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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of microphysical cloud parameters from EM-FTIR spectra measured in Arctic summer 2017" *by* Philipp Richter et al.

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This paper presents an algorithm that allows to retrieve the properties of liquid clouds as well as ice clouds from high spectral resolution measurements in the mid-infrared range from the ground. The results of the algorithm are compared with synthetic measurements to test the performance of the algorithm. The algorithm is then applied to real observations from a measurement campaign that took place on a ship around Svalbard during the summer 2017. The results of the algorithm are compared with other algorithms that reproduce the same cloud properties for validation purposes.

First of all, I was very enthusiastic about the idea of evaluating this paper, because the subject is of undeniable interest to the community, especially because this type of

measurement is not often used to retrieve cloud properties (especially in the ice phase), but also because it could provide fundamental information on cloud microphysics and in particular the capabilities of current models to simulate a spectrally resolved measurement over a wide spectral range.

Generally speaking, the paper disappointed me both in the presentation of the results, where the lack of analysis, especially concerning the limits of the algorithm, is blatant, and in the scientific conclusions, which appear hasty and without concern for physical explanations. A number of passages may even be confusing and lead the reader to believe that the algorithm could realistically deal with ice cloud properties. I am thinking in particular of the part on the microphysics of ice clouds, and in particular the models of Yang et al. which, as the reader will discover much later, are not at all processed by the algorithm, which assumes a spherical shape for these particles.

The presentation of mathematical tools such as the averaging kernel, which seems to be very interesting for a posteriori analysis of the results and which are presented as such by the author, are absolutely not used. In short, the study looks confusing and not very informative. Moreover this study does not seem to bring anything new compared to other existing algorithms cited by the author or even used to validate the algorithm presented in this paper. The author absolutely does not allow the reader to get an idea of what the algorithm brings in comparison with CLARRA for example.

This study as presented does not hint the scientific level of the journal, the authors must moreover clearly demonstrate the novelty of their algorithm in relation to what is already done.

For all these reasons this study cannot be published in its present form.

Below are indicated in a more precise way all the remarks concerning the manuscript, which if followed could improve the paper.

Line 5: Could you give an explanation on why not using wavenumber smaller than 600

cm-1?

Line 27: In the sentence - The blackbody limit . . . - rephrase it, the sentence is not clear.

Line 32: performing

Line 50: the sentence is not well written you should change it with - because of low absorption by molecules the atmosphere is transparent and allows to see the signal emitted by clouds -.

Line 51: What do you mean by total effective radius? Is it the average or the sum? Please be more precise.

Line 98: - . . . new software. . .-, here it is important to tell the reader what is new in your algorithm compare to MIXCRA and more specifically compare to CLARRA.

Line 107: You really don't need to get this kind of technical details, moreover I am not sure about this explanation, the streams are used to integrate the scattering source function from scattering by any hemispherical direction in the direction of observation, you don't solve 16 different differential equation, please reformulate.

Line 111: could you please tell the reader which parameters are you talking on.

Line 114 to 116: The sentence - Because single . . . were used- is incomprehensible, please reformulate.

Line 117: -. . . they have large uncertainties . . .- what are you talking about? Indices, scattering parameters? Which parameters?

Line 118: My first thought was which shape are they using in their retrieval? But I understood later that you were not using these microphysical models, so why are you talking about it? Unless you want to specify here that you are not using it, but you may include these model in a futur algorithm, otherwise remove it because it is confusing we might think that your algorithm can undue such shape complexity.

Line 121: -. . . modified-, What is modified compare to Rowe et al.???

Equation 3: miss the n in Kn transpose.

Line 131: Could you specify how the Jacobian is computed in your algorithm?

Line 132: Could you tell a bit more on the variance of the measurement? How do you compute it, is it something furnish by the constructor of your instrument, please give some numbers!

Line 132: Saˆ-1 is not an error but a variance-covariance matrix. How did you built it? Give some numbers.

140: Why the averaging kernel is an important quantity to characterize the retrieval quality?

Equation 8, 9, 10, 11: Please explain why you need an iterative procedure to compute the averaging kernel? Why not computing it once your algorithm converged without any iteration process beside the retrieval itself?

Line 153: How do you find the best tau_cw? Are you using LUT and find tau_cw from a simple least square method? Be more precise on this point!

