

Review of « Measurement Report: Differences in cloud optical and microphysical properties in the Arctic and Antarctic derived using thermal infrared spectroscopy », by Joseph Hung et al. Review by Quentin Libois.

General comments

This paper presents datasets of cloud optical and microphysical properties derived from remote sensing observations from the ground (mostly AERI and lidar) at two polar sites, namely Eureka (Nunavut) and McMurdo station (Antarctica). At Eureka the measurements cover the period 2006-2022 while at McMurdo it mostly consists of 2016. Using a similar retrieval algorithm (CLARRA) to estimate cloud optical depth, phase partition, and liquid and ice effective radii, the variability, average values and seasonal cycles of these properties are analyzed and compared for both locations. Beyond factual results, the paper discusses the differences obtained, pointing to distinct physical processes, atmospheric conditions, but also potential observation biases.

The paper presents an original dataset of cloud observations, certainly unique for such remote locations. It is overall well written, although a better distinction between the factual results and the interpretations would probably help the reading. More generally, reordering may improve the quality at many places. This paper is meant to be a “Measurement Report”, hence does not have the physical insight of a Research Article, which at some occasions is a bit disappointing. Yet the presented dataset can certainly be very valuable for the wider scientific community, and probably. I think the paper fits within ACP scope, and could be published after the points below have been addressed.

Specific comments

1) As the main material of the paper is the datasets, it would be worth better demonstrating that they have never been published before. In practice, several papers have used the same observations (in particular at Eureka), so this needs clarification, and the references to previous work should be more exhaustive.

2) The introduction would gain being reordered so that the transitions across the successive paragraphs would be more relevant. At present the reasoning is difficult to follow. See technical details for some suggestions.

3) It is mentioned that sampling issues (in particular many observations are discarded), due to the limitations of the retrieval algorithm or issues with the instruments, can be critical. Yet the impact on the comparisons performed and conclusions drawn is not sufficiently discussed. Indeed, the differences reported may to some extent be largely a consequence of sampling differences between both locations. More generally, the uncertainties of the retrievals and the impact on the interpretations, would deserve more attention.

4) Often, the physical interpretations provided in Section 3 remain superficial. It might be more appropriate to distinguish the Results section from a dedicated Discussion section. The latter might be less elaborated than in a Research Article, but merging the various physical interpretations spread across the current section 3 would certainly strengthen this aspect, and avoid redundancies. Currently I find this section 3 hard to follow, with many points that would better fit in the introduction. Some reordering may help the readers. Many interpretations consist more of general statements regarding cloud microphysical processes than thorough analysis.

5) The partition between ice and liquid, given in this paper in terms of an optical partition (contribution to the total optical depth), can be hard to use for modelers who handle actual liquid

and water contents. It would be helpful to better emphasize how despite this radiative definition the liquid/ice partition can still be valuable for people interested in the mass partition. Likewise, there is no mention of liquid or ice water paths, while these are properties commonly simulated and evaluated in atmospheric models. Addressing these points may help the paper to reach a wider community, beyond the remote sensing one.

6) Often, the Figures are not sufficiently well introduced, so that they are not fully highlighted. The reader needs to go the caption to really understand what they show, while one would expect to have some high level details directly in the main text. As an illustration, the Figures are mostly referred to at the end of the sentences, in parentheses, rather than stating “Figure XX shows ...”.

Technical comments

1.15: “microphysics” is unclear. Do you mean microphysical processes or properties?

1.19: having → “, that have ..., are experiencing ...”

1.22: why “high albedo snow/ice surfaces” makes it difficult to be simulated?

1.29: typo with the parenthesis, extra “;”

1.30: microphysical interactions → microphysics or microphysical processes (?)

