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Interactive comment on "Comparison of a coupled snow thermodynamic and radiative transfer model with in-situ active microwave signatures of snow-covered smooth first-year sea ice" *by* M. C. Fuller et al.

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Thank you for your revision of our work.

It has improved the content and clarity. We have also attached a zipped file which includes a word.docx copy of your original review and our response, and JPEG and PNG versions of revisions to improve the clarity and lines of the figures.

REVIEWER 1:

Review of the manuscript: Âń Comparison of a coupled snow thermodynamic and ra-

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diative transfer model with in-situ active microwave signatures of snow-covered smooth first-year sea ice Âż, 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.

1. 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

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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.

1. AUTHORS: Thank you for your suggestions. We have removed some of the detail regarding the NARR and SNTHERM from the Abstract. We have added information indicating that the novelty and importance of this work lies in the later results, while still maintaining the necessary word limitations. The Abstract now reads:

“Within the context of developing data inversion and assimilation techniques for C-band backscatter over sea ice, snow physical models may be used to drive backscatter models for comparison and optimization with satellite observations. Such modeling has potential to enhance understanding of snow on sea ice properties required for unambiguous interpretation of active microwave imagery. An end-to-end modeling suite is introduced, incorporating regional reanalysis data (NARR), a snow model (SNTHERM89.rev4), and a multi-layer snow and ice active microwave backscatter model (MSIB). This modeling suite is assessed against measured snow on sea ice geophysical properties, and against measured active microwave backscatter. NARR data was input to the SNTHERM snow thermodynamic model, in order to drive the MISB model for comparison to detailed geophysical measurements and surface-based observations of C-band backscatter of snow on first-year sea ice. The NARR variables were correlated to available in-situ measurements, with the exception of long wave incoming radiation and relative humidity, which impacted SNTHERM simulations of snow temperature. SNTHERM snow grain size and density were comparable to observations. The first-assessment of the forward assimilation technique developed in this work required the application of in-situ salinity profiles to one SNTHERM snow

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profile, which resulted in simulated backscatter close to that driven by in-situ snow properties. In other test cases, the simulated backscatter remained 4 to 6 dB below observed for higher incidence angles, and when compared to an average simulated backscatter of in-situ end-member snow covers. Development of C-band inversion and assimilation schemes employing SNTHERM89.rev4 should consider sensitivity of the model to bias in incoming longwave radiation, the effects of brine, and the inability of SNTHERM89.Rev4 to simulate water accumulation and refreezing at the bottom and mid-layers of the snowpack. These impact thermodynamic response, brine wicking and volume processes, snow dielectrics, and thus microwave backscatter from snow on first-year sea-ice."

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2. REVIEWER 1: 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?

2. AUTHORS: Each of these lines has been modified to address the Reviewer's concerns regarding word choice and clarity of meaning. It now reads

"Snow cover curtails the heat and energy exchange across the ocean-sea ice-atmosphere interface, and therefore, exerts control over sea ice formation, ablation, extent and thickness processes (Maykut, 1982; Curry et al, 1995). This is important

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to the global climate system due to the significant amount of energy involved in sensible and latent heat fluxes (Serreze and Barry, 2005) and the influence of snow due to its relatively high albedo. Snow albedo is controlled by grain size, which is both affected by, and effects, radiant exchanges. The distribution and character of snow cover is highly variable both spatially and temporally, and will undergo distinct melt and freeze cycles when forced by the same atmospheric event, based on the character and layered-arrangement of snow mass (snow water equivalent, SWE)."

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3. Reviewer 1: - 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).

AUTHORS: We agree and have cited and referenced the Sturm 2009 chapter suggested.

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4. REVIEWER 1:

- 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.

4. AUTHORS: Both instances of this statement were kept; however, the first was made into two sentences and was reworded in order to make it clearer. It now initiates the beginning of a paragraph in order to lend importance and clarity. It now reads:

"This work represents the first assessment of the suitability of an operational end-to-

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end weather-snow-backscatter estimation technique over first-year sea ice. It employs reanalysis data, a one-dimensional snow evolution model, and an active microwave backscatter model."

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5. REVIEWER 1: - 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.

5. AUTHORS: The suggestion to move the SNTHERM and MSIB information to the methods section was also noted by REVIEWER 2. We have done this in accordance with your suggested format. We have also added a more recent citation for sea ice thermal conductivity: Trodahl, H. J., Wilkinson, S. O. F., McGuinness, M. J., Haskell, T. G.: Thermal conductivity of sea ice; dependence on temperature and depth. Geophysical Research Letters, 28(7), 1279-1282, 2001.

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6. REVIEWER 1: Results and discussion - Again, results and discussions should have their own specific section. Results should include only factual results, and discussions

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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

6. AUTHORS: We acknowledge your suggested format; however, we prefer our original structure, as it preserves and highlights the strengths and weaknesses of each step in the stepwise technique. We did attempt restructuring, but found it lacked clarity.

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7. REVEIWER 1:

- 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.

