Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-256-RC1, 2020

Atmospheric Measurement Techniques Discussions

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Interactive comment on "Cirrus cloud shape detection by tomographic extinction retrievals from infrared limb emission sounder measurements" *by* Jörn Ungermann et al.

Anonymous Referee #1

Received and published: 4 August 2020

General comments

The authors present a new method, the *convex hull cloud index*, to identify clouds in atmospheric limb measurements. As compared to the conventional cloud index method, the new algorithm shows significantly improved performance when applied to the high density measurements of a new imaging Middle InfraRed FTS limb-sounder (IRLS) that was proposed as a future Earth Explorer mission. The authors also present a second, more sophisticated and accurate, cloud detection scheme based on the 2D tomographic retrieval of cloud extinction, from limb-sounding measurements. The performance of this latter method is impressive when retrieving cloud geometrical exten-

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sion and position from simulated measurements of the planned IRLS instrument. The efficiency of the method is also tested and validated on the basis of real measurements acquired by the GLORIA instrument during an aircraft campaign.

The subject of the paper is very interesting for the remote sensing scientific community and is within the scope of AMT. The methods used are sound. Apart from Section 5, which is a bit fuzzy and, to my opinion, would also require a language revision, the rest of the paper is clearly written and sufficiently concise.

I have only one main concern that, probably, can be solved by including a minimal discussion. To generate IRLS synthetic measurements, the authors assume a FTS spectral resolution 1/(2 MOPD)=1.25 cm^{-1}, that is more than a factor of 6 worse than the required value for this instrument to study atmospheric chemistry. Such a strong reduction of the spectral resolution makes it possible to implement a very fine spatial sampling step, as the time interval required for the individual measurements is reduced accordingly. This instrument setup is very favourable to study clouds which generally have broad spectral features and are scattered in space. On the other hand, most likely, measurements made in this configuration will not allow retrieving gas profiles with sufficient accuracy. Therefore, I feel that this instrument operating mode in practice could be commanded only rarely. The measurements the authors use for their 2D tomographic retrieval of the cloud extinction seem limited to the spectral radiances integrated in two narrow spectral intervals. Probably, these measurements could be acquired with an instrument much cheaper than the considered IRLS interferometer. Maybe, a 2 channels calibrated imager pointing at the limb could be sufficient?

In conclusion, I recommend to publish this paper in AMT, however the authors should at least include a minimal discussion regarding the above issue, and clarify / correct the text according to the specific and minor comments included below.

Specific comments

Lines 34–36: please note that the tomographic 2D retrieval for satellite limb measurements dates back to Carlotti et al. 2001 (I suggest to cite this paper also here, just before Steck et al. 2005). Due to technology limitations, MIPAS/ENVISAT was not an imaging interferometer. This implied that the sampling strategy had to give priority to vertical coverage and vertical sampling step rather than to the horizontal sampling. Despite of that, MIPAS limb measurements had overlapping line-of-sights, especially in the measurements acquired after 2004. Due to this characteristics of MIPAS measurements, a database of Level 2 products obtained with the 2D tomographic approach was established: see Carlotti et al. 2006 and Dinelli et al. 2010. Even if now you are getting much better results due to the improved characteristics of the planned (IRLS) and available (GLORIA) measurements, I suggest to give proper recognition also to the above mentioned work and to correct the statement at line 36, which is not true (e.g. the special MIPAS observation mode S6 was specifically designed for tomographic retrievals, see http://eodg.atm.ox.ac.uk/MIPAS/frmodes.html).

Line 75 “to point its detector...”: I guess it is the “line of sight” which is adjusted, not the detector itself.

Line 86, “15 horizontal measurement tracks covering 7○...”: do you mean across track measurements with line-of-sight spanning an angle of 7 degrees in azimuth?

Lines 85–88: see my main comment above. The configuration you assume (spectral sampling = 1.25 cm^{-1} and fine spatial coverage) is certainly optimal to investigate cloud propertied which generally have broad spectral features. However, the IRLS mission had additional science objectives (gases retrieval) which, probably, cannot be

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met if such a low spectral resolution is commanded. So, although feasible, I imagine this operation mode can be used only for very limited time periods. Could-you please comment / address this point?

Line 143: I believe CI is the ratio between the integrated spectral radiance in two different spectral intervals, not the “ratio between spectral regions” as mentioned here.

Lines 178–180: same comment as above (regarding lines 85–88). Please note that for CI analyses, a much cheaper instrument such as a two channels calibrated imager could be sufficient. If the IRLS instrument will be implemented on a real satellite, you may hardly ask to operate it with so low spectral resolution and fine spatial sampling just to perform the CI analysis, if the spectral resolution is degraded, a lot of information on gases spectral lines is lost.

Line 213: the computing power of current PCs varies by orders of magnitude, please give the main technical details of the platform in which you tested the algorithm.

Line 252: do you also add pseudo – random noise to the synthetic measurements? Does the measurement noise have an impact on the results presented?

Line 228: why do you retrieve the temperature profile? The ECMWF estimate is not sufficiently accurate for your purposes? For each sweep of the limb scan, the measurements included in the inversion seem limited to the two values of the integrated radiance in the two spectral intervals (centered at 796.25 cm^{-1} and 835.00 cm^{-1}). How the number of observations compares to the number of retrieved parameters? My concern is that temperature and extinction could be very correlated in your inversion, thus in real retrievals, forward model errors (like interference errors or model errors due to neglecting scattering) could impact strongly on the retrieved extinction, that is the target of interest to you.

