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Atmospheric Measurement Techniques Discussions

Interactive comment on "Meso-scale modeling and radiative transfer simulations of a snowfall event over France at microwaves for passive and active modes and evaluation with satellite observations" *by* V. S. Galligani et al.

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AUTHORS: We would like to thank the reviewer for his/her comments. We attempt to answer to his/her comments below.

**** Review 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.

AUTHORS: We agree. We included the following line: "This is crucial to assimilate all-sky radiances" in the text.

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.

AUTHORS: The temporal difference between Meso-NH outputs and the observations is due to a mislocation of the cloud structure, mainly due to the re-analysis inputs (MACC reanalyses from ECMWF). Nonetheless, the spatial structures are fairly well represented if we analyse this comparison (Meso-NH/simulations): the mesoscale cloud model is able to reproduce the thermodynamic nature of the precipitating system. In terms of adding more details to describe the strength of the event, the following was added in Section 2.2: "The case is associated with a broad pattern of moderate to heavy rainfall of a few mm/hr. Maximum accumulated rainfall and snowfall of 30 and 5 mm respectively has been measured over the whole event in northern and central France, with intensities of a few millimeters per hour (up to 5 mm/hour for rain and 1 mm/hour for snow)." Figure R1 is not to be included in the paper, but is shown here for reference (attached). Note that the high snowfall rates in France are located in the Paris area and north of the high rainfall rates.

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.

AUTHORS: As seen in Figure R1 and Figure 1, Meso-NH models high contents in LWP and IWP at different regions. Figure R2 clarifies this point. In the region simulated (west of 2.5 longitude) Meso-NH does not model any liquid. Note also that the freezing level dropped from approximately 4 km to below 0.5 km in the region of interest.

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.

AUTHORS: The authors have added a line mentioning this: " The accuracy of liquid water absorption models in the microwave for supercooled liquid (i.e., liquid water less than 0° C), is however, uncertain with differences up to 10 K between different absorption models especially at the higher window frequencies (Kneifel et al., 2014)."

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.

AUTHORS: Comments received by the other referee also ask for a better explanation to the steps taken in the different simulations: "My recommendation is to provide a more clear depiction of what assumptions are present for a given analysis, including but not limited to: dielectric constants and assumed temperature of ice and water, dielectric

mixing method used, shape assumptions used, particle size distribution assumptions (parameters of the PSD), how truncation of the PSD tails are handled, cloud liquid water attenuation / emission, surface property assumptions for TB calculations, meltwater generation assumptions in melting-layer regions and how the PSDs are being modified as melting occurs (i.e., how does the ice get converted to rain)."

We believe this comment was successfully addressed and that it will guide the reader better through a systematic explanation of what was done (see interactive comments reply and revised version of the manuscript)

The authors find important to make a remark about the wetness: wetness is mentioned together with many other assumptions that were run while using the Meso-NH intrinsic micro-physical properties to stay consistent with Meso-NH. None of these configurations that were run are shown. We have added a "(not shown)" for a better guidance through the text. It is true that it is dangerous to change it for all grid cells containing snow, for this reason we added the following in parenthesis which explains better how we took the wetness degree into account: "(wetness degree W(%)=0 for T<258.15 K, W(%)=T-258.15 for 258.15 K<T<273.15 K, and W(%)=15 for T<273.15 K from Skofronick-Jackson et al., 2002)".

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.

AUTHORS: The very complex microphysics of frozen hydrometeors are one reason for the large uncertainties in predicting cloud ice, meaning the frozen phase, with climate models like Meso-NH [e.g., Stephens et al., 2002; Waliser et al., 2009]. The distribution of the frozen phase between "pristine ice", "graupel" and "snow", i.e., the ability of models to accurately describe the conversion mechanisms, e.g., from pristine ice crystals to aggregated snow or the interaction of cloud ice with the liquid phase is still poorly understood. The prediction of total cloud ice by global circulation models deviate currently by a factor of 20 between models [Waliser et al., 2009]. In general, the uncertainty in the prediction of precipitating cloud ice, i.e., basically snow and graupel, is even larger, because additional assumptions about the sedimentation and aggregation processes have to be made [Kneifel et al., 2010]. The analysis from Figure 3 aimed to discuss the ability of Meso-NH to simulate the different hydrometeors. Figure 3 shows that Meso-NH model outputs west of 2.3° significantly underestimate the integrated frozen phase content, which coincides with a large difference in the simulated vs. observed radar reflectivity in the upper part of the cloud. We acknowledge that the upper part of the cloud is composed of smaller pristine ice particles, but they scatter much less than snow because they are very small as compared to the wavelengths here. Figure R3 shows CloudSat Ze together with the "pristine ice", "snow" and "graupel" species content as modeled by Meso-NH and the IWC retrievals by CloudSat at two different cross sections along the CloudSat track. The first cross section is located at 2.5° longitude and the second one at 2° longitude where Meso-NH significantly underestimates the frozen phase. As shown in Figure R3, at 2° longitude, Meso-NH fails to model the high altitude frozen phase, i.e., "pristine ice", accurately, as compared to the 2.5° longitude cross section where the distribution of "pristine ice", "snow" and "graupel" seems to more accurately match the CloudSat retrievals. This is now emphasized in the text. For the rest of the cloudsat track analyzed, however, it is reasonable to assume Meso-NH outputs are reasonable from similar analysis, and that despite uncertainties, the underlining complexities arise from determining accurate scattering properties. The 1.25 value arises from the analysis of Figure 3, where the ratio be-

