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Interactive comment on "Microstructure representation of snow in coupled snowpack and microwave emission models" *by* Melody Sandells et al.

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It is more and more evident that the exploitation of passive microwave data to infer snow mass and other properties requires auxiliary data to overcome the general underestimation of the retrieval problem. Using microwave emission models is one promising way to provide such information. However these models need as input detailed information on the snowpack including the snow microstructure (∼grain size in this paper) that are unavailable from observations. Snow evolution models suing meteorological forcing to predict time evolution of the snow physical properties are able to provide this. Because of the variety of parameterizations, modeling approach of the microstructure and existing implementation of models, an important question is which combination(s)

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of models and parameterizations to choose in retrieval algorithms or data assimilation schemes. This paper uses an elegant method to explore the behavior and performance of coupled, snow microstructure evolution / snow microwave emission models. The originality of this paper lies in the use of a large ensemble of model-physics, following pioneer work on snow evolution only models by Essery et al. (2013), which contrast to numerous other studies that are usually limited to a few model combinations and parameterizations. This study uses data from the Arctic Sodankyla site in Finland only which is limited with respect to effort put on the size of the ensemble on the modeling side. An important conclusion of this study – valid at least for this test-site – is that improvements should be focused on snow evolution models (rather than the microwave models). Overall, the paper is well written, pleasant to read and reaches its target.

The methodology is original and certainly promising to learn more about the models and their coupling because it provides complementary information compared to the widely-used traditional "calibration/validation" approach. This is attractive, but it seems also to be limited in two ways. First the generation of the ensemble is based on subjective choices for the parameterizations, models, etc, and in some case impact the results. Some conclusions of the paper are therefore specific to these choices and may be a little bit misleading when the different models are not treated the same way (they cannot be treated equally because of their intrinsic differences for the microstructure). The difficulty for the reader is to detect the dependency to the choices because it is often hidden by the complexity of the models (and their coupling). In addition, by treating on the same level the empirical parameterizations deduced from specific sites (some of which being in Finland as the study) and the physically-based models, the methodology deprives itself of the accumulated expert knowledge on the complex physics of the snow evolution and microwave emission. Only the numerical performance on the study site is the criteria used by this methodology. For the development of a particular algorithm for operational applications, this is certainly an excellent pragmatic strategy, but the paper aims at providing general recommendations on priorities of model development. The second limitation is that although the method is very powerful to inform on

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the performances of the models, it is unable (or not used here) to identify the processes responsible of the uncertainties. In other words, the main conclusion of the paper by pointing where the accuracy is insufficient is somewhat expected (though here it is demonstrated which is better than “expected”) but do not provide information on how to improve these processes, which the hard part of the task. What would it mean to the modeling community if the conclusion of this statistical method was that an empirical model performs better than a physical model ?

These two limitations are mostly inherent to the methodology, not to the paper which is acceptable or could only marginally be improved on these aspects. A part of the results section (see below for details) seems to be particularly sensitive to the choices to generate the ensemble. For DMRT-ML I would recommended to remove the none-sticky case which has been shown to be inadequate in several studies, and for instance use values proposed in studies (e.g. Loewe and Picard, 2015 and Roy et al. 2013 in their discussion). For MEMLS, only two parameterizations of the scattering coefficient have been used for MEMLS while the matlab code proposed a dozen of them. Unless there is a reason (e.g. MEMLS’s authors recommendation), it would be fair to explore all of them. This is only a few suggestions, any change that improve the objectivity of the choices, will improve the strength of the conclusions.

Other minor comments are added below. Then, this paper will be worth publishing because of its originality.

Detailed comments:

P1L5: “(JIM)” is needed for reference at the end of the abstract.

P2L7: “only”. Photogrammetry and altimetry seem to be good (better?) candidates.

