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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of the mixed phase in the Arctic with a focus on the Svalbard region: a study based on spaceborne active remote sensing" *by* G. Mioche et al.

Anonymous Referee #1

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Overall Assessment: This paper provides an overview of cloud and specifically mixed-phase cloud occurrence, and some other properties, over the Arctic with an additional focus on the Svalbard region. The clouds in question here are certainly important to Arctic regional climate and to the global climate system, there is much to be learned about them, and they are a great difficulty to represent in numerical models. Thus, the general topic of the paper is on very strong scientific footing. However, the paper falls short in many regards. The writing is disorganized and unnecessarily repetitive. There are many grammatical and sentence structure problems. These many issues with the writing itself made the scientific evaluation process difficult and a re-write of the paper,

with the help of a scientific editor, is needed. However, there are also clearly scientific issues that also need to be worked through more thoroughly in order to ensure the robustness of results given here. I will detail all of these points in my comments below, but a couple are worth noting here. First, this study needs to do a better job of providing a full analysis of the uncertainties/limitations of the satellite measurements through detailed comparisons with ground-based measurements (especially targeting the biases related to clouds below 500m). Second, the paper itself evaluates a version 2 data product and shows that it is better. The product is clearly available and was produced by a co-author on this paper. Thus, it is absolutely unacceptable to publish this paper without conducting the analysis with the clearly superior data product. Overall, this paper should not be published in anything approximating its current form. I suggest ejection, extensive reworking, and a re-submission of the paper.

Significant Comments:

*Regarding limitations to the A-Train sensors, I do not recall reading anywhere about the latitudinal limitation to this constellation. They do not go north of 82 degrees. This is implied in the spatial map figure, but it would be a very good point to specifically mention in the text. This is particularly important because the Arctic domain north of 82 degrees is largely dominated by sea-ice, which is of clear importance to the Arctic system. Unfortunately A-train measurements can provide little information on the clouds in this important region.

*P23464, lines 20-30: Yes, but there are serious limitations regarding low clouds and multi-layered clouds that should be mentioned out front. Don't hide these limitations towards the back of the paper. Also, just because the different ways of processing the same satellite data set agree does not mean that the data set / methods are correct!

*P23465, line 5-8: ISCCP has been shown to be inconsistent with ground-based observations at specific sites. While spatially confined, ground-based observations are relatively robust at getting cloud occurrence.

*P23470: In the Arctic, many clouds occur at heights <500m. You avoid the issue altogether of clouds below 500m, which is perhaps the largest issue with using the satellite sensors in this study. The errors related to missing low-level clouds (<500m) can be evaluated using ground-based observations at a few locations. This should be done in order to provide much more robust occurrence statistics. Otherwise, it is unclear how the provided results actually relate to actual cloud occurrence fractions.

*P23470, line 14-16, and also P23477, line 3-5: Just because contamination of the signal at 500-1000m is less than at 0-500m does not, by itself, justify using the 500-1000m data. You instead need to understand and demonstrating the accuracy of the 500-1000m data!

*P23475, line 17: Yes the satellite observations presented here are “uniform” however they are also biased. Ground based observations are not biased in the same way, but due to different instruments at different locations they are not so uniform. Likely the best approach to get at a true cloud fraction (or MPC fraction) is to use a combination of these approaches. But little work has been done here to really evaluate the satellite retrievals within the context of the ground-based measurements.

*P23463, first paragraph: Why are only cold clouds considered in the determination of MPC occurrence? The way (I think) you define the MPC occurrence is the portion of clouds below the freezing point. But this inflates the MPC occurrence because there are also clouds warmer than this temperature that occur at times. I see no reason to add this constraint and it is not that useful for people unless they also know the fractional occurrence of T<0C. Without further justification, you should just use the total cloud occurrence at all temperatures.

*Section 4.2: I don’t think that this section (nor the figures that go with it) is needed. The whole thing can be summarized in a couple of sentences that basically say MPC-IB follow similar patterns to MPC and represent $\sim$ X% of the MPC population. Furthermore, at P23470, lines 24-26 it is stated that due to detection issues MPC-IB could be

overestimated.

*P23471, line 4: The fact that a “second version” exists, that it was produced by a coauthor on this paper, that it was published in 2013, and that part of it was used in this paper to compare with the first version all suggest to me that better data is available for the present study. If it is better, then it should be used. There is no reason to publish this paper with the present results when it is clear that better data are easily available. This paper absolutely cannot be published without redo-ing the analysis with the better data set. This whole section on comparing the two versions can then be removed from the paper.