tween the two estimates is of the order of 1.25. Note that Figure 7 has been made larger.

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.

AUTHORS: The equations behind Quickbeam and the ARTS radar module are the same, and take into account the attenuation of both gas and hydro-meteors. The main difference is that the ARTS radar module, just like ARTS passive simulations, depends on defining externally the single scattering properties and PSD. The Quickbeam module, on the contrary, has pre-defined Mie tables or on the fly calculations, as well as particle size distribution. Quickbeam has also a melting layer flag which activates a simple melting layer model. The authors have not conducted a thorough comparison to make a detailed comparison.

*** 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?

AUTHORS: Another real scene was studied in detail where only passive observations were available. This scene corresponded to a light precipitation event over the Rhine area, characterized by the presence of suspended snow. Where only passive observations were available, similar conclusions were achieved with respect to the sensitivity analysis conducted. This case is not shown here as the authors wanted to focus on the active/passive synergy. The heavy snowfall scene that is presented in the paper showed very encouraging results, as both active and passive observations can be reasonably simulated from consistent assumptions. The authors propose to re-write this sentence as "Section 2 presents a heavy snowfall scene, [. . .]".

Line 133: terminal velocity Equation (4): Shouldn't G be g?

AUTHORS: Yes. We have corrected this typo.

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

AUTHORS: It was re-written to: "Meso-NH was initialized using ECMWF analysis from 8 December 2010 at 0000 UTC and run with lateral boundary conditions linearly interpolated from the ECMWF 6-hourly analyses (successively taken at 0600 UTC, 1200 UTC, etc.)".

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

AUTHORS: It is in the ARTS Development Version User Guide, as described in the text. (Chapter 19 in http://AUTHORS: AUTHORS: AUTHORS: www.sat.ltu.se/arts/misc/arts-doc/uguide/arts_user.pdf)

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

AUTHORS: This is another typo. It has been corrected in the text. The value used in the calculations is that of 0.917

Line 375: You should mention the brightness temperature depression already a bit earlier. Line 397: There is nowAUTHORS: AUTHORS: basis for your statement that obs at 89 and 157 GHz are most sensitive to the snow column – you need to show that.

AUTHORS: We now make reference to it at the start of Section 4.1. The first sentence now reads "Aclose examination of MHS observations from the scene of interest (top panels of Figure 2) and the Meso-NH outputs in Figure 1 (and the hourly Meso-NH outputs not shown here) reveals that the cloud system modeled by Meso-NH, evidenced by its brightness temperature depressions in the window channels of MHS, is slightly time lagged with respect the observations."

Check your spelling in text and figures for RO-IWP like in Line 440 IROIWP. It should

always be similar with "-".

AUTHORS: Thank you for noticing this. We now have consistent labels.

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

AUTHORS: The new sentence reads: "In order to carry out this comparison, the three frozen species from Meso-NH are summed (ice, graupel and snow) along the Cloud-Sat footprint and compared with three different retrieval algorithms."

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

AUTHORS: Yes. The new version reads "Consistent with Figure 2, Figure 6 fails to reproduce intense scattering"

*** Figures:

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

AUTHORS: I am assuming the referee is talking about Figure 7. Figure 7 has been discussed above already.

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

AUTHORS: The authors believe it is best to introduce the final simulations in a new figure. Otherwise there are simulations which are present and not introduced for 5 pages.

- 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 of Paris but I think the Cloudsat track is otherwise not shown.

AUTHORS: It makes a lot of sense to add the CloudSat track. Added in black and legend updated.

- 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.

AUTHORS: I am sorry, the authors do not understand this comment, this is for soft spheres.

*** 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.

AUTHORS: No comparison made with quickbeam in the article itself.

Kneifel, S, S. Redl, E. Orlandi, U. LoÌĹhnert, 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.

AUTHORS: Added.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 7175, 2014.

