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Interactive comment on "Using CALIOP to constrain blowing snow emissions of sea salt aerosols over Arctic and Antarctic sea ice" *by* Jiayue Huang et al.

Anonymous Referee #2

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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

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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 Mmˆ-1 from Jan-Apr, low in summer, and increase back to 15 Mmˆ-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.

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

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Arctic, J. Geophys. Res. Atmos., 121, 9208–9219, doi: 10.1002/2016JD025273.

Another aspect that may affect the ability to model either open water areas of frost flowers is the low spatial resolution (2 x 2.5 degree) of sea ice in the model and also the use 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.

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 significant topography might not be modeled well at these course 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 frost-flower producing area.

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.

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

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

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

Page 6, line 26. The wording of "reducing the bias" maybe could be improved (the bias

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became larger, not smaller, but closer in magnitude to zero).

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.

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