Reviewer 1

In my opinion the revised version of the manuscript "Lidar derived Aerosol properties from Ny-Alesund, Svalbard during the MOSAiC spring 2020" by J. Dube, C. Bochmann and C. Ritter suffers from the same main problems characterizing the first submitted version.

This is simply not true. We answered all your comments from the first round.

Even if some of the improvements I suggested have been considered by the authors some others (listed below and in my opinion the most important ones!) have been not addressed from practical point of view.

Find our remarks in red and the new text in the manuscript in blue, with exception of the new Appendix C (black normal text) for readability

Lack of estimation of statistical uncertainties on all presented aerosol optical properties

Concerning this point the authors state that statistical uncertainties are not reported because the tool they have been used to process lidar measurements does support such calculation. Frankly I don't think this can be considered a valid reason to exclude such an important aspect of the data analysis.

Our tool to invert lidar data is state if the art. You surely do know that.

In my opinion this is valid in general and not only for this work. So I strongly recommend the authors to implement the calculation of statistical uncertainties in the tool they use to process their lidar data. As I have already mentioned in my previous review, for any measurements (direct or indirect), it's fundamental to provide the corresponding error bars! Especially in this case where the procedures to implement are well documented in literature.

We have included a new appendix with such an error analysis based of one case study. More we cannot and will not do here. The topic you mention is surely relevant but this is a project on its own.

Despite this lack in their software analysis, the authors have included in the table 3 a sort of error bars on optical properties saying that these represent the largest possible errors without providing any additional information (how these errors have been estimated? which statistics has been assumed?).

Simply error propagation (now explicitly stated in the new version). E.g. the error of extinction depends on noise in the lidar signal, deviations between assumed and real air density profile or choice of wavelength dependence.

Moreover, they also state that this error estimation (whatever it comes from) has been not included in the calculation of uncertainties on microphysical properties (which consequently are underestimated).

We already stated that it is. (Now line 198)

Frankly, I see this procedure very confusing and also not fully correct from methodological point of view.

It is absolutely not confusing. We have described all steps in the evaluation.

Results not presented properly

In my previous review, I suggested to provide more information (which in principle should be available to the authors according to the described lidar specs) beside the backscatter at 532, the LR at 355 and the depol at 532 when they compare the observations during 2020 against the ones during 2019. In this way I would be possible to discuss in more complete and detailed way the differences or the similarities with respect to 2019. The corresponding part of the manuscript was not modified at all by the authors and moreover they did not provide any motivation for which only some of the properties they measured have been taken into account.

We are showing in this work all quantities which are necessary to compare the 2020 to the 2019 data. This is because Rader et al. concentrated on the LR at 355nm because the LR532 is noisier. Moreover, while there are small changes in the depol between 355 and 532, we have still mainly spherical particles and the depol is also uninterestingly low at 355. Hence it would not make much sense to show it here. We repeat that all data is available from the authors for further analysis.

Moreover, I suggested to provide the 3+2+2 lidar analysis (in terms of vertical profiles) from which the optical properties reported in table 3 are derived. The authors added two figures (5 and 6) in which they show lidar profiles at different wavelengths which is something not very useful (basically looking at these figures is not possible to draw any relevant conclusion concerning the atmospheric conditions occurring during the selected cases) and very different from what I suggested to add. As 3+2+2 lidar analysis I meant a set of plots in which the integrated (in the same time windows the quantities in table 3 refer to) 3 backscatters, 2 extinctions and 2 particle/volume depolarization (and all the corresponding intensive properties like LRs and angstrom exponents) are reported as vertical profiles (possibly at altitude a bit higher than 3.5km).

We have exchanged the “old new figure 5 &6 by the required ones

According to the above substantial problems, in my opinion the revised manuscript is still far to be in a form suitable for acceptance.