We thank Dr. Yang for the helpful comemnts and address the comments below.

Reply to short comments by Dr. Yang

General comments: This manuscript reports a GEOS-chem model study of sea-ice soured SSA (from both blowing snow and frost flowers) and their impacts on polar aerosol extinction. Numerous model results via changing various parameters are per- formed and compared to remote sensing (CALIPSO) data. Some results are quite interesting, adding novel information to our knowledge regarding polar SSA production. Authors even derive an 'optimized' seasonal trend of snow salinity. Due to the lack of year around blowing snow and snowpack salinity measurements on polar sea ice surface, we almost know nothing about seasonal variation regarding snow salinity. For this reason, I will treat their modelling-based seasonal snow salinity as a weakness. Instead, I think it highlight an issue which is largely unknown to our knowledge. As we know snow salinity is one of the critical factor that could determine both salt mass loading and their airborne budget (via affecting size spectrum and then lifetime). Therefore, it is a quite important to investigate this parameter in a modelling study, though it needs justification as reviewers pointed out. In general, this is well written manuscript with some interesting results presented. It fits well the scope of the 'Atmospheric Chemistry and Physics' and will benefit relevant communities in sea ice, ice core and boundary layer chemistry. Thus, I support publication of this work in ACP after a revision (see below my specific comments).

Specific comments: The STD+Snow model run overestimates satellite extinction coefficients. Authors attribute this overestimation to 'higher' snow salinity applied in their model. However, I notice that the salinity levels of 0.1 psu for the Arctic and 0.03 psu for the Antarctic sea ice is not 'very' high comparing to the observation. For example, the 0.03 psu for the SH is only about half of the 'median' surface snow salinity (0.06 psu) and ~1/30 of the 'mean' snow salinity (=0.9 psu) observed in the Weddell Sea SIZ (see information in Rhodes et al. 2017). It seems to me the overestimation of SSA by the model could be related to one 'missed' process by the model, namely the negative feedback of sublimated water vapour to the ambient air near surface layer, which prevents further evaporation from suspended blown snow particles in the BS layer [Mann et al. 2000]. Thus, it is likely that model (like GOES-chem) without this process could result in overestimated bulk sublimation and then SSA production. I will not blame them not considering this process in their model, as it is out of the range of this study, but it would be useful if some discussions can be made.

Mann, G. W., Anderson, P. S., and Mobbs, S. D.: Profile measurements of blowing snow at Halley, Antarctica, J. Geophys. Res., 105, 24,491–24,508, 2000.

Another factor that could be responsible for the overestimation may come from one assumption made in this model set-up. According to their previous model study (Huang and Jaegle 2016), they assumed that one wind-blown particles will generate 5 sub- SSA, instead of one as assumed in the original parameterization by Yang et al. (2008). Is this term making some differences? It would be helpful if some discussions can be made as a model sensitivity study.

Yang, X., Pyle, J. A., and Cox, R. A.: Sea salt aerosol production and bromine release: Role of snow on sea ice, Geophys. Res. Lett., 35 (L16815), doi:10.1029/2008gl034536, 2008.

Indeed, our derived seasonal varying surface snow salinity is based on the hypothesis that the discrepancies between CALIOP and the STD+Snow simulation in the magnitude and seasonal cycle of aerosol extinction coefficients are due to our simplifying assumption of a uniform surface snow salinity over sea ice. We cannot rule out the alternative explanations that the overestimate of Antarctic aerosol extinctions is caused by the fact that our simulation does not include the negative feedback of water vapor sublimation or by our assumption of number of particle produced per snowflake (N).

We mention these possibilities in the revised manuscript. To address Dr. Yang's concerns, we have conducted a sensitivity blowing snow simulation assuming N=1. We have included the following discussion in the revised manuscript:

"It is also possible that the discrepancies between observed and modeled aerosol extinction coefficients are due to other factors in the blowing snow parameterization as implemented in GEOS-Chem. For example, our simulation does not include the negative feedback of water vapor sublimation (Mann et al., 2000): as blowing snow particles sublime in unsaturated air, they cause an increase in water vapor and thus cooling of the surrounding air. Both effects lead to an increase in RH near saturation, reducing the sublimation rate. Another underlying assumption is that 5 SSA are produced for each snowflake that sublimes (N=5). We conducted a sensitivity simulation assuming one SSA per snowflake, shown as STD+Snow (N=1) in the supplement (Fig. S7 and S8). This change does not affect the total emissions of blowing snow SSA, but decreases the fraction of SSA in the accumulation mode (see Huang and Jaeglé, 2017). As the extinction efficiency of accumulation mode SSA is larger than that of coarse mode SSA, the assumption of N=1 leads to a 30–50% decrease in modeled extinctions relative to the STD+Snow (N=5) simulation. Overall, this results in improved agreement with CALIOP observations over Antarctic sea ice, but the CALIOP aerosol extinctions are underestimated over the Arctic. Increasing the surface snow salinity over Arctic FYI can address the model discrepancy in aerosol extinction coefficients, but will lead to a factor of 1.5–2 overestimate in SSA mass concentrations."

Tiny comments: Figure 1, colour bar needs to be improved. It is hard to distinguish the extinguish coefficient values between ~10 and ~20 Mm-1 in the upper panel, and between ~5 and ~10 Mm-1 in the bottom panel of figure 1. A similar problem also appeared in other plots. P10 line 4 and Figure 8: longitude/latitude ranges are mentioned, but not shown in in the corresponding plot. Major longitude/latitude information should be given in all relevant figures.

We have changed the colorbar in Fig. 1 and 4. We have also included the major longitudes and latitude in the relevant figures.