Review of: "Impact of wildfire smoke on Arctic cirrus formation, part 2: simulation of MOSAiC 2019-2020 cases" by Ansmann et al.

Content

The study describes the associated modell simulations to polar lidar obervations of cirrus clouds and aerosols in the time frame between October 2019 and March 2020 during the famous MOSAiC expedition described in the first part of this paper series. The idealized simulations focus on the impact of aged wildfire aerosol on ice nucleation by testing the senstivity of different synopic updrafts, temperatures, the impact of sedimentation, and the impact of very idealized gravity waves. It was found that only wildfire aerosol as heterogeneous INP can explain the cirrus observation and it is stated that heterogeneous freezing was the dominant freezing pathway in the observed cirrus cases.

Overall impression and rating

I am a reviewer of both manuscript parts and find the first part really excellent. The second part is also important and valuable for the scientific community, but I find the linking of the idealized simulations with the observations and especially with the respective synoptic situation not well done. I think the way of using idealized simulations is good in principle, but what is missing here is to show which dynamic situation fits best to the observations. As is also emphasized in the paper, the dynamics (updrafts, gravity waves, air mass history) plays a decisive role in addition to the influence of the wildfire INPs. In particular, trajectory calculations for the individual cases would be helpful to better classify the possible updrafts and to better estimate the temporal development along the air mass history (see comment below). This would clearly help to better support the main message of whether heterogeneous or homogeneous freezing is the dominant nucleation pathway.

In general, the manuscript is well written and structured, the illustrations are also excellent, but I am still missing a few points in the second part of this paper series that need to be addressed before the manuscript can be published. For this reasons, I recommend publication in ACP after addressing my comments and some manuscript revisions.

Main comments/questions:

- Section 3: The section of the manuscript in which the simulations are described (especially pages 12-17) is well explained and easy to read. However, a large part of it is already explained and described in detail in some other studies on cirrus simulations, so that the added value for the scientific community is rather limited. Examples include the studies by Kärcher et al 2019, Krämer et al. 2016 or Spichtinger and Cziczo (2010). It would be good either to shorten it a bit or at least cite some of the studies showing the same effects were cited in the text.

- Figure 5/6, e.g. line 82: Test of lower updraft velocities also in combination with gravity waves. In Kärcher and Lohmann 2002 the synopic updraft range span over 0.01m/s to 0.1m/s which was also tested in Krämer et al, 2016 in comparison to research aircraft data. The low updraft can also produce low ICNC values in case of homogeneous ice nucleation. Also the combination of low updraft with gravity waves can produce low ICNC in the range of <1-10L-1 as shown by Kärcher et al 2019. It would be good to also include this updraft range of 0.01 m/s in your study as it seem to be important for final answer about the dominant nucleation pathway.

- I find the assumption of any climatological updafts a bit too simple to answer such an important question as the dominance of nucleation mechanism. You have clearly explained the role of updraft and gravity waves. In order to show which meteorological conditions were present during the cirrus observations, it is essential to look at the air mass history. Why not simply use trajectories calculated from meteorological fields, e.g. ECMWF ERA5, to estimate the large-scale updraft. ERA5 already includes some gravity waves, so you would only need to estimate the smallest scale gravity waves in your simulations in addition. Another advantage is also that one could see how long the cloud has potentially already existed before your observation and how many nucleation cycles may have already occurred. As you describe, both have a significant influence on how many INP have already been consumed and sedimented and whether homogeneous freezing might also play a role. I therefore suggest that you make similar trajectory calculations for the cirrus cases, as already shown in Part 1, and determine the updraft of the airmasses during and especially before reaching the observation site. This could be used to create a PDF plot, which could then support the hypothesis described with your assumed updrafts. I guess this would support and better substantiate your statement.
- You state in your text that the simulations showed that the INP reservoir was never completly used. But the cloud tpyically exists over longer time periods than just the 2500s used in your idealzed simulations and also can have multiple life cycles during the airmass transport i.e. multiple uplifts, nucleation, sedimentation. And even before your observations there might already be multiple cloud occurrences over hours to days within this airmass which just passed your observation site at a specific time. Why shouldn't all the INP have already been used up in that time frame? And how do new INP get into the cloud then? This comment is closely linked to my previous comment about the air mass history and should also be shown and discussed in the text.

Specific comments/questions:

- Line 168-169: Are the radiosonde data measured outside or inside of cirrus ? This would of course has significant influence on the intial values for the simulation.
- line 257-260 and Figure 3: Extrapolation espacially on a logarithmic scale can leed to extremly large errors and deviations. As the ice crystal number concentration (ICNC) is so important for the simulation results to compare with, you should at least show maybe based from an example of another measurement campaign, that such an extrapolation is approximatly valid and reasonable.
- line 267: "and would probably widely prevent the occurrence of high ice saturation ratios of 1.3-1.4." This is true, if not all INPs are already consumed. Otherwise, it is of course possible that the supersaturation will continue to increase until homogeneous nucleation sets in at some point.
- line 391-392: "Figure 9 provides an overview of the smoke impact on ice formation for the main range of MOSAiC cirrus top temperatures from 199-213 K". Why are the cloud top temperatures lower than showed in Figure 4 of part 1? There you could see temperatures ranging from 197-225 K.
- Line 395: Difference between ICNC values in the virga (range of 0.1-20 L-1) and cloud top (4-300 L-1, line 259) obtained by your extrapolation method. High values are also partly visible in your cases which are shown in part 1 with ICNC in the upper part of the ICNC observation in the range of 50-100 L-1. Were do they come from ? Are they comming from multiple nucleation events and are just to small to sediment ?
- Figure 9: What would be the impact on the starting time of your idealized gravitiy wave? You always start with the ascending part of the wave together with the start of the simulations. I would assume, if you start with the descending part of the wave you could create even higher cooling rates / updrafts

at the time of nucleation and maybe possible even high enough to trigger homogeneous nucleation to occur. I guess the phase shift is similar sensitive as the different wavelengths of the wave and should also be tested in this study.

Technical comments/suggestions:

- line 39: " level, in (c) the ", I guess you ment "level, and (c) the"
- line 97: Skip one "is".

References

- Kärcher, B. U. Lohmann, A parameterization of cirrus cloud formation: Homogenous freezing of supercooled aerosols, J. Geophys. Res., 107(D2), doi:10.1029/2001JD000470, 2002.
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