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Geoscientific Model Development Discussions

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Interactive comment on "PAMTRA 1.0: A Passive and Active Microwave radiative TRAnsfer tool for simulating radiometer and radar measurements of the cloudy atmosphere" *by* Mario Mech et al.

Anonymous Referee #2

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This paper documents a new simulator for passive and active measurements, with a series of test cases illustrating its features. It is a useful documentation of the model and also a nice demonstration of how microwave observations can inform microphysical model developments. The work is in good shape and I only have minor comments.

Bigger minor comments

1) Throughout the examples in section 3, it would be good to have clearer documentation of the atmospheric model (ICON-LEM) and the exact settings of the radiative transfer model (as PAMTRA has a number of options, shown in Table 1).

a) It would be useful to have a short section to centralise the description of, and give

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further details on, ICON-LEM. It is important to know the type of microphysics schemes being employed, and which prognostic and active variables are used (e.g. which hydrometeors are represented, and which moments?) Is there any possibility of a mismatch in assumptions (e.g. PSD, shape, fallspeed) between those in PAMTRA and in the model?

b) The PAMTRA settings used in the examples in section 3 need to be more clearly stated. One option might be to extend Table 1.

2) Section 3.1 uses the IFS as input to PAMTRA and compares to AMSU-A and MHS. There are a few issues here:

a) The inputs to PAMTRA likely only include the four prognostic hydrometeors from the large-scale cloud parametrisation (P12 L17). This is insufficient to replicate observed brightness temperatures. In the all-sky forward modelling of passive microwave data at ECMWF, the convective hydrometeors from the convection schem are also included (see e.g. Geer and Baordo, 2014, section 2.2). However these fields are not available from the standard archived ECMWF products. If the convective hydrometeors were added, brightness temperature depressions in frontal areas (which often contain embedded convection) would likely be deeper.

b) This text is overly strong: <Brightness temperature depression> "is even stronger than in the observation of MHS for the north-eastern area. With the aforementioned capability of SSRGA to reproduce TB depressions in agreement with observations, this overestimation can be linked to an overestimation of snow water content of ECMWF IFS". The implication from more extensive comparisons in Geer and Baordo (2014) would be that snow water content in the IFS in frontal areas is consistent with observations, at least within the uncertainty on the assumed PSDs and particle shapes in the radiative transfer. The authors seem to be claiming that SSRGA is perfect and the IFS is wrong. It is unlikely that simple, especially given point (a) which, if addressed, would likely make the overestimation of simulated brightness temperature depressions look even worse when using SSRGA.

Other minor comments

1) The introduction motivates the idea of using remote sensing measurements "for improving the atmospheric models" (e.g. P2 L29). However (P2 L30) when describing the importance of these measurements in data assimilation and NWP, it would be possible to infer that model validation was still their main purpose. It would be worth making it more explicit that the main aim of using these observations in NWP is to infer initial conditions for weather forecasts (the aim to improve models is not yet so well developed in NWP.)

2) P3 L17 suggests that hydrometeor single scattering properties for fast R/T models are derived from line-by-line models, which is not correct. It would be best just to remove the mention of single scattering properties here.

3) P3 L21 I suggest to delete "principally" as it is not clear what this means in the context.

4) P4 L12 -> P6 L2 gives a discussion on horizontal homogeneity, suggesting it is not important in microwave radiative transfer. This is not correct, because the beamfilling effect (due to the nonlinear dependence of backscatter or brightness temperature on water content) means there is ambiguity between water mass and water inhomogeneity at scales below the model grid or sensor field of view. The importance of horizontal inhomogeneity in forward modelling for NWP is illustrated by, among others, Geer et al. (2009) and references therein. However, it's easy to deal with horizontal inhomogeneity by using the independent column approximation. Presumably what the authors really mean is that full 3D radiative transfer with horizontal inhomogeneity is unnecessary.

5) P6 L5 The description of the doubling-adding method in this paragraph is not particularly helpful, and it closely follows the description in Evans and Stephens (1995) which itself doesn't much help summarise the method or ideas like the "interaction principle"

or “initialisation”. There might well be a textbook that can help the authors formulate a clearer and simpler description of the technique - is it covered in Petty (2006) or Thomas and Stamnes (2002), for example?

6) P6 L25 The word “However” suggests a dependence between the first part of the paragraph (on the dielectric factor) and the second part (on multiple scattering). In practice these are two completely separate issues, Maybe the second part of the paragraph would be better introduced with “Another issue” rather than “However”?

7) P6 L30 “the minimal sensitivity” - this is unclear and would still be unclear if what the authors mean is “the minimum sensitivity”. Is it rather the radar noise that is being referred to?

8) P7 L16 “v_nyq” - is it worth explaining why this parameter is called “nyq” or giving it a simpler notation? (since Nyquist is not mentioned in the text here)

9) Section 2.4 describes the Stokes reflection matrix but is insufficiently clear on how this is being set up, particularly for the components that describe non-specular reflection. For example TELSEM, TESSEM and FASTEM are all emissivity schemes that assume specular and non-polarised reflection at the surface, and provide a simple emissivity to describe this. Yet the text implies they provide a full reflection matrix. There is also an ambiguity as to whether TESSEM is providing just the roughness and foam coverage corrections, or the entire emissivity and reflectivity calculation (P9 L8). A much clearer description is needed here, given that determining the full Stokes reflection matrix (including polarisation changes and non-specular reflections) is not at all straightforward.

10) P9 L23 “particle maximum extend” should have a clearer definition (and “extent”, not “extend”, is probably intended)

11) P10 L7 consider defining M_k with an equation so that it’s easier to understand why q=aM_b.

12) P11 L9-10 suggests that the reason to choose Mie or T-Matrix is simply speed - surely it's whether you have a sphere or a spheroid?

13) P14 L2 - the surface is very often visible in satellite 157 GHz observations, outside the humid conditions of the tropics, so errors in the surface representation could very well be suspected here. If the authors want to claim that "the surface influence can be neglected" this would need to be backed up by a map of the surface-to-space transmittances at 157 GHz for this case study.

14) P15 L32-33 - the Arctic is very far from a "measurement void", since polar orbiting operational meteorological satellites cover it with very high temporal frequency. The authors should be more specific on this point.

15) P20 L26 Using "adiabatic" to describe the droplet size variation with height is loose terminology and should be improved - "adiabatic" of course refers to thermodynamic processes, and the radius of water droplets won't change much under a true adiabatic assumption (water being incompressible).

16) P20 L32 and surrounding discussion is initially confusing. It could be more clearly stated in the text that the Doppler spectrum is only simulated, not observed. The suggestion that the larger droplets (secondary peaks in the Doppler spectrum) are "invisible" in the in-situ measurements is confusing as they must be present in the data, just not visible on the colour scale chosen for this plot, or possibly hidden under the white line.

Typos

P4 L14 "plan parallel" -> "plane parallel"

P18 L11 and L22 "extend" -> "extent"

P18 L24 "resulting" -> "resulting simulated"

Bibliography

Only citations not already listed in the bibliography of the paper under review are given here.

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