P2L9: “because of the because”

P2L30: “carried out Tedesco and Kim”

P3L20: the information in parenthesis is not clear

P5 Eq 6: add “i” in subscript to omega

P5L23: incidence-> zenith

P6 L24: 1) Is the Eq 10 only valid for average angle or this assumption is used to perform the integral ? 2) Please also check if this is the angle or the square sine/cosine of the angle that is averaged. 3) At last, I’d recommend to quote the original statement (in the reference paper) to avoid attributing this (possibly wrong) statement to your study. Or better, provide your own demonstration in Annex.

P7 L3: “Eps_eff” should be defined, here.

P7 Eq 13: “d_o” should be defined.

P8L1. It is not immediately clear where 189 comes from (3 times 63 I suppose). Maybe a slight reformulation would help.

P8L14: “there where”

P8L23: change of the state of the ground is not taken into account whereas it is an important factor in the arctic environment. Could you add information on this and provide some hints on the impact on the simulation performances ?

P9L25: this results seem dependent on the scaling factor for the precipitation. Another factor would give in different results, isn’t it ? If yes, maybe better to remove this part.

P10L11 – L34 and Figure 5. The results on the spread presented in this part and in the figure depend on the choices of the parametrization and these choices seem to me unfair, i.e; orientated so that one model appears to behave a very different way from the others. This difference is however only the consequence of the choice 1) of considering the stickiness as a free parameter and 2) of the particular stickiness values. The very large value (ie. None-sticky case) is known to be unrealistic based on several recent studies. Note also that the Tsang’s group usually uses values of 0.1 or 0.2. The fact that DMRT theory has two parameters to describe the microstructure

while others (apparently) use only one, does not mean that any choice of these two parameters represent snow and are valid. Conversely, why only two parameterizations of Ks in MEMLS has been used while ∼12 difference ones are available in the code ? For HUT, why the numbers appearing in equations 13, 14 and 15 (which are no more than parameters of these equations recommended by some authors) are not freely changed in this study (e.g. +-20%) to reflect the treatment in DMRT ? These choices are subjective and have too much consequences on the conveyed message of this part. This is not critical for the paper as this has no impact on the major conclusions but it suggests that the models are very different, while I thing this is mostly due to the choice. My recommendation is to narrow the range of stickiness in DMRT-ML and to explain why all the MEMLS parameterizations have not been used (or if possible/relevant to used them)

P12L12 – 15. References are needed to support the statements.

P12L25: The given reason is probably not valid, DMRT-ML predicts exactly the same propagation (within the layers) between the two polarizations (isotropic medium). This is the expected behavior for a random medium made of spheres and is indeed a sanity check used to verify the implementation. The difference in the terms of the phase function for H and V polarization in DMRT-ML are purely geometrical and the difference is canceled by the subsequent numerical integrations whereas in IBA MEMLS, it is canceled by analytical integration (or more precisely, it is removed by referring to the physical principles, the same used in DMRT-ML for the sanity check). The difference between TbH and TbV in DMRT-ML as well as in MEMLS and HUT is solely due to the interfaces. The scattering plays a role, but just because of the interaction between the volume and the interfaces, not because of difference of propagation in the volume.

A possible reason of the difference between DMRT-ML and the other models is because the initial choice of scaling (=none, with none-sticky) used in DMRT-ML makes the model really far from the observations, so that the scaling results in a general improvement.

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P13L28. Please distinguish here H and V polarizations.

P14L8. Update the reference.

Figure 7. I suggest to add (light) lines between related points. In addition, the legend is difficult to understand. Please add a reference for “cluster analysis” or use a simple wording to explain what it is. Remove the external reference for the value on the x-axis and if possible add a few words to make the Figure legend more self-sufficient.

Table 3. Ratio is well not defined and difficult to understand. Why not “Comparison of grain diameter simulated by different microstructure models. The mean and max ratio between pair of models is given in columns.” ? Please reformulate.

Table 5. Similarly “Ratio of mean brightness temperature ranges” is not clear. Also: would be “pair” more correct than “two” ?

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