

We would like to thank referee #2 for helpful review and suggestions.

Reply to comments by Anonymous Referee #2

This is a well written manuscript describing modeling of Arctic aerosol and comparison of these models to observations from CALIOP satellite lidar observations. The model, GEOS-Chem, uses various parameterizations of aerosol production mechanisms, and addition of a blowing snow mechanism brings the model closer to observations. The blowing snow model is further refined by varying the surface snow salinity to improve agreement with observations. An example of an event of blowing snow is shown.

Overall, I feel that this is a well written manuscript, but that the identification of model modifications with specific physical processes sometimes goes further than is justified and/or alternative hypotheses have not been explored fully. The CALIOP data indicate that there is larger extinction present near the surface than the model would indicate, so a wind speed and snow salinity dependent blowing snow model is added, increasing the modeled aerosol extinction, which brings it closer to observations. However, one needs to consider how definitive the identification of these model variables is with physical processes. Specific questions in this regard are:

1) After adding "blowing snow", the model is tuned to reduce surface snow salinity in MYI areas as compared to FYI areas and over the wintertime season. How robust is the necessity to tune down the salinity? For example, Figure 3 shows distributions of extinction in FYI, MYI, and CAA areas. Visually, I can barely see any difference between the CALIOP observations in panels g, h, and i. Values are about 15 Mm^{-1} from Jan-Apr, low in summer, and increase back to 15 Mm^{-1} towards the end of the year. Is there any statistical difference between these monthly observational distributions? Given the lack of difference between these locations, it seems like the need to optimize the model is weak. Specific monthly values are listed, but it doesn't seem like there is enough information to actually map out this amount of information. For example, could a different single fixed value of salinity be used to optimize the model similarly? It is not unreasonable that surface snow salinity would decrease as you add new snow (which is of low salinity), but the question is how strong the modeling evidence for this decrease is. Please show that the trend from the "optimization" is a real effect larger than statistical errors.

We agree with this reviewer that the need for an optimized time-dependent snow salinity was not very clear in our original manuscript. This was because we made changes with opposing effects partially cancelling each other out, especially for the Arctic: reducing the salinity of MYI and applying a monthly-varying optimized salinity for FYI based on CALIOP extinction observations. As described in the introduction of our manuscript, the lower salinity of MYI and snow on MYI has been clearly demonstrated by observations and is well accepted. In the revised manuscript our STD+Snow simulation now includes this more realistic assumption by lowering the salinity of snow on MYI to 0.01 psu in the Arctic and 0.003 psu in the Antarctic. The new STD+Opt. Snow has the same salinity for MYI as the STD+Snow simulation and uses the fit to CALIOP extinctions to optimize salinity. The impact of the changing snow salinity can be more clearly seen in the revised Figure 3 for FYI (panel g), especially for October-December when the higher salinities on young sea ice (0.36-0.19 psu) lead to a near doubling in aerosol extinction,

consistent with CALIOP. We also perform Student's t-test for each cold month and find that significance between these two models' bias are smaller than 0.05 on both FYI and MYI, except for Antarctic MYI in May, which indicates that STD+Snow and STD+Opt. Snow are statistically significantly different. Applying a single scaling factor for the salinity of surface snow on FYI does not address this model discrepancy over the Arctic. This is now made clearer in the revised manuscript.

“We also examined whether a single fixed value of salinity over FYI can lead to similar improvements in the agreement with CALIOP. The resulting fixed salinities are 0.11 psu over Arctic FYI and 0.018 psu over Antarctic FYI, leading to good overall agreement with CALIOP over the Antarctic (NMB of +5% on FYI and -9% on MYI) with no significant improvement seen in the Arctic (NMB of -7% on FYI and -18% on MYI). We found that over the Arctic, a simulation using a single salinity of 0.11 psu (STD+Const. Snow, Fig. S8g-h) yields results similar to the STD+Snow simulation and cannot explain the high extinction values during fall/early winter. Over Antarctic sea ice, the performance of a simulation with 0.018 psu over FYI shows results similar to the STD+Opt. Snow simulation. Thus there is a stronger case for using a seasonally varying snow salinity over Arctic sea ice than over Antarctic sea ice. We speculate that this might be linked to relatively smaller seasonal variation in sea ice thickness and snow depth for Antarctic sea ice compared to the Arctic. In their snow climatology, Warren et al. (1999) report that the mean snow depth at an Arctic sea ice site increased from 8.7 cm in October to 28.9 cm in March. Satellite-based observations of Arctic FYI thickness show an increase from 0.95 m in October to 2.15 m in May (Kwok and Cunningham, 2015). In contrast, over Antarctic sea ice the mean sea ice thickness and snow depth remained fairly constant during fall-winter (April: 0.48 m for ice thickness and 0.11 m for snow depth; August: 0.52 m for ice thickness and 0.11 m for snow depth) as described in Worby et al. (1998).”

2) Open water areas can produce aerosol directly (by wind blowing over the exposed sea water) or via re-freezing, which might produce frost flowers and/or simply provide a non-snow-covered highly saline surface that snow could blow onto/across. The manuscript does not do justice to hypotheses other than frost flowers. It should leave open the possibility that open water or thin snow cover on ice could be responsible. For instance, the citation below indicates that open water is a source of sea salt aerosol.

May, N. W., P. K. Quinn, S. M. McNamara, and K. A. Pratt (2016), Multiyear study of the dependence of sea salt aerosol on wind speed and sea ice conditions in the coastal Arctic, *J. Geophys. Res. Atmos.*, 121, 9208–9219, doi: 10.1002/2016JD025273.

