General comments

The paper by Galligani et al. attempts to improve the understanding of the interaction between microwave radiation and frozen particles with the ultimate goal to better exploit current and future space borne measurements. The authors nicely illustrate that observed deviations between simulated observations and their real counterpart can be caused by both 1) deficits in the simulation of hydrometeors in the underlying atmospheric model (here Meso-NH) and 2) the different assumptions (density, shape..) used for deriving the single scattering properties. Shedding light on the second point is urgently needed to pave the way for important applications of the satellite data, e.g., the assimilation of radiance affected by ice scattering, improvement of the representation of ice clouds in climate model, and makes the paper worth publishing. The paper is also innovative as to my knowledge the approach to exploit both the active and the passive microwave signal in a physically consistent way to explain the observed signatures within a snowfall event has not been published before.

The main deficit of the paper is that the case for finding out which assumptions/setting provide microwave signals reproduce the observations is made in a try and error study and for only one single case study. While I understand that for detailed investigations it is ok to stick to only one case, section 4 “Comparison of the simulations with coincident observation” needs to be revised to undermine the conclusion that the Liu soft sphere parameterization is indeed the best one. Aren’t there any other combination of that could provide the same answer? Below I will give some specific ideas how this could be realized, e.g. by including a table, modifying figures.

It would be good to know more about the specific case and why it was chosen. I am in particular interested whether surface temperatures were below zero in the region of the low brightness temperatures and (hopefully no) liquid complicates the RT. My worry is that also other factors - not only the Meso-NH modelled snow that the authors adjust within a limited range - could contribute to the differences between model and obs, namely 1) the existence (and modelling) of supercooled liquid water and 2) the distribution of ice between “pristine ice”, “graupel” and “snow”.

Specific comments

1) Introduction, l50: it might be worth to also mention that without better understanding of the snow scattering the huge potential of satellite radiance for data assimilation is lost.

2) Section 2.3: Is it possible to give some information on how homogeneous the “stratiform” snowfall event was to better justify the temporal difference between the observations. Maybe there are some ground-observations that support this. Anyway it would be good to know more details on the strength of the event, e.g. snowfall rate, accumulation.

3) Line 247: How strong is attenuation during this event? I would hope that there is hardly any liquid in the core region leading to significant attenuation.

4) Line 306: You should mention that the absorption coefficient of supercooled liquid is quite uncertain with big differences (up to 10 K) between different absorption models especially at the higher window frequencies (Kneifel et al., 2014). Unfortunately the Liebe model is the not the one which seems to perform best. Furthermore the existence of liquid could dampen the scattering effect.

5) Section 4.2: This is my main point: Several changes are made but only results from a few of them are shown in figures. There need to be a more objective and traceable criteria for decision-making. For example, the short sentence on the impact of the wetness degree of snow in the text is confusing as very little information is given. As Fig. 6 mentions dry snow I was always looking for the wet.... In this respect it might be also dangerous to change the degree of wetness for all grid cells containing snow as the wetness degree is probably a function of humidity. My suggestion is that the authors generate a table where they list the

different settings and explain better what they did. It would be good to get some objective criteria for judging the impact of the assumption/change in respect to the control run. This could be the minimum (or better 95 percentile) of BT at the different frequencies or the maximum (95 percentile) of radar reflectivity. This is in line with something like the 5K statement in line 487 but it would be good to better define how such number is derived, e.g., over which range/interval, and how it compares to the other assumptions. As it is now described in the text I find the argumentation not very convincing

	MHS MIN (BT)	Cloudsat Max(Z)	Comment
control			

6) The big question is always how well does the model simulate the different hydrometeors. Couldn't be a large factor of integrated snow than 1.25? As shown for example in the Waliser paper models have different distributions between pristine ice and snow. This has a big impact on the scattering properties as the small pristine ice particles scatter much less than snow. Looking at Fig. 7 there is quite a difference between model and Cloudsat in the upper part of the cloud where pristine ice should dominates. In particular I am worried that snow is dominating ice and thus the pristine clouds disappears in the Liu approximation. It would be very helpful if also the vertical distribution of hydrometeors could be shown for Fig.7 in analogy to Fig. 1. Fig. 7 is very small and difficult to read so it would be very helpful to enlarge both of – maybe by cutting at 10 km.

7) For many readers it would be good to also mention how the ARTS radar simulator compares to Quickbeam (Haynes et al., 2007) which is frequently used for model evaluation.

Technical corrections

Line 77: you say "one of the studied snowfall cases" in the introduction but later only mention one case: Is there more information available?

Line 133: terminal velocity

Equation (4): Shouldn't G be g?

Line 153: The sentence is not correct – better "..ECMWF analysis from 8 Dec .. and run with lateral boundary cond.."

Line 255: I couldn't find information on the radar module in the ARTS user guide – please specify availability.

Line 301: Why density of 0.941 and not the literature value of 0.9167?

Line 375: You should mention the brightness temperature depression already a bit earlier

Line 397: There is now basis for your statement that obs at 89 and 157 GHz are most sensitive to the snow column – you need to show that.

Check your spelling in text and figures for RO-IWP like in Line 440 IROIWP. It should always be similar with "-".

Line 418: "..Cloud-Sat footprint and compared with three different algorithms." Otherwise it is confusing. You can remove the three algorithms in line 435.

Line 477: Well you might have expected it but I would say "Consistent with this figure (Fig.&)..

Figures:

- Could you maybe show the zero degree line at the surface in these plots?

- Remove Fig. 8 and put it in instead of Fig. 2.

- Add the CloudSat track in Fig. 2/8 A landmark is urgently needed to better compare the different images – first I thought just of a cross for Paris but I think the Cloudsat track is otherwise not shown.

- Fig. 3. Add the PDF also for the soft spheres to show that there is no other trade off. YOu might leave out the other channels as they are not discussed anyhow and enlarge the 89 and 157 GHz.

References

Haynes, J.M., R.T. Marchand, Z. Luo, A. Bodas-Salcedo, and G.L. Stephens, 2007: A multi-purpose radar simulation package: QuickBeam. *Bull. Amer. Meteor. Soc.*, **88**, 1723-1727.

Kneifel, S, S. Redl, E. Orlandi, U. Löhnert, M. P. Cadeddu, D. D. Turner, and M-T. Chen: Absorption Properties of Supercooled Liquid Water between 31 and 225 GHz: Evaluation of Absorption Models Using Ground-based Observations, *Journal of Applied Meteorology and Climatology*, 53, 1028–1045, DOI:10.1175/JAMC-D-13-0214.1.