Review of “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”, Guyot et al.

Overview of paper:

This paper presents cloud microphysical and associated measurements from a ground-based site in Svalbard. Three “episodes” are identified, which are conditions where the following was sampled: liquid and mixed-phase layer (LMPL); Precipitation layer; Blowing Snow. These three regimes are characterised in terms of their optical scattering and particle size distribution characteristics.

Next, there is a focus on the cloud characteristics of “clean” vs “polluted” cases. Numerical modelling is used to ascertain source regions; however, from the presentation it is quite difficult to see how these source regions were ascertained. It may be useful to show the backtrajectories.

Lastly analysis of the indirect effect parameter is presented. Admittedly these show fairly poor correlation, as expected. It is useful, but I think this needs to be presented more clearly because there seems to be a jump in the conclusions when stating that the results confirm the first and second aerosol indirect effects. I did not clearly see how this conclusion comes from the results or discussion.

I am recommending a major revision to this manuscript: the observations are very useful to the community, and the topic is highly relevant; however, the results are not presented in a clear, coherent way and at times I feel the data do not fully support the conclusions / main findings

General overview / readability comments:

There are essentially 4 parts to the paper: 1) characterisation of the different episodes that were sampled; 2) numerical modelling to ascertain source region; 3) analysis of clean vs polluted cases; 4) analysis of the indirect effect.

As presented I feel that the modelling does not add a great deal to the main paper, and may be better in supplementary material, which might help give the paper a clearer focus. This may also be the case for the section that characterises the three types of episode. Then the paper could focus on the indirect aerosol effect as its main message. Unfortunately there is a danger that the main message of the paper could be lost because too much is being covered.

I do not have issues with the measurements perse. However, I believe the CPI size distributions are not accurate for particles of sides smaller than 60-100 microns, where there are significant uncertainties. This should be discussed with literature cited to support the discussion.

Specific comments:

The title is far too long and unfocussed. I believe the paper would provide the community with a clear message if it focussed on aerosol indirect effects measured from a ground-based site, Svalbard.
There are many typos that will need to be picked up through this manuscript. I have not focussed on picking them up, but it would have to be done before publication.
E.g. Wrong words used in places: introduction line 58 “Specially”….line 76-77: which suspects than clouds… not good gramma – the greenhouse gas effect cannot “suspect” anything
E.g. Contractions used throughout: e.g. “don’t “ line 117, doesn’t line 335 - scientific writing should avoid this.
Abstract and text throughout is often written in the future tense. Past tense is more appropriate for scientific journals
More examples of future tense:, line 239-240: “will be 1 minute for the FSSP…”
Abstract: This is in agreement with the first (Twomey) and second (Albrecht) aerosol indirect effect. I think this statement is inaccurate. I agree that is consistent with droplet activation theory. I am not sure you can argue it is consistent with aerosol indirect effects.
The site is explained along with related measurements. More care is needed here..e.g. a “ceilometer, CL51 model” does not give the manufacturer, Vaisala. The same is true for the cloud instrumentation. The models are given, but not the manufacturer. Has the PMS FSSP been updated to the latest DMT electronics? Note, the acronym PMS should also be spelled out in full on first usage. In short, more care / attention to the details is necessary here. This is the same throughout these sections. I believe for the aerosol instrumentation section too.
“Due to high discrepancies”, line 195. It is not clear what this means to me. High discrepancies in what exactly? And due to what?
Figure 3: should stand alone, but LMPL is not defined in the caption.
Line 282: “the station is so below the mixed layer”, not sure “so” is the correct word here
Line 310: talks about true “mixed phase” clouds being rare. This has also be observed from other ground-based sites - See e.g. Lloyd et al. (2015) , so these papers should be cited.
Section 4.1 the modelling seems to be a small part here. I think it would be better presented in supplementary material, as the only addition they make to the argument is where the air was coming from. However, back trajectories are mentioned, but not presented as far as I could see. The back trajectory plots should be available so that the reader can assess the statements being made.
The explanation of how to find the activation diameter in section 4.1 could be clearer. “the aerosol PSD is necessary” this is fairly obvious, so why bother complicating the discussion? Just say the DMPS was used to find the diameter where the cumulative number (integrated from right to left) was equal to the drop concentration from the FSSP.
There are two sections labelled 4.1
Section 4.3: this is a key part of the paper. These are the main findings in my opinion. Perhaps these should be the focus of the paper, but yet I still have some misunderstanding.

Your equation 5 says:

$$NE = \frac{\partial \ln y}{\partial \ln x}$$

Your Figure 13 has a curve fit: Y=-0.3X+8.5

If we take the derivative of Y wrt X we obtain:

$$\frac{dy}{dx} = -0.3$$

So

$$\frac{d \ln y}{d \ln x} = m\frac{x}{y} = 1 + \frac{0.3}{y}$$

Therefore by these arguments NE depends on y, and is not a constant. But in the manuscript NE=0.43 is presented. It is not clear to me where the numbers for these coefficients come from. Was a different regression performed that is not in the paper?