We now mention this source from leads in the revised manuscript both in the Introduction and in Section 2.3:

“Over polar regions, SSA can also be generated via sublimation of saline blowing snow (Simpson et al., 2007; Yang et al., 2008), wind-blown frost flower crystals (Rankin et al. 2000; Domine et al., 2004; Xu et al. 2013), and by leads in sea ice (Nilsson et al., 2001; May et al, 2016).”

“In this study, we neglect the role of leads as a source of SSA as we found in Huang and Jaeglé (2017) that while this additional source could potentially be important on local scales near leads, overall the regional increase in SSA emissions is less than 10%.”

Another aspect that may affect the ability to model either open water areas or frost flowers is the low spatial resolution (2 x 2.5 degree) of sea ice in the model and also the use of a weekly product (Page 5, line 30) for sea ice concentration. This low time resolution and linear interpolation could affect the ability of the model to represent the small spatial scale (few km) and temporally transient sea ice lead features.

Point well taken. Indeed very small and temporary features such as leads might not be well resolved in the MERRA sea ice fields (based on the 12.5 km resolution observations from the SSMI instruments on DMSP satellites), which is why we do not consider this source in our manuscript. As noted above, this is now explicitly mentioned in the revised manuscript.

3) The Canadian Archipelago is a region where there is a great deal of land near sea ice. The land can affect the ability of passive microwave satellites to detect sea ice concentrations (called land contamination), and thus could affect the ability to predict frost flower presence. Also, surface winds in the presence of significant topography might not be modeled well at these coarse spatial resolutions. Therefore, I think that there may be a number of factors in this region and caution against overinterpretation. For example, page 9, line 7 indicates a surface snow salinity of 3 psu (nearly 10% of that of sea water) could reconcile differences. Also, it is stated that Alert is near frost-flower producing regions. I think of Alert being in a MYI area, largely surrounded by older sea ice that builds over years. Please cite sources to indicate evidence for Alert (and Neumayer) being in a frost-flower producing area.

The MERRA sea ice component is based on observations from Special Sensor Microwave Imager (SSM/I) instruments on Defense Meteorological Satellite Program (DMSP) satellites, which have a native resolution of 12.5km x 12.5km. The proximity of coastal ocean grid to land, can indeed lead to false ice concentration signals, and the land contamination errors are quantified in Maslanik et al. (1996). The land contamination errors are relatively small in SSM/I, compared to Scanning Multichannel Microwave Radiometer (SSM/R) due to higher resolutions (Maslanik et al., 1996). In addition, land masks are used to alleviate the impact of land contamination (Maslanik et al., 1996). Intercomparison of SSM/I sea ice concentrations with ship-based observations and independent satellite products are discussed in Kaleschke et al. (2001), Kern et al. (2003) and Andersen et al. (2007). Overall, the SSM/I with ARTIST Sea Ice algorithm (ASI) algorithm yields a reasonable representation of sea ice concentrations.

As noted by this reviewer, Alert is in proximity to MYI region. Our frost flower model predicts the most active frost flower emission region to be over the Canadian Arctic Archipelago, and Alert is closest to this region among the three Arctic sites. Therefore, Alert may receive large influence from frost flowers compared to other Arctic sites. We have changed the wording in the revised manuscript to clarify this point.

Andersen, S., Tonboe, R., Kaleschke, L., Heygster, G., and Pedersen, L. T.: Intercomparison of passive microwave sea ice concentration retrievals over the high-concentration Arctic sea ice, *J. Geophys. Res.*, 112, C08004, doi:10.1029/2006JC003543, 2007.

Kaleschke, L., Lüpkes, C., Vihma, T., Haarpaintner, J., Bochert, A., Hartmann, J. and Heygster, G.: SSM/I sea ice remote sensing for mesoscale ocean-atmosphere interaction analysis. *Canadian Journal of Remote Sensing*, 27(5), pp.526-537, 2001.

Kern, S., Kaleschke, L. and Clausi, D.A.: A comparison of two 85-GHz SSM/I ice concentration algorithms with AVHRR and ERS-2 SAR imagery. *IEEE Transactions on Geoscience and Remote Sensing*, 41(10), pp.2294-2306, 2003.

Maslanik, J.A., Serreze, M.C. and Barry, R.G.: Recent decreases in Arctic summer ice cover and linkages to atmospheric circulation anomalies. *Geophysical Research Letters*, 23(13), pp.1677-1680, 1996.

Minor comments:

Page 2, line 20. This sentence is somewhat confusing with respect to what surface is being discussed. Is the top of the newly forming first year ice's salinity being discussed? If so, please clarify that this is the ice surface rather than snow.

Yes, this has been clarified in the revised manuscript.

Page 3, line 3. There is no discussion of open water as a sea salt source.

The role of leads has been included in the introduction of the revised manuscript as part of our response to comment #2.

Page 3, line 27. The wording of "aerosol extinctions and the layers beneath" maybe could be improved.

We have changed the wording in the manuscript.

Page 4, line 20. I think it should be "...with a 1-year..."

This was modified in the revised manuscript.

Page 6, line 26. The wording of "reducing the bias" maybe could be improved (the bias became larger, not smaller, but closer in magnitude to zero).

This has been improved in the manuscript.

Overall, I feel that this manuscript argues well for the need to add a wintertime sea salt aerosol source to the Arctic and this source seems to be effectively modeled by a blowing snow model, but that some further refinements of this model may not be appropriately linked to physical processes (e.g. surface snow salinity changes and frost flowers). Those aspects of the manuscript should be further defended by statistical methods or should be written in a more cautious manner, including alternate hypotheses that seem consistent with the data.