components description in three distinct sections and, as mentioned above, move the related information from the introduction to here. 2. Split each section (except the NARR one) in two paragraphs (just paragraphs, not subsections) dedicated to the model description itself and configuration matters (setup, experiments, maybe give a bit more information about time stepping, resolution of the snow model...). Avoid mixing statements of a different nature.
- P 3302 L12: the Schwerdtfeger looks a bit dated to me, there as has been many formulation for sea ice thermal conductivity since then.

## Results and discussion

- Again, results and discussions should have their own specific section. Results should include only factual results, and discussions reasons for observed biases, inter-comparison and interpretation of those biases... As it is, everything is mixed and the message gets blurred. An example of this is the discussion on the errors in temperature and RH in the NARR section, explaining how these errors impact on the snow grain growth rate in the model. At this stage, the reader learns how it impacts on the grain growth rate but does not know how it relates to the observed biases in the snow model or the backscatter model. When those issues are tackled later, then the message from the forcing section has been forgotten.

So, considering this and my previous comment in the abstract about the hierarchy in the conclusions, I would suggest the following structure:

3. NARR forcing and SNTHERM versus in-situ observations

3.1 Results

### 3.1.1 NARR

### 3.1.2 SNTHERM

## 3.2 Discussion

(Mixed, to explain the reasons for NARR and SNTHERM errors and how they relate to one another)

## 4. MSIB backscatter signature comparison

### 4.1 Results

### 4.2 Discussions

## 5. Conclusions

- About the radiation forcing errors and their impact on snow temperatures. This is my sole concern about the content of the paper. The biases are very large and weaken the conclusion of the paper. Have you explored solutions to try to reduce the errors in longwave radiation time series to ultimately reduce the errors in the snow temperature profiles? Vancoppenolle et al. (DSR-II, 2011) in particular discusses optimal formulas to reconstruct shortwave and longwave fluxes. This would imply rerunning the model using other time series for longwave radiation instead of the NARR forcing, but it may be worth a try.

An other thing that could be done would be a sensitivity experiment introducing a bias correction in the longwave forcing, to see if it actually decreases the errors in temperatures. That would strengthen the associated discussion and this aspect of the conclusions.

## Conclusions

- Try to avoid weak and general statements such as “reasonable agreement” (P3309 L9), “reasonably captured” (L21, same page) or “slightly underestimated” (L22).
- Again, organize the conclusions into a hierarchy of their importance, based on what really brings new knowledge, so as to get a clear message.
- Avoid ending your paper on such a long and tortuous sentence.

## ***Technical comments:***

Those comments include suggestions about the phrasing / choice of words in the text. English is not my mother tongue and I do not pretend to be right on everything that follows. Still, I believe that there are a few things that could be improved, here are my suggestions:

- I insist a bit on this, but the manuscript contains a good number of long and thus unclear sentences, with many “and” that are hard to read... Please reword them and/or split them into simpler sentence.
- “Snowcover”. After quickly looking in a few dictionaries and on the web, I can find it only in two words “Snow cover”. Besides I would add an article “the” before it, at several places in the introduction, in particular.
- P3297 L14: fix “downwelling”, or maybe use “downward”?
- To avoid the overuse of “pertinent”, e.g., “relevant”, “of importance”...
- When you speak of the “character” of the snow cover, is that really an appropriate term?
- “first-year” vs. “first year”. I believe this is a question of American English or British English. Anyway, choose a standard (it seems that “first-year” is used more often here) and adopt it everywhere.
- Just a detail: at two places in the manuscript (title and methods), the use of “smooth” ice is used. If it refers to the fact that it is undeformed, I would use “undeformed” or “level”.

- The first sentence of the “Meteorological data” section is weird, especially in the way information within brackets is given. Simplify, for instance saying something like “Relative humidity (RH) was acquired by...”. Same for other variables.
- Section 2.3, L6, change “the thermal capacity” by “its thermal capacity”. L7 and 18, specify “air temperature” and “snow temperature”, respectively.

### Figures

- They are generally well presented, but sometimes difficult to read. I suggest enhancing all Line widths/styles (for time series, not the scatter plots).
- In the same line of idea, Figure 6, left panel, would not suffer from being enlarged.
- Figure 5, the meaning of the asterisks should be included in the caption (even if it is already mentioned in the text).
- Figure 9 and 10. I understand what “SNTHERM 1” and “SNTHERM 2” mean from the text, but they were never referred to as such elsewhere in the manuscript. This could be a little confusing.
- Figure 11: Maybe enhance/highlight some specific curves depending on which of them illustrate the important conclusions of the paper. Also, define “VV” and “HH” backscatter.

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