1.36: “tends to interact more strongly” can be misleading as the difference mainly comes from differences in the size of the particles (droplets being smaller than ice crystals in average), rather than intrinsic differences between liquid water and ice optical properties

1.48: “differences in cloud feedback strength” suggests that the differences especially come from the representation of ice clouds. Is it the case? Please clarify

1.49: the link between this paragraph (very descriptive, focusing on the sites) and the previous one (describing cloud microphysical processes and radiative impacts) is not obvious. Consider improving the transitions, or even reordering some parts of the introduction to improve the flow

1.54: what is the altitude of “SAFIRE”? If it’s nearby OPAL please specify

1.61: Again, I find the transition to the FIR quite abrupt. Especially because the FIR can be either a “tool” to probe clouds, or a way to measure spectrally resolved cloud radiative impact. Hence “The far-infrared (FIR) is difficult to study” is unclear. Maybe say that cloud observations are needed, that IR observations are relevant, and the FIR has a great added value for that (if this is the point here).

1.65: “is responsive” is unclear → sensitive, bears the signature?

1.80: IR measurements are not well introduced. The idea should be to explain right here why IR has some advantages compared to other techniques. The introduction could more explicitly feature the common scheme “context, motivation, objective, approach”

1.83: “from downwelling thermal emission measurements” should not be the objective, but rather the strategy used to answer the objective, namely “obtain a long-term record of cloud microphysical and optical properties”. Unless the focus of the paper is on the retrieval algorithm.

1.84: “is highly complementary...” does not seem to be at the right place in the introduction. Can it be a perspective, or instead should it be in the previous paragraphs in the context, to specify why ground-based measurements are used in the present study, beyond existing airborne measurements?

1.87-88: “will be beneficial”, “will allow” → this should be in the perspectives rather than in the introduction

Figure 1: why showing other points than PEARL on A? On B specify which facility is shown.

1.90: “Previous work has suggested” → this should be in the context, not in the objectives

1.95: it may be more relevant to mention first the instruments and data, and then the retrieval technique

1.116: some information about the frequency of AERI measurements would be useful in this paragraph

1.122: is it useful to detail what products (if not only backscatter profile) of the HSRL are used?

1.128: maybe explicit that CLARRA only works for single layer clouds (if correct). What happens in the case of multi-layer?

1.130: is there any assumption in CLARRA regarding the vertical distribution of cloud mass or extinction? More generally, slightly more information about the assumptions made in CLARRA may be useful.

1.131: are there a single liquid and ice effective radius for the whole cloud?

1.134: “equivalent”

1.136: the database of Yang et al. (2013) contains many shapes. Specify which habit is used, and what assumptions are used regarding the size distributions

1.136-139: I think this sentence is hard to understand and should be clarified. I guess the point is the inconsistency between the shape assumed in CLARRA and the actual ones, such that a different r_{eff} may just mean a different ice habit (with actually similar r_{eff})?

1.151: are the profiles used as such, or included somehow in the state vector? As they may vary along the day. If the actual temperature is not well captured by the RS (which can be performed with some delay), what are the consequences on the retrieved properties?

1.153: remove water vapour (H_2O) (I assume)

1.165: what about the interpolation from Yang database to AERI resolution for cloud properties?

Figure 2: consider using a and b instead of A and B to label the figures (the same holds for Fig. 1). The labels in panel a are too small

Figure 3: CO_2 -slicing is not introduced. Does it give the upper boundary of the cloud?

1.174: with so many references, hard to know where the actually used database comes from

1.177: the bias issue is not clear. Did you test on simulated spectra where another shape is assumed for the forward computation?

1.178: “inhomogeneity” seems inappropriate here. Diversity?

1.183: “best-knowledge thermodynamic state” is not clear. Does it mean spectra taken close to the soundings?

1.203: total column PWV or below cloud?

Table 3: consider swapping McMurdo and Eureka, as in the text Eureka is generally mentioned first

1.216: the information regarding hatch closure is redundant, can be removed here

1.226: it is not clear if “cloud boundaries” in the paper refer to bottom and top or only bottom. Does CLARRA need both boundaries?

1.229: “may manifest in the retrievals as warmer clouds”. I don’t understand. If the cloud bottom is considered lower (hence warmer most of the time) it means the emission signal is attributed to temperature rather than to optical thickness, so I would imagine a compensation of biases in the retrievals, which might be critical

Fig. 4: is “Incidence” a standard name for the probability of occurrence?

