Review comment on "Annual cycle of aerosol properties over the central Arctic during MOSAiC 2019-2020—light-extinction, CCN, and INP levels from the boundary layer to the tropopause" by Ansmann et al.

Anonymous Referee #1

This paper presents the annual cycle of height-resolved aerosol properties measured over the central Arctic by the MOSAiC lidar complemented with in situ observations. This unique dataset provides insights into the vertical distribution of optical and cloud-relevant aerosol properties (CCN, INP) throughout the troposphere and different seasons, which is important for modeling clouds and the present and future Arctic climate. The analysis of the annual cycle was extended with several case studies, with an emphasis on the presence of a persistent wildfire smoke layer in the upper troposphere.

The paper is well written and has a clear structure. I recommend publication after addressing the comments below.

General comments

1. In Sect. 3.1 you present three case studies with clean and polluted conditions during summer before you discuss the annual cycle of the aerosol conditions during the MOSAiC year. The purpose of presenting these case studies is not yet clear to me. For example:
    - How did you select these case studies?
    - Is some information on the case studies required for the analysis of the annual cycle (e.g., derive lidar parameters that are used in the following sections)?

    Please extend the description of the analysis regarding the case studies (e.g., goal of the analysis, selection of the case studies, …).

2. The detection of a pronounced and persistent wildfire smoke layer in the upper troposphere and lower stratosphere is an important MOSAiC highlight. However, I have the impression that the discussion about the smoke layer from previous studies was sometimes interrupting the flow. For example:
    - In line 474-475 you state that the annual cycle of the aerosol optical properties is shown in Fig. 6 and 7, so I was expecting that you would discuss the profiles of backscatter and extinction coefficients that you observed during MOSAiC. However, in the following lines you don't focus on the discussion of these figures/your observations, but you report the findings regarding the smoke layer from previous studies (see also specific comment 14).
    - In line 52-63 you put the discussion of the smoke layer together with the description of the instrument setup aboard Polarstern, which does not really fit.

    I fully agree that the presence of the persistent wildfire smoke layer is an interesting feature/finding that needs to be discussed, but please revise if the structure of some paragraphs could be revised to maintain a clear flow.

Specific comments

1. Line 113-118: This paragraph seems a bit out of place. You already introduced in the previous paragraph that local INPs of biogenic origin seem to control ice nucleation in the boundary layer in summer (line 101-102). Also, the definition of homogeneous and heterogeneous nucleation should be moved forward in the paper (before discussing the results of different INP studies). Please restructure the paragraphs regarding INPs. You could also add reference to Carlsen and David (2022) and Sze et al. (2023) as additional motivation for the importance of INP on cloud phase. For example, Carlsen and David (2022) saw a clear relationship between cloud phase over open ocean, snow and sea ice cover suggesting that local INPs are important for cloud phase.
   Carlsen, T., & David, R. O. (2022). Spaceborne evidence that ice-nucleating particles influence high-latitude cloud phase. Geophysical Research Letters, 49, e2022GL098041. https://doi.org/10.1029/2022GL098041
   Sze, K. C. H., Wex, H., Hartmann, M., Skov, H., Massling, A., Villanueva, D., and Stratmann, F.: Ice-nucleating particles in northern Greenland: annual cycles, biological contribution and parameterizations, Atmos. Chem. Phys., 23, 4741–4761, https://doi.org/10.5194/acp-23-4741-2023, 2023.
2. Line 165-173: In subsection 2.2 you describe the lidar instrument. Is there a reason why you only present the parameters measured/derived by the lidar in Sect. 2.5? I suggest presenting the parameters measured by the lidar already in Sect. 2.2. If you want to keep the parameters in a separate section, you should refer to section 2.5.
3. Line 175: In the introduction you mention that sun photometer measurements are possible from March to September (line 88-89). Why were no measurements performed between March and June aboard Polarstern? I think it would be nice if you could include the sun photometer measurements in Fig. 9 (see specific comment 19).
4. Line 182-185: In Sect. 2.1 you state that Polarstern was mainly drifting at latitudes > 85 °N. The CALIOP measurements published by Yang et al. (2021) were obtained at latitudes between 65 °N and 82 °C (between 2006 and 2019). Thus, the CALIOP and MOSAiC measurements do not cover the same region and time period. How does this influence the comparison between MOSAiC and CALIOP measurements (e.g., Fig. 8 and 9)? Please add a few sentences regarding this point.
5. Table 1: What is the difference between the term 'exemplary' and 'typical' uncertainty? I would suggest sticking to one term.
6. Line 299-301: Please add a reference.
7. Line 395-410: In Sect. 2.9 you describe the in situ measurements of aerosol microphysical properties and INP concentrations that were conducted on Polarstern. Maybe it makes sense to move Sect. 2.9 forward to the other instrumentation. Like that you first describe the applied instrumentation and then introduce the different data analysis methods.
8. Line 396-410: Please include some information on how the particle number, CCN, INP concentrations were measured (e.g., instrument type) aboard Polarstern.
9. Line 437: Where can I see the backward trajectories for the 4 km height? If they are not shown, please state that.

