Reviewer 2:

The manuscript presents the results of a Raman lidar sounding campaign in Ny-Ålesund, Svalbard, using available lidar equipment and already published analysis techniques. The campaign aims at characterizing aerosol properties as these are, according to the authors, not very well understood in Svalbard, and may help with the definition of Arctic Haze.

I strongly support publication of results and conclusions of all remote sensing experiments, that have scientific merit in Remote Sensing, and this work definitely has the potential for such a publication. Unfortunately, while the paper presents the results of atmospheric characterization, it fails to make any significant conclusions (or point out specific instrumentation / analysis advances), which would be the true added value to science of this work. I would strongly recommend the authors to re-think what are specific advances to the understanding of the atmospheric conditions and point them out clearly. I recommend a major revision of the manuscript.

Thank you for your review. See our answers below and all new text in the manuscript is in blue color. Generally, we have extended our conclusions by summarizing bullet points. We hope that this improves and facilitates the readability.

Specific comments

- In section 2 the authors describe the lidar they used and the methods they applied to extract atmospheric properties. Here, it is not clear whether they used standard (already published) methods only or they used them as a basis for their own. This should be more transparent.

For the evaluation of the lidar data we are using the Ansmann 1992 paper (our quote 19). This was already mentioned. We do not employ own methods, so we decided to be short here. However, reading your remark we add:

The channel at 1064nm has been evaluated according to Klett (1985) with a LR of 45 and LR =20 for those times and altitudes in which clouds were detected.

- While details of experimental setup are given in reference [18], it would still be worthwhile to state the operating conditions of the device, in particular the settings of the Licel transient recorders, the firing rate of the laser, etc.

Thanks, we give a bit more detail on our system (text in blue) as also the third reviewer had a similar remark

- From Jan 9 – April 26 2020 campaign, (107 days) only 2954 time steps (23.6 days) of data were taken, which is roughly 1/5. Why? Is this because of adverse weather conditions in

4/5 of the time? Was the 2954 time steps data further reduced due to “cloud mask”, etc? More details on data handling should be given.

The cloud cover over Spitsbergen (North Atlantic!) is even in spring higher than 65% (Maturilli, Ebell 2018 – not mentioned in the paper but in the data set described therein). Most of these clouds are low (around 800m) liquid or mixed phase clouds too thick for lidars to operate). Further, due to local regulations (specific for Ny-Ålesund) we have to provide an operator in the observatory whenever the lidar is running even if the system could technically be controlled via internet. Finally, we are not allowed to switch on the instrument whenever air traffic is foreseen on the local air field. Hence, we normally do not have more than 10% data coverage at best. Due to the fact that we had an additional engineer on site and slightly reduced air traffic due to Covid, we obtained our 20% data coverage for the data of this paper which is very good for us. We can hardly do better. Now we believe that these specific conditions are not interesting for the community so we suggest to simply add: (new text in blue)

Overall 2954 time steps between 09 January and 26 April were analysed, which roughly correspond to 1/5 of data coverage. Due to high cloud cover in the European Arctic and safety regulations of the local air field this is already a very good data availability for our station. Further, we applied

- In section 4 (case studies), the authors go into details about the retrieved values for specific cases, but these details can not really explain the atmospheric processes at hand at that time (and give rise to speculations, e.g. lines 200-223). I would suggest a more concise reporting on the results and more concrete conclusions in the final section, separated from results.

OK Thanks. We summarized and sharpened our conclusions. See the new summary in blue

- The main conclusions in section 5 are:
    - Higher backscatter in 2020 than in 2019, offering no clear explanation as to why. The authors guess at hygroscopic growth and at the same time state the absence of “continental” (dust?) aerosols. Which type of aerosols experienced the growth then? Maritime? Why?

Thanks, this is again a good remark. Due to our typically low to moderate lidar ratios and the low to moderate refractive index in our inversions (and the low polarization) our results are consistent to the fact, that nss-sulphate and local marine aerosol dominate the Arctic spring time (at least on the ground) this is e.g. in our quote of Udisti (No 9). So, not surprisingly we have seen in the

column in general terms what could have been expected from ground-based in situ stations. We will point this more prominently.

- Decrease of the mean of particle size distribution with altitude is not unexpected, it was observed and documented under many different conditions (even in the investigation of volcanic plume). Please clarify why is such an observation meaningful in your case.

We agree to you. We do not want to stress too much on the fact that the small aerosol is above. The new, interesting facts of this study are the large variation of the particles below 1.5km, which are at least partly related by local boundary layer interactions (and hence, potentially different above Svalbard compared to the undisturbed Arctic). Secondly, we have the feeling that there is only very little temporal fluctuation in the aerosol microphysics above 2km. Now our data is limited, but this is worth mentioning. Because if this could be confirmed for other sites it would mean that it should be fairly easy to describe aerosol properties in a regional climate model in the Arctic for the free troposphere.

- The authors are not consistent with citations, as they sometimes cite papers in brackets only (e.g. [33] on line 230), and at other times by adding the name of the first author (e.g. lines 35-36). This is in my opinion not needed, the details on the authors are available in the references. Above all, I advise the authors to be consistent and uniform with citations.

OK Thank you! We hope to be consistent now!