

Interactive comment on “Characterization of the cloud microphysical and optical properties and aerosol-cloud interaction in the Arctic from in situ ground-based measurements during the CLIMSLIP-NyA campaign, Svalbard” by Gwennoé Guyot et al.

Anonymous Referee #3

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Goyout and colleagues present an analysis of ground-based observations of thin Arctic mixed-phase clouds, snow precipitating from such clouds, and briefly blowing snow that were observed during the CLIMSLIP4 NyA campaign in Svalbard.

Recommendation:

Continued collection, documentation, and analysis of cloud and aerosol data from Arctic stations are important tasks. The present paper analyzes data from only a small

C1

number of events (4 mixed phase cloud, 3 precipitation, and 2 blowing snow events). Nonetheless measurements in Arctic mixed phase clouds are sparse and the paper well worth publication, as it is valuable to have measurements from many sites (to help minimize the possibility of regional biases). While I do have some concerns about the material (comments below), these concerns are reasonably minor. As-is, the paper was (rather obviously) not written by native English speakers. While I was able to follow the text reasonably well (except where noted below) and it is not strictly necessary, I recommend the authors seek additional proof reading.

Overall Recommendation: Minor Revisions.

Note to Editor: While none of my comments are particularly difficult or will require a major revision, as you can see I have a rather long list of comments. I know that some journals require “Minor Revisions” to be completed within two weeks. If that is the case for ACP, you may want to change the recommendation to give the authors more time to respond.

General Comments:

1) Include number of cases and uncertainty.

The analysis is based on only a small number of events. I think you should state the number of cases in the abstract.

Obviously this raises the question of how representative one should consider these results. It would be helpful if the manuscript contained more discussion of how similar (or dissimilar) the results obtained here are to other Arctic measurements.

2) Clean vs. Polluted.

I am not sure the “clean” case is really that clean. How do the aerosols in the clean case compare to background conditions (nominally when there is no influence from continental areas)? In general, it might be better to refer to the cases as the “relatively clean” and “relatively polluted” cases.

C2

Specific Comments:

Abstract. Please use present tense when possible.

Line 23. Suggest change to "This study examines cloud microphysical . . .".

Line 28. Strike "these" and change to "In situ cloud measurements are combined with aerosol measurements and air mass back trajectory simulations to study arctic cloud-aerosol interactions."

Line 37. Suggest change to "A relatively polluted case, where aerosol properties are influenced by anthropogenic emission from Europe and East Asia, is compared . . ."

Line 58. Strike "highly".

Line 60. I know this is a minor point, but the analysis of Dufresne and Bony applies to global clouds, and the bulk of the model spread is (I believe) due to changes in tropical and subtropical clouds. The arctic is important, and it would be better to site and discuss studies pointing specifically toward the importance of Arctic clouds here (and in particular low-level arctic mixed phase clouds).

Line 69. Suggest change to ". . . are often . . .".

Line 75. I am not sure I understand the intent of the sentence starting "In the Arctic, the umbrella effect . . ." and suggest you simply delete this sentence. Alternatively, you need to expand on your point here, and define what the umbrella effect is."

Line 93. I think "proving" is too strong an ascertain. Perhaps change to "suggesting".

Line 105. Change "complexified" to "complicated".

Line 106. I think you mean "proposed" not "assumed".

Line 129. Strike "if possible". You do quantify several parameters.

Line 145. Perhaps note manufacturer is Vaisala, and provide reference that document instrument performance. Has any research with this instrument be published previously

C3

from this site?

Line 165, Figure 2. "Sampling rate", by which I assume you mean the flow rate, is shown, not sampling volume is given in the figure. Suggest you put sampling volume in figure (and perhaps add short appendix to paper describing calculation based on flow rate if this is not published elsewhere).

Figure 2. I presume units are supposed to be m/s? And the slash or "-1" exponent is missing.

Line 169. Have a fixed alignment seems problematic to me. How often was the alignment changed? Does the data processing include restricting the analysis to periods when the instrument was facing into the wind?

Line 175. I think this uncertainty estimate is somewhat optimistic. Is the presence of (small) ice rather than water an issue for the FSSP measurements? This deserves some discussion.

Line 185. I do not see where the papers by Baker and Lawson conclude the uncertainties on the concentration and the effective diameter are 50 % and 80 %. Please clarify.

Line 192 – 196. I don't think this is a very good description, but in any event, since you don't use the data I suggest you either remove these sentences entirely or simply write "A Nevzorov probe that measures liquid and total water content was also present, but due to concerns with the data quality and inconsistency with the other measurements, these data are not used in this study."