10. Fig. 1: In the caption of Fig. 1 you state that the observations with the near-range telescope are shown by the dashed line in Fig. 1.a. This is not the case. The near-range observations are represented by thinner lines. Please change the plot or caption accordingly.
11. Fig. 3: Comments to Fig. 3:
    a. The 0 and 1 labels on the x-axis overlap. Please revise the labelling.
    b. For the right panel you show the residence time on the upper x-axis, while for the other panels you show the residence time on the lower x-axis. I suggest showing the residence time on the upper x-axis for all panels and the interval time on the bottom x-axis.
12. Line 475-486: The aim of this section is to present the annual cycle of the optical aerosol properties measured during the MOSAiC year (Fig. 6 and Fig. 7). However, in the first paragraph you focus mainly on the pronounced wildfire smoke layer that was discussed in detail by previous studies. I would suggest focusing on your observation and presenting Fig. 6 in detail before you discuss the results of other studies (see also general comment 2). For example, you could highlight the presence of the smoke layer in the upper troposphere (especially in winter) in Fig. 6.
13. Fig. 6: In the caption of Fig. 6 you state that 9-15 (October-March)/5-8 (June-September) daily observations per month were considered. Are those all measurements that were available or according to which criteria did you chose the observations? Please specify. How many observations were considered for April and May? Please add the number of observations also for April and May. Alternatively, you can specify the number of observations for each one-month/two-month period in brackets in the legend of the figure.
14. Line 532-533: Here you state that the MOSAiC and CALIOP observations agree well during the winter months (Fig. 8). However, above 7.5 km, the extinction coefficients measured during MOSAiC are higher compared to the CALIOP measurements, which is an effect of the smoke layer. You could include this in the interpretation of the figure. The same holds true for the extinction coefficients measured in the upper troposphere during the summer season.
15. Fig. 8: Comments to Fig. 8:
    a. I think it would be beneficial if you could include the standard deviation for the CALIOP measurements. This allows to assess if the MOSAiC year was significantly different compared to the 15-year mean CALIOP profile.
    b. To support the comparison between the 15-year CALIOP and MOSAiC lidar measurements, you could investigate if the MOSAiC and CALIOP measurements of the same time period are in good agreement: I.e. can the CALIOP profile from October 2019 to September 2020 capture the vertical profile measured by MOSAiC?
16. Line 537-549: In this paragraph you compare height-resolved aerosol observations from MOSAiC, a TBS and CALIOP. However, these observations were obtained in different regions and over different time periods, which makes a direct comparison difficult. What is the goal of this comparison? In my opinion it does not make sense to compare the aerosol observations of these three datasets, as they are quite different. As this paragraph is not required for the outline of the paper, I suggest removing this paragraph. If you decide to keep this comparison in the final manuscript, you should include a figure to illustrate the comparison. Also, I am a bit surprised that you state that good agreement between the MOSAiC, CALIOP and TBS

aerosol profiles was found (line 544-545), when you write in line 533-534 that in summer, the lower troposphere measured during MOSAiC was much cleaner than described by the 15-year mean CALIOP profiles.