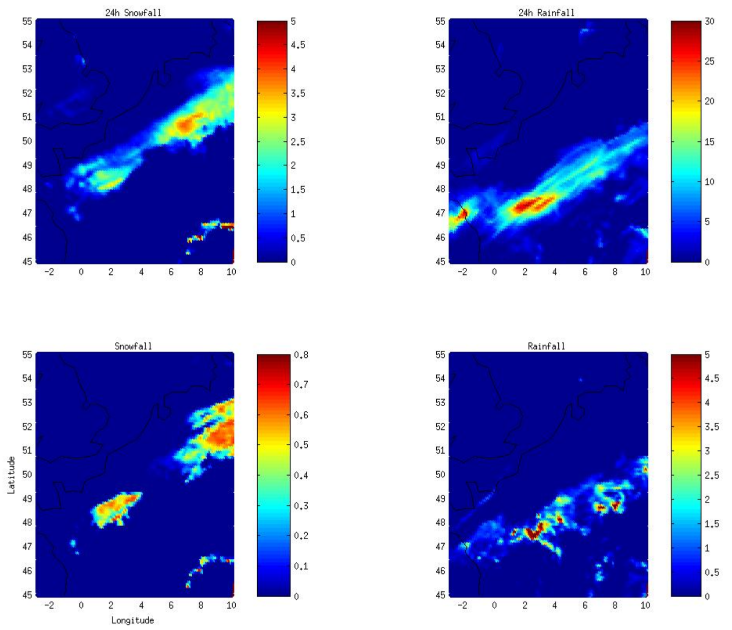

Fig. 1. R1: The top panels show the accumulated snowfall and rainfall over the 24 hours Meso-NH was run. The bottom panels show the snowfall and rainfall rates Meso-NH modelled at the time of the MHS passage

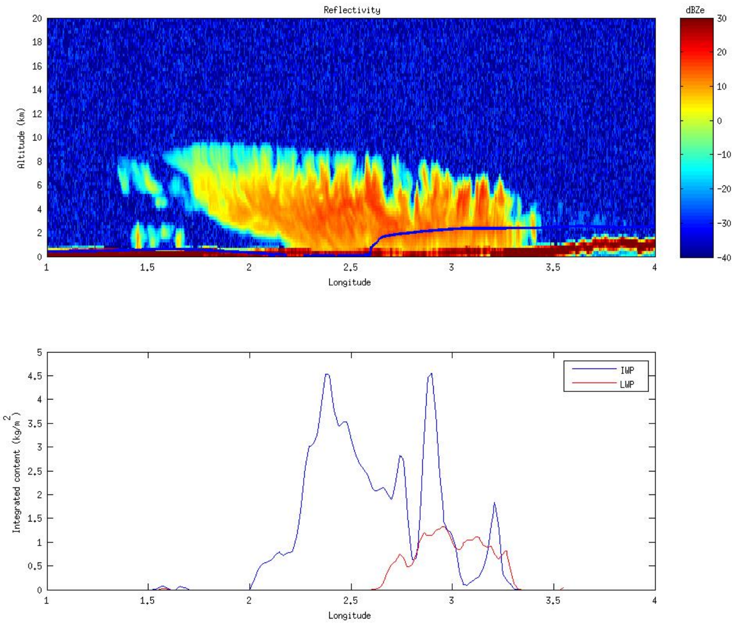

Fig. 2. R2: The top panel shows the CPR Ze and the CPR retrieved freezing level in blue, and the bottom panel shows the coincident Meso-NH total IWP (snow+graupel+ice) and total LWP (rain and cloud)

