

***Interactive comment on* “Finely laminated Arctic mixed-phase clouds occur frequently and are correlated with snow” by Emily M. McCullough et al.**

Anonymous Referee #1

Received and published: 10 April 2020

Review of “Finely laminated Arctic mixed-phase clouds occur frequently and are correlated with snow” by McCullough, Wing, and Drummond.

This study by McCullough et al. is one of few studies presenting very high-resolution observations of clouds. This study reveals that laminated features in Arctic clouds are not uncommon and attempts to quantify the relative occurrence of this phenomenon. It also estimates correlations between the occurrence of laminated clouds and the occurrence of precipitation, which could help improve our understanding of the formation mechanism of these laminations and/or of the impact of these laminations on precipitation.

[Printer-friendly version](#)

[Discussion paper](#)



Although I believe answers to these questions would make a great contribution to the field of atmospheric science, I have several issues with this manuscript as it now stands. For reasons detailed below, I would recommend this manuscript be rejected, but I would encourage the authors to resubmit.

Highlights

-Science

This study is one of few studies presenting very high-resolution observations of clouds.

-Figures

The figure presented through the manuscript are impeccable. The authors have used appropriate font size, color contrast, labels, and legends.

Major comments

- Lack of precision in the identification of laminated clouds and in the use of certain terminology

This manuscript relies on manual inspection of plots to create a climatology of the occurrence of laminated clouds. Manual inspection is a highly subjective way to classify observed scene and an impossible one to reproduce. I believe it is imperative for published science to be 100% reproducible. I would recommend the authors start from the vague rules they provide in P 5 L 15-19 to create a precise, programmable set of rules defining what are laminated clouds. As a last resort, I would ask the authors to provide all figures as supplemental material each one labelled according to their scene classification. I believe "Available upon request from the corresponding authors" is simply not sufficient in this case.

If the rules defined in P 5 L 15-19 were more precisely defined and implemented I would agree they could be appropriate to identify laminated clouds. That being said, they would not be sufficient to identify mixed-phased or multi-layer conditions which the

[Printer-friendly version](#)[Discussion paper](#)

authors claim to study as stated in the manuscript and in the title.

Mixed-phase: According to work by Shupe and others, additional information besides photon count is helpful to assess cloud microphysical phase. Thus, the authors statements throughout the manuscript and in the title that “the clouds observed over this 3.5-year climatology are mixed-phase” is not strongly supported. This work would benefit for example from using depolarization information.

Multi-layer: Multi-layer clouds are generally defined as clouds containing multiple liquid layers. Given this, to sustain their claim that the statistics presented in this study pertain to multi-layer clouds, the authors should probably perform a phase classification to distinguish between liquid and ice which both can produce high photon counts. Moreover, I would argue that the types of cloud presented in Fig. 3a and in Fig. 3d are quite different yet the authors consider them together in their statistics. At first glance, I would label the cloud in Fig. 3d as a “traditional multi-layer clouds”, and I would certainly need to be convinced that the cloud in Fig. 3a is a “multi-layer” clouds. Of course, this study would also be valuable if it was simply describing clouds in general (i.e., without multi-layer statements). That being said, this approach would require a rewriting of the introduction which claims that part of the uniqueness of this study is that it focuses on multi-layer clouds.

- Statistical bias caused by the scene classification methodology

This study is based on the analysis of entire days (i.e., 24-hrs) of observations. I believe this was done to keep data size manageable for manual inspection. This however creates an issue related to data gaps. The authors attempt to address this issue by defining “interpretable” days. That being said, they do not use this method consistently. For instance, as this classification now stands, a day with only 30 min of laminated clouds is interpretable even if 23.5 hrs of data are missing, but a day with 22 hrs of non-laminated clouds and 2 hours of missing data is non-interpretable. This methodology is sure to make all relative statistics presented in the current study biased high toward

[Printer-friendly version](#)[Discussion paper](#)

laminated conditions. I believe it would be fairer first to remove days with > 1-hr of missing data, then days with low-level cloud obstruction. I would consider the rest of the scenes as interpretable. Then I would classify those as clear or cloudy and I would further classify the cloudy ones as laminated and non-laminated. This would ensure that laminated and non-laminated conditions are estimated using the same sample size of “interpretable” cases and would generate unbiased relative frequency of occurrences. I would also recommend that the authors use 1-h scenes rather than 24-h scenes. This would correspond better with the time resolution of the weather reports and would likely increase the number of interpretable scenes.

————— Minor comments —————

- Abstract:

The abstract could be written in such a way as to be much more insightful. For example, “P 1 L 10-11” would be more informative if some actual correlation coefficients were given. Also, it would be more informative if information about part II of investigation A was provided; for instance, are there notable monthly differences in the occurrence of laminated clouds?

P1 L 2-4: This sentence is very long. Please consider rewriting it. P 1 L 6:” the expression “interpretable days” is not defined in the abstract thus creating confusion for anyone who has yet to read the complete manuscript.

- Number of tables

Have the authors consider putting some of their tables in an appendix or perhaps submitting some of them as supplemental material?

- Introduction

I would encourage the authors to shorten their introduction and to be more focused on what makes their study unique which is the fact that they provide very high-resolution observations of clouds and put them in context with precipitation occurrence. For exam-

Printer-friendly version

Discussion paper



ple, I would remove P 1 L 15-17, L 23-25, P 2 L 1-4, and in particular L 8-13 (Anyway “measurements which parameterize” is an incorrect statement since measurements do not “parameterize” they are “used to evaluate” or “used to construct parameterizations”).

I think your best statement in the introduction is P 3 L 3-5

I think the introduction would benefit from more background information on previous studies focused on high-resolution observations of mixed-phased clouds such as those conducted by Verlinde and coauthors.

- Organization:

Figure 1 is presented in the introduction before any information has been provided about the sensor used to record the information presented. I encourage the authors to move this figure after or within the methods section.

- Spelling and grammar:

There are several spelling and grammar errors throughout the manuscript. For one, the word “occurrence”, which is written at least three different ways: “occurance”, “occurrance”, and “occurence”. I would encourage the authors to run a spell check before resubmitting their manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-186>, 2020.

Printer-friendly version

Discussion paper