17. Fig. 9: Comments to Fig. 9:
    a. In Sect. 2.3 you introduce the sun photometer measurements and state that these measurements were performed between June to September. I think it would be beneficial to include the sun photometer measurements in Fig. 9.
    b. Please include the definition of the vertical bar in the caption (uncertainty?). Why is there no vertical range for AOT 0-20 km? Why are there no data points between May and September for AOT 0-20 km? Please revise.
18. Fig. 10: Comments to Fig. 10:
    a. The lower standard deviation bar is missing for some values. Please include it.
    b. What is the difference between the filled/empty blue circle? Please specify.
19. Line 637-650: In this section you describe the strong difference between the winter and summer INP levels that is visible in Fig. 11. This pronounced annual cycle in the INP concentration can be mainly explained by the difference in summer/winter temperatures, as ice nucleation is strongly temperature dependent. To investigate the annual cycle of the INP concentration you should show the INP concentration for the same ice nucleation temperature over the entire year in Fig. 11. For example, Creamean et al. (2022, Fig. 4) observe constant INP values at T=-25°C over all seasons, whereas an annual cycle in the INP concentration is observed at T=-15°C. It would be interesting to see if the lidar can reproduce the in situ INP concentration over the entire year. Thus, I suggest to extrapolate the data shown in Fig. 11 to warmer/colder temperatures in winter/summer (e.g., show in situ and lidar INP concentrations at T=-25 °C and T=-15 °C over the entire year).
20. Fig. 11: Comments to Fig. 11:
    a. Why do you use the same temperature for 250 m and 2000 m in winter (-25 °C) but a different temperature in summer (-10 °C/-15 °C)?
    b. At which temperature were the in situ measurements conducted in winter? Please specify.
    c. Why are some symbols filled/empty? Please specify the color-coding.
21. Line 689-693: Here you state that the INP concentrations of 1-20 L^{-1} are in consistency with MOSAiC retrievals of n_{ICE} and that 10 MOSAiC cirrus systems were analyzed. To support this statement, I suggest to include the n_{ICE} data points in Fig. 12.

Technical comments

1. Line 8: cloud condensation nucleus (CCN) and ice nucleating particle (INP) concentrations
2. Line 66: ‘question’ instead of ‘questions’
3. Line 68-71: Long sentence. Please revise.
4. Line 72: ‘Observations’ instead of ‘observation’
5. Line 108: The acronyms SML and BWS are introduced but not used in the paper
6. Line 109: ‘midlatitudes’ instead of ‘midlatitudes latitudes’
7. Line 151: The acronym AOT was only introduced in the abstract. Should also be introduced in the main text.
8. Line 152: ‘Sect. 3.2 and 3.3’ instead of ‘Sect. 3’
9. Line 158: ‘between’ instead of ‘betwee’
10. Line 296: Add ‘above to surface’ to make it clear that the height levels of 250 m and 2000 m do not refer to ‘below the tropopause’
11. Line 332: Replace one ‘n_{INP}’ by ‘n_{ICE}’
12. Line 355: The acronym HULIS is introduced but not used in the paper
13. Line 369: Not sure if the acronym DIN is needed.
14. Line 436: Please reference to figure Fig. 1: ‘… above 1.5 km height (Fig. 1)’
15. Line 450: add reference to figure Fig. 1 ‘… was observed (Fig. 1)’.
16. Fig. 4 caption: ‘smoke lidar ratio’ instead of ‘smome lidar ratio’
17. Line 475: add ‘… of the year-around backscatter observations …’