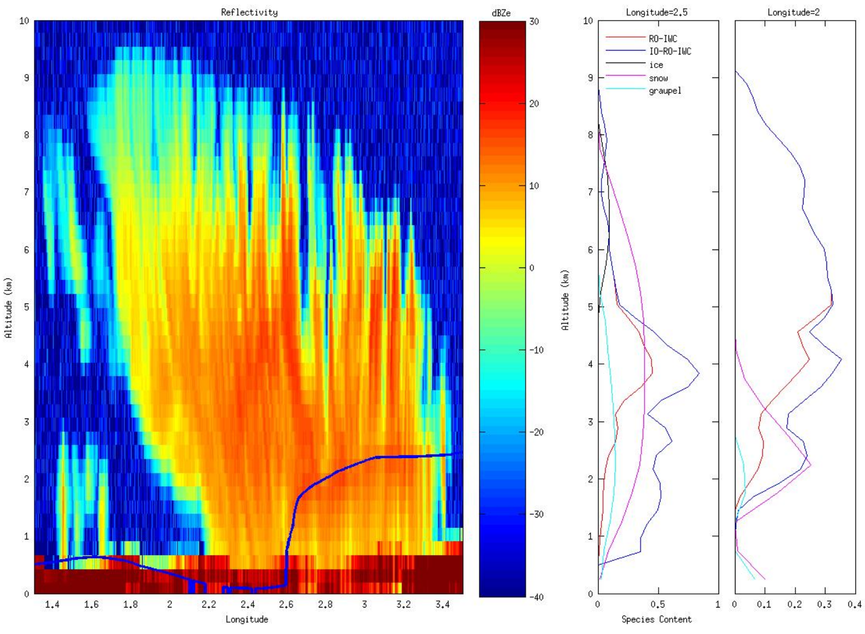

Fig. 3. R3: The observed Ze and the Meso-NH frozen phase and CloudSat retrieval profiles at crossections located at longitudes 2.5° and 2.3° along the CloudSat track.

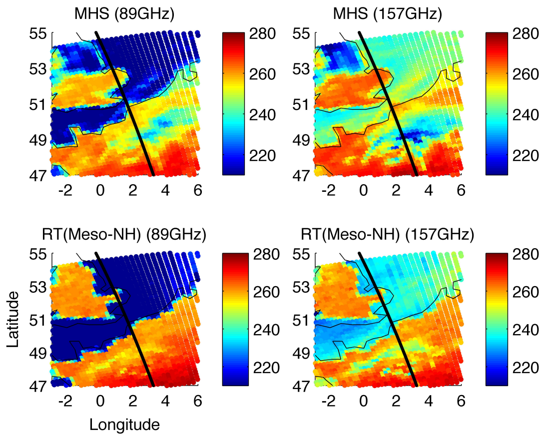

Fig. 4. Review Figure F2

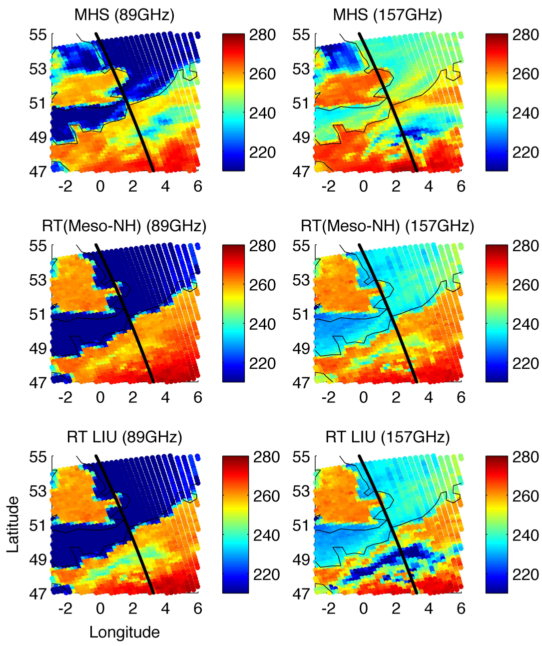

Fig. 5. Review Figure F8

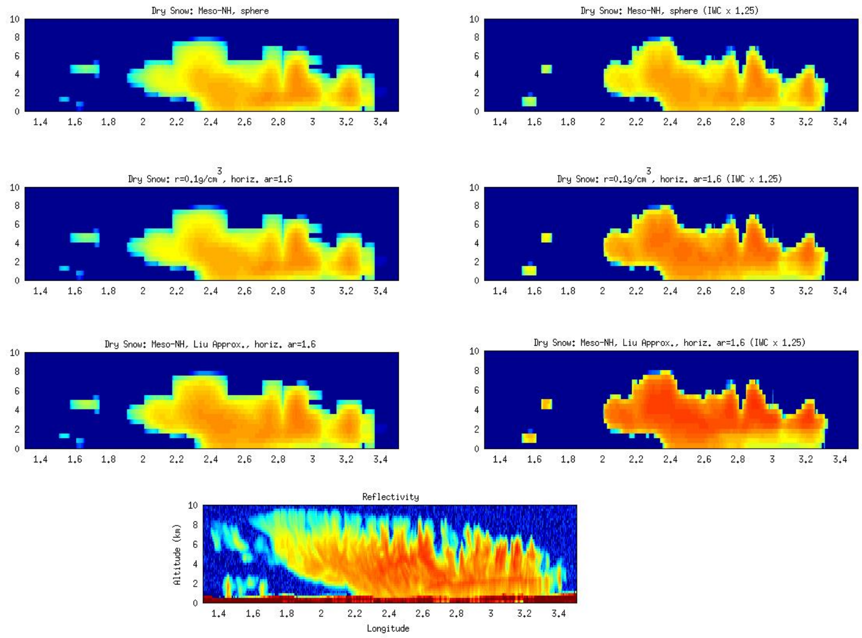

Fig. 6. Review Figure F7