Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-216-RC1, 2019
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Atmospheric Measurement Techniques Discussions

Interactive comment on "Toward autonomous surface-based infrared remote sensing of polar clouds: Retrievals of cloud microphysical properties" *by* Penny M. Rowe et al.

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Received and published: 9 July 2019

This paper describes a cloud retrieval algorithm (CLARRA) that is applied to synthetically generated radiance data to provide a dataset of several hundred clouds (single phase ice, single phase liquid, mixed-phase, etc). The objective of this work, as stated in the abstract, is to show how the bias in the retrieved cloud properties changes as the spectral resolution of the ground-based instrument used to provide the radiance observations becomes coarser. The overall goal is to show that instruments with relatively coarse spectral resolution (e.g., 8 cm-1) yield more-or-less similar results as a high spectral resolution (e.g., 0.1 cm-1) instrument.

I have a multitude of concerns with this work, and believe that the paper should not be published in its current form.

My primary one is that the "new" CLARRA algorithm is almost exactly the same as the so-called "MIXCRA" algorithm of Turner (J Appl Meteor, 2005). In fact, it is almost like the authors were trying to disguise this as there is not a single reference to the MIXCRA paper in sections 1, 2, 3.1, or 3.2; yet it is clear the authors know about MIXCRA because it is referenced at the top of section 3.3. It is not clear what makes CLARRA different from MIXCRA.

My second primary concern is simply: Is this paper really adding any new knowledge? I presume that they are using observations in spectral regions that are between absorption lines (these are often called "microwindows"), and the cloud properties in these microwindows are essentially unchanged. This allows the radiance to be averaged from higher spectral resolution to that of the width of the microwindow; this was done in the MIXCRA paper (see table 1 in that paper). Indeed, the microwindows used in the MIXCRA paper are between 2 and 8 cm-1 wide, depending on the spectral region. So while the MIXCRA paper didn't specifically test retrievals performed at 0.5 vs. 4 cm-1, it already is showing the impact. [As a side note: the authors really do need to include a table on what microwindows are being used in this study. In addition to the actual microwindows, if the microwindow is actually only 4 cm-1 wide, do the authors limit their spectral width of that "channel" to only or do they include the absorption lines that exist on the sides of these microwindows? If they are testing 0.1 cm-1 resolution, do they allow multiple "channels" within a given microwindow, or only a single channel? Without this information, the results here cannot be reproduced.]

The uncertainties assumed for temperature and humidity profiles in the reanalysis (around line 355) are shockingly small. These uncertainties might be true for a large average, but in a scene-by-scene way the errors will be much, much larger. For example, if the reanalysis believes that the sky is cloud-free above the instrument, the reanalysis may have developed a surface-based inversion that would not be there in reality because of the cloud. An example of how the presence of a cloud modifies the temperature profile beneath the cloud is given by Miller et al. JGR 2013 (which includes Walden as a coauthor). Because clouds are so hard to represent properly in large-scale models, I think the authors need to use more representative uncertainties (e.g., many degrees for temperature, and at least 20% for water vapor) for the temperature and humidity profiles, and show the impact of these uncertainties.

The authors are using the far-infrared for several of their channels; indeed, using those channels are really important for discriminating the cloud phase (see Turner et al. JAM 2003 as well as Turner JAM 2005). However, water vapor absorbs strongly in the far-infrared, and if the PWV is large enough the window will be opaque purely due to water vapor absorption. Thus, the range of cloud optical depth that can be sensed depends strongly on the PWV; this needs to be discussed in this paper.

I think that the authors have missed a real opportunity to talk about how the uncertainties in the retrieved products covaries (i.e., by looking at the off-diagonal elements of the posterior covariance matrix). This was one of the shortcomings of the MIXCRA paper, and expansion of that here would add some new insights to the community. For example, as the cloud emissivity moves towards unity, there will likely be a high amount of correlated error between Reff,ice and Reff,liq. Ditto when the cloud emissivity is small. How does the ice fraction uncertainty covary with the other retrieved variables, especially in different areas of the solution space?

A different question along the same lines as above is this: does the accuracy and covariance between the cloud properties change if the retrieval is configured to retrieve (total_tau, ice_fraction, Reff_ice, Reff_liq) vs. (tau_liq, tau_ice, Reff_ice, Reff_liq)? The MIXCRA algorithm was initially the first (in JAM 2005), but was changed to the latter (Turner and Eloranta TGRS 2008) and showed pretty good results relative to the HRSL during MPACE.

The authors really didn't spend any time discussing the different technologies that could

be used to provide these radiance observations, or why they would be "cheaper" from the instrument that they assumed (which seems to be the AERI). Radiometers using a finite number of channels with bolometers as detectors are one possibility; there are many papers by the so-called "TICFIRE" project being run out of the Canadian Space Agency that might be useful. But how important is the calibration of cheaper systems like this? The authors did show results if the radiance bias was 0.2 RU, but that is a pretty small error – a more realistic error might be 1 or 2 RU. Do these results scale? [For example, even with a carefully calibrated AERI, radiance biases close to 1 RU have been reported – see Delamere et al. JGR 2010. It is hard to imagine that a cheaper radiometer would have better spectral calibration than the AERI.]

Some more minor points: âĂć How many streams are being used in DISORT? Fewer streams make the RT code faster, but will decrease the accuracy. âĂć Eq 18 is incorrect if gamma > 0. The correct formulation was originally provided by Masiello et al. QJRMS 2012, but was also presented in Turner and Löhnert JAMC 2014. âĂć "the ideal range for tau is between 0.4 and 5" (line 425). The authors really should indicate how often this is expected to happen in the Arctic. The Cox et al. JAMC 2014 paper provides some information on this, at least for that site. âĂć Is there a minimum number of spectral microwindows that need to be used? Stated a different way, how do the errors in the retrieved cloud properties change for different microwindow subsets?

I would be very happy to talk with the authors about this review and my thoughts to how this paper could be improved, should they want to do so.

Sincerely, Dave Turner

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-216, 2019.