Equation 13: This equation is not appropriate, you miss the particle number concentration N0, you should rewrite it by introducing a normalize size distribution and make explicitly appear N0, like: IWP=rho_ice*N0*Vmean*tau_ice/sig_ice

where sig_ice is the ice extinction (m-1) Vmean is the mean volume computed over the normalized size distribution...

Line 162: V0 is not the volume of an ice droplet but the mean volume computed over the normalized size distribution. Same for ext, it should be the extinction coefficient of ice (mˆ-1). . .

Line 164: What is - cVo - ? Same for Cext? Please define all the variables you are

using in your equation!

Line 180: This sentence is very confusing - Here the maximum ... - how can you use the residual to define Sy. The residual will be known at the end of the iteration process and needs the Sy matrix in the iteration process! Sy should be defined before and should be provided posterior to the retrieval, by the instrument manufactor, which provide NeSR and absolute accuracy of the instrument!

Line 191: Again why not using the Non-spherical model here? Or at least use it to define a forward model error due to the simplest assumption of spherical ice particles.

Line 193: How did you choose this value of 0.2 (mW/str/m2/cm-1)? Please give some justification.

Line 194: How did you disturb the Temperature, is it like a white noise, do you use any gaussian statistics?

Line 195: What do you mean by a radiance offset, is it like a bias? If so why did you choose this value, is it related to the instrument you are using? If yes give some justification/reference!

Line 202,203: Please describe this instrument (Vaisala CL51), and tell the reader why you can take the cloud top height (CTH) from this instrument and give some accuracy to this value.

Line 206: What are the errors introduce by the assumption of spherical ice droplets?

Section 4.3: Why do you present the results here when there is a specific section on the subject afterwards (section 5)?

Section 4.4: It seems from this section that your algorithm is identical to the CLARRA one. You really need to tell the reader the main difference and how they can complement each other!

Line 235: what do you mean by - Incorporated - ?

Line 239: The sentence is not clear, please rephrase it!

Line 248: - tau_cw > 6 are ... available = It is not clear what you want to say here! Rephrase it.

Line 248: - Also the spectra ... - From which spectra are you talking about? This something that you say but don't show... A figure would has help to proof it.

Line 250: - The condensed water path can retrieved - strange! - can be retrieved - I guess - without the exact knowledge of ice ... - What do you mean? Is it always the case? Or only when the ice fraction is low compare to the liquid one? Please be more precise!

Line 249: This value of 20 microns is very subjective, how can we state that this value is an indicator of the accuracy of the retrieval!! you have presented some mathematical tools ,like the Averaging kernel or a-posteriori errors that can be used as an indicator for bad or good retrievals, why not using them?

Line 258: - However ...- Why did you present the Ping Yang model then? One could think that this retrieval algorithm might include some non-spherical particles which would have been very interesting, because it is well known that ice particle are non-spherical. So I am wondering what this algorithm is bringing to the community, The method (optimal estimation) use to make these restitutions is sophisticated enough to integrate the errors of the model in order to propagate them on the retrieved parameters, from the definition of the forward model variance-covariance matrix. Here the author could have used this formalism to take into account the error linked to the assumption of using spherical particles for ice for exemple ...

Line 264: What is this factor (slope or correlation)?

Section 5.2: Why The posteriori errors attached to each parameter are not given? It would have been interesting to add them in order to be able to evaluate the accuracy of each algorithm.

Line 302: This sentence is not clear, the points are always presented in the center of the error bar with a length equal to a factor times the standard deviation, therefore the points can't be outside of the standard deviation. . .

Line 302 to 307: This part is not clear we don't understand what you are trying to explain. . .

Line 327.328,329: Instead of saying this kind of evidence, it would have been interesting to give some information on the accuracy that the instrument should have reach in order to get accurate enough retrieved information of cloud.

Line 338: If cloudnet is the reference you should have said the results from TCWret show some overestimation of . . .

Line 339, 340: - However, retrieval of microphysical cloud parameters . . . - one could have wait something a bit more precise, which parameter is well retrieved ans which one not, why not using the averaging-kernel to give some indication. The a posteriori error analysis is completely non-existent, it could have highlighted the limitations of the algorithm by correlating, for example, to the thermodynamic conditions or to the cloud column (presence of several cloud layers, high humidity, cloud layer close to the ground, fractional coverage).... which strongly not serves this study.

Section 7: This section is not necessary and does not add anything to the paper, the conclusion is already in the previous section.

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