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Single Footprint Retrievals for AIRS using a Fast TwoSlab Cloud-Representation Model and All-Sky Radiative Transfer Algorithm"

by DeSouza-Machado et. al.

We thank the reviewers for their comments, questions and suggestions to improve the paper. Below we detail our responses to their concerns. For ease of review, we type-faced the reviewers questions in blue. When we refer to pages and line numbers in our answers, the context should make it clear whether we are talking about the original manuscript or our current revised manuscript.

Reviewer 2

General comments

1) It should be mentioned in the title that the All-sky radiative transfer algorithm cover the infrared spectral range.
Fixed

2) The last sentence of the second paragraph of the introduction seems a bit too simplistic considering the large amount of works done by national weather services to assimilate infrared cloudy radiances in NWP. If the cloud-clearing method is operationally used by NOAA and NASA, other methods such as CO2-slicing, Maximum residual method and 1DVAR are used to characterize single layer cloud for operational application. I suggest the author to provide at least some references to these works.
As noted in the document (Page 2, line 7-8) NWP centers currently only assimilate clear sky infrared radiances - they determine cloud top altitudes, and then assimilate the radiances of channels whose weighting functions peak above these cloud tops. Assimilating allsky infrared radiances is still a work in progress. We have rewritten that sentence and included the following two references at the end (Page 2, line 8) : "In addition for any given scene, from a pre-determined subset of IR sounder channels, Numerical Weather Prediction (NWP) centers generally only assimilate the radiances that have been deemed unaffected by clouds."
Reale, O., K.M. Lau, J. Susskind and R. Rosenberg (2012), AIRS impact on analysis of an extreme rainfall event (Indus River, Valley, Pakistan 2010) with a global data assimilation and forecast system, J. Geophys. Res., 117, DOI: 10.1029/2011JD017093.
Bauer, P., T. Auligne, W. Bell, A. Geer, V. Guidard, S. Heilliete, M. Kazumori, M.-J. Kim, E. H.-C. Liu, A.P. McNally, B. MacPherson, K. Okamato, R. Renshaw, L.-P. Riishojgaard (2011), Satellite cloud and precipitation assimilation at operational NWP centres, DOI:10.1002/qj.905

3) I do not see the utility of Figures 13 and 14. Does the authors want to explain why ECMWF is better than climatology for the retrieval ? If yes, then it must be stated in the text.
This has already been partially covered in the first paragraph of section 6.1.2 : you need to start with a first guess that is as close to the actual state as is possible. Our work with comparing clear sky subsets of AIRS radiances, against radiative transfer calculations using ECMWF model thermodynamic fields, as well as other comparisons against radiosondes, demonstrates that even with space/time mis-matches these ECMWF thermodynamic fields are quite accurate. Conversely for the granule used in the paper as our retrieval demonstration, there were many local convective regions which would not be in the climatology. Figure 15 shows that climatology was very smooth and did not have any of the structure seen by the AIRS L2 retrievals or our retrievals, or that was in the ECMWF model fields.
We have added/changed the following sentences in Section 6.1.2 (bottom/top of Pages 22/23) "We point out that the thermodynamic fields from ECMWF 3-hour forecasts (and/or analysis) are nearly identical to global radiosonde measurements (see for example the figures in Section 3 of [Ingleby,

2017]), and would also be an ideal starting point for the temperature and humidity profiles. However for this "proof-of-concept" paper the temperature and water vapor profile linearization point and *a-priori* is instead taken from a climatology in order to more easily demonstrate the performance of the retrieval algorithm and the cloud and thermodynamic information contained in the AIRS radiances. "

Specific comments

1)Page 4, line 31: What is the spectral resolution of AIRS? Is the typical 0.2 K noise for cold or hot scenes?
The nominal resolution is $\nu/\delta\nu \sim 1200$ so for example FWHM $\sim$ 0.5,1.0,2.0 cm^{-1} at 600,1200,2400 cm^{-1} respectively. We chose to add that the NeDT is 0.2 K noise at 250K on Page 5, line 5

2) Page 5, line 10: replace 00.00 by 00:00
Fixed

3) Page 5, line 12: (latitude,longitudes) can be remove if the unit of 0.25+/-0.05 is given.
Fixed

4) Page 6, line 8: The sentence is not precise enough. Do you mean gamma size distribution? If yes then the unit of the effective variance should be added as well as the effective radius or diameter.
Agreed, this line is fixed to now read "water cloud scattering parameters are computed using Mie scattering coefficients using water refractive indices from the Optical Properties of Aerosols and Clouds (OPAC) database. The parameters are integrated over a modified gamma droplet size distribution of effective variance 0.1 (dimensionless), and effective radius (typically) of 20 μm "

5) Page 6, line 16: Does SARTA use the same refractive indices as PCRTM ?
See above

6) Page 6, line 26: A space is missing before the reference.
Fixed

7) Page 7, line 11: Why cloud content profile are smoothed?
The profiles can have a lot of vertical structure and are sometimes multi peaked; we wanted to smooth out fine structure and so have a cleaner profile for our algorithm to locate the slabs. As is mentioned in the manuscript, we do have a lot of flexibility in the final slab placement. The relevant sentence (Page 7, Line 26) has been changed to "is first smoothed in order to make it easier to localize the positioning of the (ice or water) cloud slabs."

