

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Reviewer comment on acp-2021-24

Anonymous Referee #1

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Referee comment on "Characterization of aerosol number size distributions and their effect on cloud properties at Syowa Station, Antarctica" by Keiichiro Hara et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-24-RC1>, 2021

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### General comments:

The manuscript at hand characterizes aerosol number size distributions observed at the Japanese Antarctic Station Syowa. In particular, the authors deduced the origin of air masses during new particle formation (NPF) events from their data evaluation. In a further approach, they discussed the impact of the stratospheric ozone depletion on NPF events and their potential effect on cloud properties. The findings are based on measured particles size distributions, size segregated chemical analysis, meteorological observations as well as back trajectory calculations. There are several highly interesting conclusions drawn from this study:

- NPF occurred mainly in the free troposphere (FT); only during summer, boundary layer (BL) NPF was dominant.
- Enhanced UV radiation during the seasonal ozone hole period provoked enhanced NPF in the FT, indicating a link between stratospheric ozone and tropospheric aerosol properties.
- Particles formed during ozone hole induced NPF events could grow to sizes relevant for acting as cloud condensation nuclei (CCN).
- In this vein, so formed CCN potentially effected cloud properties and may have caused the increasing long-term trend of cloud amount observed above Syowa during December / January.

Unfortunately, the article in its present appearance has some weak points. It is regrettable that in several issues the reasoning is barely traceable and inadequately supported by the presented data evaluation. This is especially obvious concerning the claimed link between ozone hole → NPF → CCN → cloud properties → cloud amount. I think it is worth the effort addressing this weakness and considering a more in-depth analysis.

Notwithstanding, I am confident that this study has the potential to substantiate at least part of these highly interesting results, provided that a more stringent and rigorous analysis is presented. Overall, the subject is appropriate to ACP. Hence, I recommend accepting the paper after major revisions according to my specified suggestions listed below.

### **Specific comments:**

Chapters 2.2.2, lines 112-119: To be honest, I do not understand the mathematics behind your estimate regarding J5. Why do you need the term Coag.S10-20N10-20, but then neglecting the condensation sink Cond5-10N5-10? Moreover, it is not even clear in which way you calculated the condensation sink shown in Figure 6d. Please specify input values for eq. (2) and in addition show the measured total particle concentrations in Figure 6.

Chapters 2.3: Back trajectory analyses are a crucial tool in this study, so the authors should provide more details: Why did you rely on the NCEP meteorology data set? The GDAS dataset has a higher resolution and is more accurate in general! Why an initial starting point of 500 m above ground has been chosen, well above the aerosol measuring point? With hysplit (using GDAS input), it is possible to start trajectory ensembles from different high levels. This option could be useful to assess the reliability of the back trajectory analysis. Another point: 5-days back trajectory may not be sufficient to address the origin of particles in the accumulation mode. Finally, does the chosen 1500 m boundary level between FT and BL refer to height above ground?

Chapters 3.2, lines 180-181: Is there any evidence from your data that in this case sea-salt originated from the snow surface? This should be specified, otherwise a reference is needed.

Chapters 3.2, lines 185-186: In this case, 10-days back trajectories could be beneficial!

Chapters 3.3, line 223: "Particularly, fresh nucleation mode appeared only in end-August..." you mean fresh nucleation mode without aged nucleation mode?

Chapters 3.3, lines 259-264: Please delete this sentence as well as the (R1) and (R2), because it is (very) basic textbook knowledge.

Chapters 3.3, lines 266-267: This is not visible in Fig. 6g! I daresay that UV radiation is roughly comparable in Oct/Nov and December. Please provide numbers of the measured difference.

Chapters 3.3, lines 270-279 and Chapter 3.4: This part is rather speculative and barely convincing! I agree that enhanced UV-radiation under ozone depletion conditions may potentially have an impact on NPF and CCN concentration during this period. However, in my view it is hard to believe that those NPF events and their subsequently grow to CCN relevant diameters will last more than around 2 months in the FT and will then have any significant influence on cloud properties in Dec/Jan! First, you estimated particle lifetime solely based on coagulation sink. Is there some evidence, that such a simplification is adequate? Moreover, consider that comparable photochemical processes provoking NPF in BL and FT surely also proceed during Dec/Jan (note comparable UV radiation and even more prominent DMS emissions). Thus, I cannot realize that NPF happened more than 2 months before in FT could have any significant impact on CCN concentrations and cloud properties in Dec/Jan. If at all, only detailed model simulation may give a robust answer concerning this conclusion.

Chapters 3.4, line 293, Figures 8, and 9b: Does the calculated significance level refer to an ANOVA variance test, meaning that the corresponding distributions are significantly different on this level.

Figure 5: Please specify the black lines and dots in the figure caption.