1.240: why not showing in Figs. 4 and 5 the monthly mean ice fraction, which would help match the 0.1 and 0.6 values given here (which currently cannot really be seen in Fig. 5)

1.241: where can the reader see that mixed-phase clouds are present all year long? In Table 3 only?

1.241: shouldn’t 40% rather be 44% ?

1.243: not clear what “interaction” means here. Does it refer to the emission, to the extinction, to the size of the particles (hence optical depth)? Why not assuming a seasonal cycle in the total column of condensed water ? Do you have any information of these integrated quantities? Also, “cloud liquid” is awkward.

1.245: I fully agree with this warning, which questions the assertion just before, as ice fraction is not really an ice fraction...

1.254: this statement regarding mixed-phase clouds is redundant with 1.240. Consider merging the information

1.262: why is this observation counterintuitive?

1.269: “parameterization” is unclear, maybe clarify the meaning here

1.273: again, where do you read that there are no purely liquid clouds? Because the ice fraction is never 0? Because the mean occurrence is low? It deserves some clarification

1.275: the question of ice nucleation is indeed central, it deserves more discussion

1.277: which “bias” do you refer to?

1.281: is “feature” appropriate? “strongly dominate”?

1.284: does this “60%” value appear in one figure? If not, would it be useful to complete Fig. 7?

1.285: redundancy with 1.275

1.287: “adjacent” does not seem an appropriate term here

1.288-290: I don’t understand this sentence, and the conclusion drawn from the temperature similarity

1.292: I don’t understand this statement. Shouldn’t you look at the low tail of the mixed phase clouds to derive such a conclusion?

1.294: not sure it is needed to remind that, might be counterproductive

1.296: liquid effective radius actually seems to feature some seasonal cycle

1.297: you mention seasonal differences, but no seasonality, this is unclear

1.298: please explicit the expected variations in atmospheric conditions and aerosol load. Actually the explanation comes later, consider reversing the sentences or merging them to help the reader

1.303: it is not clear what would be an “interesting avenue”. Why mentioning this potential source of INP? Does it help interpreting the results?

1.308: it might be interesting to give some information regarding the differences of INP, beyond the references. Also, would it be more appropriate to mention that in the introduction rather than here?

1.310: the mention of ice number concentration comes without any quantitative information. It is not clear whether this sections tries to interpret the differences or presents general statements regarding ice clouds

1.314: why mentioning models here while the study focuses on observations?

1.316: this looks more like a motivation for the study, than a discussion of the results

1.317-318: not clear what the meaning of this sentence is. It deserves more explanation

1.322: and so, what are the consequences of removing a certain amount of observations due to this threshold?

1.325: is Fig. 9 mentioned before Fig. 8? If that is the case, figures probably need to be swapped. Also, just reading the text, we have no idea what Fig. 9 contains.

1.327: not clear what you mean by “may have been anomalous in some way”

Figure 8: would it be easier to see correlations if the colored points were shaded, so that a more intense color highlights a higher density of points? Currently dense regions do not pop out. The same holds for Fig. 6.

1.343: “due to the prevalence of thin ice clouds”. I don’t understand why this explains the absence of correlation with temperature

1.354: this conclusion is not straightforward. You may have liquid clouds at Eureka at low temperatures as well, but warmer clouds (not present at McMurdo) may drive the mean to higher values there

1.357: this is interesting, but does it contribute to explain the differences between McMurdo and Eureka?

1.363: The figure is well introduced, which is appreciated, but it has already been referred to earlier

Fig. 9: specify that the offset is 6 months, otherwise it’s hard to understand.

1.386: remove one “during”

1.404: what is specifically “unique” to Eureka?

1.419: it would be easier to compare the min or max, but not the min of McMurdo with the max of Eureka

1.425: could you elaborate on these implications as it seems to be an interesting result

1.433: the decadal time series is only for Eureka.

Figure B1: “Rayleigh”