8) Page 7, line 23: Are case 2 often happen?
A random check of our data shows 5-10% of the cloud profiles were reduced so both slabs were water (typically over tropical regions) and 1-4% of FOVS had both clouds as ice (typically over the polar regions).

9) Page 7, line 30: What is the justification of adding a random offset to the effective diameter?
Water cloud effective diameters vary with season and geographic location. Without getting too much into details we wanted a first cut at modeling this, and plan to be more systematic in the future. We have changed Page 9, Line 9-10 to reflect this and added a reference
"Water cloud droplet effective diameters vary with season and geographic location (King et al., 2013); to model this we use an effective diameter of 20 um plus a uniformly distributed random offset. "

10)Page 9, line 6: I think the word types should replaced by layers if case 2 happen.
We agree this could be better written, and changed "types" to "slabs". We also noticed we had an indexing problem in the same line and now use $cx_j, j = 1, 2$ (and now explicitly state i is the channel index)

11)Page 11, line 17: What are the standard deviation or RMS of the difference Observation minus simulation? Are they comparable between SARTA and PCRTM?

Table 1 gives very representative numbers for the case of all night time observations (standard deviations of about 11 K). For this subset case of 1000 scenes (which as explained in Appendix III of the text were explicitly chosen for cloud variability), the standard deviations between obs and (SARTA/TwoSlab or PCRTM/MRO) are also very similar (about 22 K); we have now included this information.

12) Page 13, line 15: The first bracket of the second reference is not at the right place.

Fixed

13) Page 14, line 3: replace 1 1/2 by 1.5 for consistency

Fixed

14) Page 15, line 1: I do not see difference in the slab position between blue and cyan. Can you explain it better?

We assume the referee is looking at Figure 6. There indeed are differences between the blue (P = peak) and cyan (C = centroid) curves. We have slightly changed the wording of the final paragraph of Page 16, adding Lines 8-13 "This panel magnifies the differences shown in the left hand panel. For example when the observed clouds are cold (high clouds), one would expect placing the (ice) slab cloud would produce as high as possible (P) would produce a smaller bias than if you placed the slab cloud lower down in the atmosphere, at the centroid (C). Indeed this is clearly seen in the right hand panel - the blue (peak) bias for the cold clouds (BT 1231 $\leq$ 250 K) is noticeably less than the cyan (C) bias."

15) Page 15, line 5: The first bracket of the reference is not at the right place. I also suggest to refer the listing (1), (2), (3) and (4) to the figure.

Fixed

16) Page 16, line 3: Positions (1), (2) and (3) are not represented on the right panel of Figure 6.

Fixed

17) Page 17, line 1: Are pdfs normalized? If yes it should be mentioned both in the text and in the figure caption.

Correct, thanks for pointing out this omission

18) Page 17, line 19: Is these interpretations have been already shown by other studies?

Not as far as we are aware of. More groups are now producing scattering infrared RTAs, so this could be one of the topics of future allsky RTA inter-comparisons studies.

19) Page 17, line 22: I suppose ice contamination is sea-ice?

Correct, thanks for pointing this out, we added the clarification

20) Page 17, line 28: This sentence seems to repeat the sentence before, please reformulate.

We have rewritten this (Page 19, Line 14-16) as "The calculations for the polar regions are noticeably warmer than the polar observations, with the SARTA/TwoSlab and PCRTM/MRO clouds simulations much more similar to each other than to the observations."

21) Page 19, line 5: There is again a bracket problem with this reference.

Fixed

22) Page 20, line 11: Replace Tikonov by Tikhonov.

Fixed

23) Page 20, line 13: Put the references before the dot.

Fixed

24) Page 20, line 16: The forward model error has been set to be ≤ 0.2K. This is very optimistic for infrared cloudy simulations. As comparison in figure 5, you found a standard deviation of 20 K when comparing observation with RTM. How do you justify that?

The 20 K standard deviation mentioned in Figure 5 arises is a consequence of the above mentioned cloud mismatch between observations and NWP model fields, so is not a forward model error.