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Another 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.

7. AUTHORS: Thank you for your suggestions. In this paper our intent was to focus on an assessment of the errors in the system as they exist in the operational NARR data set. However, we acknowledge these current limitation, and in future work intend to consider higher resolution Global Environmental Multiscale Model (GEM) data, developed by the Canadian Meteorological Centre (CMC), as well as other methods, including sensitivity analysis, in order to reduce the error.

As suggested, we have also revised and removed the weak language and have added sentences to highlight the importance and novelty of the 3rd and 4th objectives. The Conclusion now reads:

“3) How do simulated backscatter signatures based on SNTHERM89.rev4 output compare to simulations from observed snow structure and properties, and observed backscatter for complexly-layered snow over first-year sea ice?

As previously noted, to the authors’ knowledge this study represents the first assessment of an end-to-end modeling suite to estimate active microwave backscatter over sea ice. The use of NARR data to drive a snow thermodynamic model, which in turn drives an active microwave backscatter model at C-band provides a novel methodology to resolve snow and ice properties that produce ambiguity due to the one-to-many issue (Durand, 2007) in active microwave image interpretation.” . . .

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“4) What are the implications of the use of the SNTHERM89.rev4 thermodynamic model in an operational approach for a radiative transfer simulation of C-band backscatter over first-year sea ice?

This first assessment shows that although, there is the possibility of achieving comparable MSIB simulated backscatter from both SNTHERM derived and in-situ snow geophysical samples for complexly-layered snow on first-year sea ice, there are several constraints and considerations for improvement.” . . .

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8. REVIEWER 1:

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.

8. AUTHORS: We agree, and have changed several long sentences in to shorter, and clearer sentences. These were also noted by Reviewer 2.

- “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. AUTHORS: We have changed all instances in our text to “snow cover”.

- P3297 L14: fix “downwelling”, or maybe use “downward”? AUTHORS: Downwelling is a standard term for incoming radiation.

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- To avoid the overuse of “pertinent”, e.g., “relevant”, “of importance”. . . AUTHORS: We have replaced several incidences of ‘pertinence” and replaced with alternatives.

- When you speak of the “character” of the snow cover, is that really an appropriate term? AUTHORS: We have added the changed the term to “geophysical character” for clarification. The physical character of snow in this context refers to the primary variables important to SWE and backscatter. This includes information regarding grain size, density, SWE, stratigraphy, and dielectrics.

- “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.

8. AUTHORS: We have adopted “first-year” throughout the paper.

- 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”.

8. AUTHORS: The term “smooth” is commonly used as well. We prefer to use “smooth instead of “level”, as it better describes the characteristics of the ice surface.

- 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.

8. AUTHORS: Thanks you for your suggestion. We have changed the order of this first paragraph to for clarity. It now reads: “The in-situ meteorological instruments were located on sea ice 500 m adjacent to the snow sample sites and measured relative humidity (RH), sampled every 10 minutes and averaged to hourly data. Environment Canada’s ‘Churchill A’ station (N58.733, W 094.050) is on land approximately 20 km from the study site and measured air temperature. The NOAA NCEP NARR data was downloaded for the 32 km grid containing the sample site. This data included reanalysis

of air temperature, RH, wind speed, longwave and shortwave incoming and outgoing radiation, and precipitation amount. The NARR grid data were resampled from 3 hour to hourly data using a linear interpolation and contains a roughly even split of land and bay."

- Section 2.3, L6, change "the thermal capacity" by "its thermal capacity".

8. AUTHORS: This has been done.

-L7 and 18, specify "air temperature" and "snow temperature", respectively.

8. AUTHORS: This has been done.

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9. REVIEWER 1:

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 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.

9. AUTHORS: We have enhance line widths figures and styles for all figures, and clarified Figure 6 by changing line thickness, and bringing forward certain lines, in order to provide better clarity. We have defined VV and HH backscatter in the text as the respective send and receive microwave polarisations. The text now reads: "The surface-based C-band backscatter measurements (σ0VV, σ0HH) were acquired continuously throughout the day (May 15th, 2009) for a 20° to 70° elevation range (in 2° increments) and an 80° azimuthal range (where the first and second letters indicate the emitted and received polarizations, respectively)."

- Figure 5, the meaning of the asterisks should be included in the caption (even if it is

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already mentioned in the text).

9. AUTHORS: We have noted the meaning of the asterisks in the appropriate figure captions.

- 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.

9. AUTHORS: The SNTHERM 1 and SNTHERM 2 cases are now described specifically in the methods section of the text and can be referred to there for clarity. The text now reads: “SNTHERM 1) Cases A1 and B1 were assigned typical salinity values for first-year sea ice and overlying snow (Barber et al, 1995). SNTHERM 2) Cases A2 and B2 and were assigned average salinity values observed in-situ (Fuller et al, 2014).”

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C1724/2015/tcd-9-C1724-2015-supplement.zip

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Interactive comment on The Cryosphere Discuss., 9, 3293, 2015.