Lines 211-221. Is this the first paper to use this instrument set at the Zeppelin Station? This description is rather succinct, and I would generally like to see a more detailed summary with stronger supporting references and discussion of uncertainties, unless this is covered in some other manuscript.

Lines 225. Please elaborate on this. What "conclusions" are you following?

C4

Line 230. How is it that there are more droplets than CCN? Do you mean only during the brief period before noon on March 29? Please explain further.

Line 232. You write, "Thus, this study will not provide quantitative results but qualitative ones based on case comparisons and variation studies." But you DO derive quantities in later section. Suggest you strike this sentence.

Line 234-240. Perhaps move this material up and integrate with earlier discussion of probes. Either that or add some comment to the top of this section indicating you will discuss processing in section 2.2.3.

Limiting CPI data to daily time scale is particularly limiting. What about the time scale for the Neph?

Lines 242-256. Are shattering affects completely avoided? It is not clear to me this is true, how do you know? A variety of correction / shattering detection approaches have been developed (based on the arrival times and looking for bursts of small particles). Are such algorithms being used here? Perhaps better to write "... the advantage that shattering effects may be much smaller."

Lines 254-261. I presume the cases are identified by manual interpretation of lidar imagery and FSSP data, meaning precipitation cases are defined by no significant FSSP signal but the lidar (or CPI or Neph?) shows particles are present? Please clarify.

Also, what was happening at other times? Surely it wasn't entirely "clear sky" conditions, except during these periods. Were there other events when the station was briefly in cloud (but not for long enough to get a good CPI distribution) or events with precipitation falling from higher clouds or deeper systems. Please describe the criteria that led you to these cases.

Line 274. The quoted term "precipitation layer" used here is called "snow precipitation" or simply "precipitation", at various points prior to this line. Please adopt a consistent

C5

term for the entire manuscript.

Line 278. How do you know there is no higher level cloud precipitating through the liquid layer? Perhaps you could use satellite IR and/or sounding data to help ensure these are simple single-low-level cloud layers?

Line 287. What is the "solidification point"? Do you mean you mean the homogenous nucleation temperature for ice of -40 C?

Lines 290-294. Obviously one can simply use the FSSP data to discriminate when you are in a "cloud" (with a small particle mode) or not. Are you suggesting something more sophisticated than this? See comments lines 254-261. If not, suggest you delete these lines, otherwise please clarify.

Line 299. What do you mean by "succesion of layers"?

** Line 305. What is the averaging interval for the CPI? Earlier you seem to indicate only daily averages would be used ... but clearly you have many samples in Figure 4. Have you mixed time scales?

Line 311. I suggest rephrasing this sentence to read, "When liquid water droplets are present, they tend to contribute far more to the total condensate than the ice crystals."

Lines 311- 314. In general, I think it would be more useful to quantify the fraction of time (when liquid is present). That is, how often is $F_{liq} >$ than 0.9 or 0.95. It seems to me the number of bins you have chosen is somewhat arbitrary and if I add up all the occurrences when $F_{liq} <$ 0.9 or 0.95 it might be an appreciable fraction of the time?

Line 318. I think I understand what you mean here, but would rephrase as, "One expects a greater range in g for ice particles due to their larger variation in size, as well as shape/habit differences. Thus it is not surprising to observe larger deviations in g (from a pure linear fit) at values of F_{liq} below 0.5." In general, you might add bars show the standard deviation (or standard error) in g for each category to make it clear this is not simply sampling variability.

C6

Line 325, Figure 4. So the wind speed is always 13.33 m/s? Perhaps this is the mean horizontal sampling interval? I suggest also listing the standard deviation, or some measure of the variability.

** Line 331-336. Costa 2014 is a weak reference (a conference poster) and the link that is provided was not even functioning when I tried to look at the poster. In general, I do not understand what you are trying to communicate. Please expand this point to make the point clear without relying on this weak reference.

Line 340. What do you mean by absolute values? I don't follow. As far as I can see, you are making quantitative use of the IWC and modal values.

Line 355, Figure 6a. Automated classification techniques vary considerably in their classes and results. It would be helpful to see some representative images (good and bad) for the various classes that are heavily occupied. And more broadly, I think researchers might find it interesting to see –and it would be good to document - crystal images from your site. Perhaps you could add such as an appendix?

Line 360. I think you mean “confounded” where you have written “confronted”. Please rephrase this sentence.

Line 361. Strike “so”.

