Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-960-RC1, 2019

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Small-scale structure of thermodynamic phase in Arctic mixed-phase clouds observed by airborne remote sensing during a cold air outbreak and a warm air advection event" *by* Elena Ruiz-Donoso et al.

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Received and published: 9 November 2019

The authors provide a well-motivated study into the small-scale vertical and horizontal structure of cloud thermodynamic phase. The experiment is impressive in scope, including both passive (horizontal mapping) and active (vertical) airborne instruments, combined with additional large eddy simulations of vertical structure. The authors conclude that, given their data, "the cloud top small-scale horizontal variability reacts to changes in the vertical distribution of the cloud thermodynamic phase." This is an important topic for GCM parameterization. The manuscript is very clear and well-written,

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Discussion paper

though I have identified some potential weaknesses in both the methodology and scope that the authors might consider.

Overall: I am sympathetic to, and appreciative of, the authors' attention to fine-scale thermodynamic phase. However, such measurements may eventually require a more quantitative and comprehensive account for other potential confounding variance at scales of 100m or less. Taking this on would significantly improve the paper.

1. 2D photon transport effects in the cold outbreak case. As far as I can tell from the description, the authors' phase index retrieval method assumes RTMs of a homogeneous plane-parallel cloud surface; this is fine, except that they then apply that to interpret a heterogeneous cloud body where there is likely to be considerable horizontal photon transport due to scattering within the cloud itself. Horizontal transport implies the retrieval has a locale-specific geometric sensitivity "footprint" which I suspect is larger than the 10m spatial resolution of AISA. But the authors' maps seem to interpret every pixel at native resolution as if it were an effective discriminator of cloud phase at that location - hence the frequency distribution histograms of Fig. 5. Additionally, the exposure to incident sunlight and slant path through the cloud might vary since the cloud thickness and altitude are also varying on similar scales (i.e. "Domes" and "Holes" but also even finer-scale structure in the retrievals)

   While a full 3D simulation may be overkill, the authors would ideally find sufficient account for the cloud horizontal heterogeneity in their algorithm to avoid hallucinating compositional features which might, for example, be artifacts of self-shading effects or other horizontal heterogeneity. As a thought experiment, imagine where a reasonable estimate of horizontal sensitivity could be determined and the AISA data were convolved with a sensitivity kernel before applying the retrieval. If this caused the LWP retrieval to look more like the 100+ m rows of Figs. 10 and 11, how would it affect the authors' top-level claims and interpretations of fine-scale structure?

2. Accuracy of the slope phase index for characterizing mixed phase clouds. While $\mathcal{I}_s$ has been demonstrated as an effective way to discriminate pure clouds, it is not always clear how to interpret $\mathcal{I}_s$ for mixed phase clouds since, as the authors note, the vertical partitioning can favor one or the other. In fact, the particles can themselves be mixed together at *very* small spatial scales - such intimate mixing could contribute to the strong overestimation of LWP for the warm air advection scenario (as noted at the bottom of page 11). Even the interpretation as intimately-mixed particles is suspect though since the "fishnet" manifold is so tight near the AISA data in Fig. 8. Does the ratio really provide enough measurement power to discriminate mixed phases, particularly given the uncharacterized uncertainties I mention above - how can we exclude self-shading and sky view fractions as an alternative account for the apparent difference between "Holes" and "Domes?"

3. Formal hypothesis testing, or uncertainty quantification or propagation. The general character of the manuscript is to bring together multiple measurement modes (LWP by AISA and MiRAC) and conclude that both are important to interpret diverse cloud structure. Fair enough, but do the authors have an uncertainty budget for either instrument? For example, what is the error in the Backscatter - the vertical profiles seem very qualitative in nature and it is unclear to what degree the temporal axis represents meaningful change in structure. This is important in comparing said structure to the LWP maps - which are themselves uncertain up to a level determined by the instrument noise and unknowns in the observation system. LES analysis is of a similarly qualitative nature, and an important first step which is meritorious as an exercise, but given the large mismatch between distributions of simulated and measured $\mathcal{I}_s$ it is not clear that the LES has successfully explained or even accounts for the observations. Can the authors formulate this as a formal hypothesis test of some kind?

Minor technical clarification: How do the horizontal (i.e.) spatial scales compare between Fig. 3 and the AISA data?

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