Report on the paper:

Microstructure representation of snow in coupled snowpack and microwave emission models

by Melody Sandells and co-authors

Very interesting paper on the retrieval problem of snowpack brightness temperature (Tb). The approach includes a coupling of a snow model with a microwave emission model (radiative transfer model, RTM). The authors present the results of an impressive number of combinations between snow models with 3 RTM (they used the system JIM, previously developed by R. Essery). Unfortunately, 2 well known snow models, considered as the state-of-art of snow models, Crocus and SNOWPACK, were not considered here (because not compatible with JIM). Moreover, this paper also lacks for providing some recommendations about the best (or the least worse!) of all the combinations tested!

However, this paper is publishable, but needs some clarifications for several points suggested below.
The main concern is that the reader should have a better idea of the order of magnitude of errors compared to those obtained when the RMT are driven by snow measurements (which can be considered as the reference level for RMSE and bias generally obtained in practice, under best conditions). There are a lot of papers from Finish, Canadian or American groups who provide such values. Do the scaled snow models reach the same mean level of RMSE when compared to measurements?
End of page 13 and beginning of page 14: You must clearly mention that when considering SSA measurements for retrieving the optical diameter as inputs of RTM, one must scaled the measurements too.
Also few details are given about the stratification of the snowpack considered in simulations? This is a well known significant problem for simulating Tb.

All the figures are of very poor quality, making this revision very difficult, perhaps this is an issue with building the pdf for submission, but it should be considered prior to next revision; text of scales is really too small.

Specific comments

I am not sure about the interest of giving all the equations in Section 2.3, especially if there are errors! See Eq. 7: (1-*f*) should be in numerator, i.e, factor of (es-eb).

Eq. 8 "it is given by the largest solution to…" largest of what?

Page 7 beginning of the Section 2.4: Why to combine HUT-MEMLS? not clear.

Table 6 : The too much detailed analysis of the differences obtained here are probably not significant? A more synthetic analysis of results should be presented: mainly RMSE identical for the 3 models, large differences between years?

Again, page 11, line 17: "the lowest RMSE in brightness temperature is reduced, as shown in Table 8 " probably not statistically significant?

Page 12, line 28 : "For DMRT-ML, consideration of the stickiness is imperative, with the choice of microstructure evolution model of secondary importance. " This statement is in agreement with Roy et al. (2013).

Page 13 "whereas the range in Brucker et al. (2011) was 0.25-0.4 for snow density between 100 and 400 kg m ^{3}. " In the Brucker's paper, it is the reverse. The scaling factor noted Beta = 0.63, and this sentence refers to : A = [2/3 Beta (1 –*f*)]. Thus, when the density is 100 kg/m^3, A = 0.4 and when the density = 400 kg/m^3, A = 0.24.

Figure 2: I cannot read on the this figure if ice crusts have occurred during these two years (i.e precipitations during winter with T>0) ? This has certainly been observed if this was the case. Please clarify.

Figure 3 : A minor observation for the SWE measurements using the GMON: the authors observed a typical artefact of this instrument at the end of the season, when the soil is very wet due to the melting snow, leading to a strong anomaly in measurements (very high values in what is interpreted as SWE, while there is no more snow!).

Table 4 could be interesting in percentage?