*P23473-23474: I have some difficulties with the interpretations on these pages. Lines 24-26: I don’t get the connection here. Sure moisture is important for cloud formation, but the linkage to ice-liquid equilibrium takes a few more details! Line 29>line3: No, the inversions can contribute to the longevity of clouds simply because they are a moisture source, but preventing clouds at higher altitudes has little to do with persistence at lower altitudes. Line 3-4: Not really. The reason for transition season prevalence is to first order a product of the annual temperature cycle. Transition seasons have temperatures in the range were mixed-phase conditions are more probable from a microphysics perspective. Line 11-15: Mixture of concepts here is done inconsistently. Also, this study has not demonstrated any links like this. That would require some analysis of sea-ice data, which has not been done.

*P23477, line 9-18: A couple pages ago it is argued that ground observatory climatologies are NOT representative but then here it is argued that in situ measurements at Svalbard are! In situ measurements easily have as many issues as ground-based observations AND Svalbard is a single location that does not represent the full Arctic. A few case studies of mixed-phase clouds can be useful for understanding processes, however, multiple year’s worth of cases above ground-based observatories provides a much more generalized understanding of cloud processes than a couple of cases.

Minor Comments:

*There are many incorrect uses of words, grammar, punctuation, sentence structure, etc. Far too many to list here. The state of the writing in this document makes it difficult to read and even difficult to evaluate. A future re-worked manuscript should be sent to a scientific editor before it is submitted in order to ensure proper writing, sentence structure, and clarity.

*P23454, line 19-20: What does this sentence mean? "Prevail" over what? Do you mean that they are the most common cloud type over sea?

*P23455, line 12: "global" should be removed

*P23456, 1st paragraph. A couple of the sentences in this paragraph are not very clear. Should do some careful re-writing for clarity.

*P23456, line 28: Mixed-phase clouds are often simply referred to as "mixed phase" as is done here. This does not make sense. Should instead refer consistently to mixed-phase clouds or MPC.

*P23457, line 10: "unique"? This should be clarified, does it mean "single" or "distinct" or something else?

*P23457, line 12: "peculiar" should be "particular"

*P23457, lines 12-20. You are missing many of the earlier aircraft campaigns that were really formative in the understanding of MPC, and upon which many of these other campaigns were built. i.e., BASE, FIRE-ACE (1998), MPACE (2004), etc.

*P23458, line 9: A-train doesn't go north of 82 degrees. This should be mentioned explicitly.

*P23458, lines 10-13: Need to reword this for clarity.

*P23459: What is the pixel size used? i.e.., across and along track dimensions.

*P23460, line 14: greater than 30 what?

*P23460, line 25: What does "in the vicinity" mean? Is this horizontal, vertical? How far?

*P23461-23462: There is a lot of redundancy with the 2 degree latitude and 5 degree longitude statements.

*P23462, line 15-16: So this means >180m total thickness?

*P23463, line 8 and P23464, line 5: Need to introduce acronyms.

*P23463, lines 18-24: Redundancy.

*P23464, lines 20-22: This is info for the introduction that has already been stated.

*P23465, line 22-24: SHEBA does not equal Barrow.

*P23466, line 1: Need to be careful with definitions. Some of the numbers that are being presented from past studies are % of time, while those given in this study are % of the time that clouds occur.

*P23466, line 5: The word "ubiquitous" is used a number of times in this document and I think it is incorrectly used. Ubiquitous might be appropriate for describing the general cloud occurrence in summer (i.e., upwards of 80-90%). But at times the word is used in this document for much smaller fractions.

*P23466, line 22-23: I don't see anything special about 3000m. The same trend exists above 3000m

*P23467: I'm getting a bit confused. Please make it very clear which results relate to the whole dataset versus those that relate to Svalbard region. This is not always clear.

*P23467, line 4-5: Relative to what?

*P23467, line 14-15: It might be easier to combine this information with Fig. 3b.

*P23469, line 1: “Global” scales are not studied here.

*Section 5.2: The first two paragraphs of this “discussion” section are not that effective in that they simply repeat results that have already been presented with little analysis or interpretation. Moreover, this is not presented in a clean way. The points are jumbled together. There are also missed opportunities for actually using surface type data to draw solid conclusions instead of just referring to other analyses that have unknown applicability to the data presented here.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23453, 2014.