Line 363. Change to “... and a high percentage of regular shapes...”.

Line 366. So what should I conclude here? Would you say your results suggest that vapor deposition is NOT the primarily growth mechanism, and its dominated by riming? Or are both important?

** Line 355 - 366. Is aggregation of ice particles important? Where do aggregates fit in the classification? In general one might expect ice effective radius below cloud to increase in size because of aggregation. Is there evidence for aggregation in either you “MPC” or your “precipitating” cases? Why are the particle sizes larger in the precipitating cases.

C7

Lines 376-381, Figure 6b. Why have you focused on the case averages? Given the earlier discussion (Figure 4) regarding the asymmetry parameter and F_{liq} , I was expecting some analysis of the phase function as a function of F_{liq} .

Line 383. Suggest you change “lateral diffusion” to “lateral scattering”, which you used in the preceding sentence. Is some distinction here that I am missing? If so, please explain further. Perhaps change to “Indeed, lateral scattering increases when F_{liq} decreases, as one would expect if there is relatively more scattering by ice crystals and less by water droplets.”

Line 408 and 515. Plaque? Perhaps you meant sideplanes??

Line 438-440. See comment lines 290-294. I am not clear on what this procedure is. Please unify this discussion to a simple point in the manuscript (perhaps the concluding section).

Line 438. I think you mean “explored” rather than “explicated”.

Line 440-442. I think leaving off a comparison of results from other Arctic regions to a future publication is rather weak (see general comments). It seems to me that such a literature examination can be accomplished in a week, certainly less than two weeks. At a minimum, some discussion of effective radius observed at ISDAC and MPACE should be included.

Line 465. Change “output” to “outputs”.

Line 499. Presumably it is only the cloud top you don't know from the lidar. Why do you write that you can't assess the location of cloud base, as well?

Line 513. You have written “DMPS” here, but I think you mean “CPI”. Otherwise, something is very wrong with this result.

Line 535. Change to “Northern Europe”.

Line 547. Include “possibly”. Possibly this is due to scavenging, but might it be due

C8

simply to spatial variability in the aerosol "plume"?

Line 552. Given the description of Da you give on line 554-6, I suggest you show a plot of Da. If not, I suggest you provide some characterization of the variability and reason to believe this change is meaningful.

Line 555. I think "proves" is overly assertive. Rather I would say that it is consistent with expectations of the Towmey effect.

Line 568. Change to "Northern Europe" or otherwise make consistent with early description.

Line 569. Do you mean the aerosol were airborne for a longer period of time = "longer residence time"? Or that they can from farther away? or both? I assume both. Perhaps provide some values for the residence time. Previously you noted the aerosol where "slow moving" in the clean case.

Line 548. Being is not the correct verb tense. Suggest you change to present tense, active voice : "Indeed, McFarquhar et al. (2011), indicate that CCN abilities are mainly due to the aerosol size in the Arctic."

Line 618. I am not sure that assuming the LWP variations are small is a good approximation – and least I don't find your reason compelling. It might help if you provide an estimate for the LWP range for each case, given that you can estimate cloud-base from the lidar, and cloud-top from the radiosonde (I think it is reasonable to assume cloud top at the inversion), and LWC from the FSSP. Obviously with only 4 cases you can't afford to restrict the analysis. But I don't think you should just dismiss this factor. Rather (unless the above estimated do fall in a narrow range) I think you are simply going to state it is a limitation of your analysis and provide estimates for the LWP.

**Line 635. The values for IE and NE shown here are NOT the slopes shown in Figure 13. Shouldn't these data be on a log-log plot?

Lines 637-639. Perhaps change to, "These values for IE and NE are similar to those
C9

and reported by Garrett et al. (2004) and support his contention that these values are higher in the arctic region than at lower latitudes."

Line 709. Change "stellar" to "stellars".

Line 713. Change "According" to "Following".

Line 757. If I understand, the lidar is not at the site but in the nearby town. Is it possible there was cloud over the mountain and not over the town? Are there notably higher wind speeds for these cases ?

Line 765. To me, the important question is not whether you can tell the difference between being cloud and blowing snow, but can you tell the difference between "precipitating snow" and "blowing snow". Is the only difference that blowing snow is composed of a large fraction (> 90%) irregulars? That is helpful, but doesn't this leave open the possibility that precipitating events composed of heavily rimed particles will look just like blowing snow by this criteria.

Line 773. I don't understand the intent of this sentence regarding resuspended particles. Please rephrase to make the relevance clearer.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-672>, 2017.