As noted in Comment 18 above, there are now a number of RTAs capable of handling cloudy calculations, but they have different cloud representations (for example Maximum Random Overlap, Exponential Random Overlap, our TwoSlab approach and so on). Plus they use different cirrus/water scattering parameters. Due to the time mis-match between observations and NWP fields, no definite study has/can be made, about which is the most appropriate cloud representation that reconciles hyperspectral infrared observations with cloudy RTA calculations. A future intercomparison of these codes against in-situ cloud observations would be beneficial.

In any case, we run into the problem of the practicality of these complex cloud representations for use in a physical retrieval algorithm. Our cloud model is simple, fast and accurate and can easily be used to produce jacobians. The PCRTM/MRO code has been extensively validated against LBLRTM/DISORT; this paper shows good agreement between PCRTM/MRO and SARTA/TwoSlab. Figure 3 shows that when both SARTA and PCRTM use TwoSlab clouds (ie same cloud representation but different scattering algorithms), the biases are on the order of 0.5 K and the standard deviations are on the order of 1.5 K.

Also noted in the paper is that in the thermal infrared there are very few degrees of freedom for clouds. Coupled with the variety and complexity of cloud representations, even if the noise level used in this paper are adjusted, they will not adversely change the essence of the results.

25) Page 20, line 21: What do you mean with logarithmic multiplier for ozone? What is the unit of the cloud amount?

If you take the multiplier for the original profile as unity, and you add/subtract perturbations that come out of the OEM formalism, you run into the danger of accumulated subtractions for the profile multiplier becoming less than zero, hence giving unphysical profiles. If instead you use a logarithmic multiplier, then even if the OEM method gives negative deltas, you have to take the exponential before applying the multiplier, which will always be larger than zero.

In this paper we use integrated cloud loadings (g/m2), as noted in Section 3.1

26) Page 20, line 25: Please, correct Tikhonov.

Fixed

27) Page 20, line 29: How the 10 % cloud amount uncertainty has been chosen?

We base this on looking at the final retrieved cloud amounts versus the cloud amounts we started with (after the cloud swap/initialization). As noted in the manuscript this is a proof-of-concept; in the future we will probably allow for the adjustment of cloud particle size and cloud top as well.

28) Page 21, line 3: Correct physically-based

Fixed

29) Page 21, line 5: Appendix II instead of 10

Fixed

30) Page 21, line 9: remove file.

Fixed

31) Page 21, line 26: the value of the surface temperature uncertainty is not consistent with line 28 of page 20.

Thanks for pointing this typo. Fixed.

32) Page 24, line 13: Please explain what is "final UMBC" in the legend of Figure 11?

That was a mistake, should be "final retrieval"; now fixed (in Figs 11,13,14)

33) Page 27, line 4: the DOFS and FOVs numbers are not consistent with those of the figures 13 and 14 labels. Please clarify.

We mistakenly bracketed the DOFs spanning regimes for the plots and the text differently; we have now fixed the text to agree with the figures.

34) Page 27, line 7: I do not see on the figure where is the little difference between the retrieved profile temperature and the a-priori. If it was the case then the red full line would be close to the 0 line ?

We have rephrased the sentence (Page 27, lines 5-10) to read "The low DOF case shows a smaller difference between retrieved profile temperature and *a-priori* in the lower troposphere, compared to the free and upper troposphere."

35) Page 29: please indicates the units of relative humidity both in the figure and in the label

Fixed

36) Page 30, line 1: please correct deg

Fixed

37) Page 30, line 20: this paragraph is difficult to understand since there is no figure to help to reader. Is this a general feature of the retrieval or is this a feature of the track B ?

This is a general feature of the retrieval for this granule. Basically NWP models underestimate deep convective cloud tops. So in order to produce calculations which are cold, our cloud initialization needs to match colder observations with cloud fields that have a lot of ice cloud associated with them; increasing the ice cloud fraction means the observed emission is now better localized as coming from these cold cloud tops.

38) Page 30, line 30: This result is very interesting and I suggest the author to compare this results with other works (for example the work of Heymsfield, A.J., S. Matrosov, and B. Baum, 2003: Ice Water PathOptical Depth Relationships for Cirrus and Deep Strati- form Ice Cloud Layers. J. Appl. Meteor., 42, 13691390, https://doi.org/10.1175/1520- 0450(2003)

Thanks for pointing out this paper to us. We seem to be within a factor of two of the OD:IWP ratio in the paper you mentioned. This could be explained by differences in the cirrus habit scattering parameters used by them versus those used by us. In addition the amount of cloud (or aerosol) loading required to minimize infrared biases depend sensitively on height. Exploring this further is outside the intent of the paper, but we will certainly keep this in mind for future studies when for example we could include more parameters in the state vector (such as including ice cloud effective particle size and cloud top height in the retrievals) in order to have the best possible estimate of cloud loading.