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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.

Anonymous Referee #3

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This paper presents an algorithm used to derive cloud properties (total optical depth, effective radius, and condensed liquid water) from a ground-based infrared interferometer. Results from the algorithm are compared against synthetic radiances, where the true cloud properties are known, assuming bias errors in the instrument / assumptions to evaluate the impact on the retrievals. It is then applied to downwelling radiance data observed during a multi-month summertime field campaign, with the results compared against a similar infrared retrieval algorithm and against an active+passive method (CloudNet). The results show high correlation between the retrieved properties using this new method, and those from the other methods.

My primary question is what is the new contribution of this work? The authors have stated that this algorithm is very similar to the MIXCRA and CLARRA algorithms, and indeed results from the latter are used to evaluate the results from this "new" algorithm. The authors need to state clearly how this algorithm differs, and ideally is an improvement, upon the previous two algorithms. As it is written, I do not think this paper merits being published until this question is answered.

The rest of my comments are much easier to address, and are offered to help the next version of the paper.

L22: your "in general" statement does a great disservice to a lot of previous research that has demonstrated that clouds have a cooling effect in tropical to mid-latitude locations, and a warming effect for many months in the polar regions (esp when the sun is below the horizon). This statement needs to be removed or totally rewritten

L 26: the downwelling LW radiative flux at the surface becomes less sensitive to changes in the LWP when the LWP > 40 g/m2 (by that Turner et al 2007 reference). But the LW radiative heating rate (and the SW radiative heating rate) profiles are sensitive to changes in LWP for LWP values that go much higher than this threshold (e.g., Turner et al. JAMC 2018)

L27: the sentence that starts "The blackbody-limit for. . ." is unclear – I don't understand the point you are trying to make

Multiple misspelled words throughout the document, e.g., on L33, L35, L53, L73, L187, L236, ... Please check entire document more carefully

Please add more references: e.g., to ARM on L44, AERI on L45, the other instruments on L90-L91

Is there a paper that describes your IR interferometer in detail? If not, then I believe that substantially more detail is needed here (but perhaps put in an appendix as to not disrupt the flow of the paper too much)

L73: this equation is a complex number, as the interferogram is almost certainly not symmetric. The Revercomb method shows that, if the instrument is well-behaved, the imaginary part is zero (with noise) and the radiance is the real part of this equation. This should be indicated here.

L79: This assumes that the blackbodies have an emissivity of 1.0. That is almost certainly not true, and even slight deviations of the emissivity from unity can affect the calibration. See Knuteson et al. JTECH 2004 part 2 on the AERI to see how they handle non-unity blackbody emissivity

L131: how do you determine Xa and Sa? This information is critical for understanding this retrieval.

L153: Why is the iterative method (i.e., starting with tau_total=tau_ice+tau_liq with tau_ice=tau_liq, getting a solution, and then individually retrieving tau_ice and tau_liq simultaneously) required if in the end you only really desire tau_total?

L157: Your algorithm retrieves four parameters, from which you derive three. Why not evaluate the 4 parameters you retrieve?

In same vein: your algorithm will provide uncertainties for these four parameters. Your paper would be stronger if you propagated those errors into your three derived parameters, and show the sensitivity of those uncertainties in tau_total, r_total, and CWP to the uncertainties of the four (tau_liq, tau_ice, r_liq, r_ice). This will come up again in L273 (see my question on that line below)

L205-208: where did these percentages (e.g., 98.50, 95.35, 98.28, etc) come from? For example, are you confident that of all of the clouds seen during the summer cruises that 98.50% of them had tau_total less than 6? If you only used your TCWret to determine the total cloud optical depth, then this is likely due to the sensitivity limit of the algorithm/data; does the CloudNet retrievals show the same fraction of cases with tau < 6?

L220: It seems that you have observations from your spectrometer in microwindows below 600 cm-1 (which is why we need more information on your instrument – see my comment above), but you chose not to use them in TCWret – Why not? Or is this paragraph only talking about the synthetic observations created by Cox that you used to evaluate your method? This paragraph is very unclear.

L273: does the fact that the CWP does not show the same dependence as tau_total imply that there are compensating errors (e.g., in r_total)?

L297: the instruments also have markedly different field-of-views

L306: If the assumed cloud temperature is too warm, then the retrieved optical depth will be too small – it is the only way to match the observed radiance. So the logic here seems to be backwards: using the cloud base height from the ceilometer would result in the cloud being too warm (relative to if the entire profile from a cloud radar was used to determine the cloud's temperature), and thus the TCWret's value should be LOWER than reality – but is not what is shown in Fig 11. This requires some more investigation / explanation, and will be important for a future submission (which I hope you will do).

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