Lines 238 and ff: please explain the meaning of all the symbols appearing in Eq. 1. The Levenberg–Marquardt damping, the Jacobian F', and the iteration index i are not defined.

Line 238: you assume 0.1% error on the integrated radiances. Is this error figure compliant with MIPAS / IRLS NESR specifications?

Lines 242 and 243: regarding the constraint applied to extinction, it is not clear whether you constrain only the first derivative or also the actual retrieved values (usually, this would not be called regularization). Which first derivative do you constrain? The horizontals, the vertical, or all?

Line 249: presumably here you refer to the amount of RAM required. I would use the more common unit of Megabyte (MB) instead of the Mebibyte (MiB). By the way, the difference between the two is only 4.9%, thus not really relevant.

Lines 256–257: not obvious to me, I recommend to include at least a “minimal” explanation.

Line 273: did-you mean “a coarser horizontal retrieval grid”?

Figures 2 to 6: could you specify somewhere, where is the satellite instrument located in these figures? I guess the satellite is on the left, given the difficulty to detect exactly the Cloud Top on the right side of the more opaque clouds.

Figures 2 to 7: the top title is not very informative for the readers, it could be removed.

Lines 338–339: in panel (e) of Figure 9, I would have expected to see the retrieved extinctions, however the text is not very explicit on this point. Moreover, the caption of Figure 9 states that panel (e) shows the extinction values “used for generating the simulated measurements”. This is confusing, could-you please clarify what you are really showing?

Lines 341–347: this is strange again. From the left plot of panel (d) on Fig. 9, I see that the CI determined on the basis of the convex hull, results with cloudy pixels at 5 km at latitudes of 45–50 degrees North. However, looking at panel (b) of Fig. 10, it does not seem that the cloud system extends down to 5 km. Again, here I don’t understand why in Fig. 10b you don’t plot the 0.001 km^{-1} contour surface of the retrieved extinction, which should give the best performance. Did I misunderstand something? Please clarify.

Lines 359–362: why do you change the retrieval approach in the case of GLORIA measurements? Specifically, why the ECMWF temperature is considered sufficiently accurate for this case and not enough accurate in the test simulations presented earlier? I agree that if you do not retrieve temperature, the microwindow centered about 796.25 cm^{-1} is no longer required, however, you also change the second microwindow that in earlier tests was centered about 835.00 cm^{-1}. Why? Please justify. Please, justify also the choice of ignoring gases concentrations in this retrieval.

Lines 364–365: It seems that two cameras were operating together with GLORIA in this flight. Please introduce earlier all the instruments used and define also the main features of the cameras, e.g. which is their spectral band-pass?

Lines 365–368: not clear at all. Maybe it will be more clear when the operation mode of the two cameras will be better explained.

Line 374: the inversion problem is said to be “ill-conditioned” (and this is especially true because relatively few measurements were used in the inversion, as compared to the amount of retrieval parameters).

Line 379: please specify how you define the “well resolved region”. How many degrees of freedom (DOF) do you have, for each retrieval grid point, in the well resolved region?

Figure 11: please add labels in the horizontal and vertical axes. In the figure caption, I guess you meant “visible” in place of “visual”. The top title does not provide useful information to the reader.

Line 383: which instrument measured the brightness temperatures reported in Fig. 12? GLORIA? To which wavenumber or spectral band do the reported brightness temperatures refer?

Lines 384–393: the retrieval or inversion of measurements is the “rigorous” approach while, I guess, this is a qualitative interpretation of auxiliary / additional measurements that were not adequately introduced.

Lines 395–400: I think a Figure with some maps reporting the number of DOFs per retrieval grid point at some fixed altitudes would be more illustrative and clear than this qualitative explanation.

Line 442: extinction is in km^{-1}. From Fig. 13, the extinction of cloud B seems of the order of 0.01 km^{-1}. Please explain how you infer the extinction of 0.1 km^{-1}.

Minor comments

Figure 1 caption: profile / profiles ==> limb scan(s)

Lines 236–237: all symbols of the equation are either vectors or matrices, therefore I would not use italic fonts. Unlike the others, the last “F” is italic.

Line 233: for “F” I would use bold, roman fonts everywhere.

Line 267: “size”.

Line 278: “finer spatial sampling”

Line 282: next ==> nearly

Line 332: structures

Line 387: “shrunken” ??

Line 485: “please note that”

References

Massimo Carlotti, Gabriele Brizzi, Enzo Papandrea, Marco Prevedelli, Marco Ridolfi, Bianca Maria Dinelli, and Luca Magnani, “GMTR: Two-dimensional geo-fit multitarget retrieval model for Michelson Interferometer for Passive Atmospheric Sounding/Environmental Satellite observations,” Appl. Opt. 45, 716-727 (2006).

Dinelli, B. M., Arnone, E., Brizzi, G., Carlotti, M., Castelli, E., Magnani, L., Papandrea, E., Prevedelli, M., and Ridolfi, M.: The MIPAS2D database of MIPAS/ENVISAT measurements retrieved with a multi-target 2-dimensional tomographic approach, Atmos. Meas. Tech., 3, 355–374, https://doi.org/10.5194/amt-3-355-2010, 2010.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-256, 2020.