

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

### **Reply to comments by Anonymous Referee #2**

This is a well-written paper that explores in a model the relative influence of three sources of sea salt aerosol in the polar regions. Although the blowing snow source has been explored in a model with a similar parameterisation in another model, this is the first time that all 3 sources have been tested in a similar setup. The paper compares model output with aerosol data at a number of polar sites, with rather impressive results in terms of concentration, seasonality and episodicity. It is able to conclude on the importance of the blowing snow source in wintertime, and provides reasonable evidence to dismiss the frost flower source as a significant player in most circumstances. Overall, it is a good paper, clear and well-argued, and certainly worth publishing in ACP. It provides a basis for exploring other aspects of the influence of different sources of sea salt aerosol. My only substantial quibble with the authors is that set store by the fact that they get the concentrations right and that they are testing the balance between the sources. However this ignores the fact that they have had to take several decisions (such as the number of salt particles per snowflake, the salinity of the snow, and the scaling factor (Page 5, line 29), which are essentially tunings (ie they chose them in order to match the data). I think the paper should be a little clearer in recognising this, and in admitting that the relative strength of the different sources is influenced by this rather strongly.

- We have added more discussion of our assumptions in the revised text and have recognized more clearly how these assumptions impact our conclusions. In particular, we have added the following in our conclusions:

“The SSA parameterizations for blowing snow and frost flowers have several intrinsic assumptions, such as the salinity of snow and the scaling factor for frost flowers, which influence the relative magnitudes of these two sources in polar regions. The geographic distribution, seasonal cycle, and daily variability of these sources, however, is controlled by sea ice extent and meteorological parameters (winds and temperature). In this study, we showed that the temporal and geographical variability of SSA observations at five polar sites is more consistent with blowing snow than with frost flowers. Based on this comparison, we conclude that blowing snow is likely to be the dominant source of SSA in polar winter, although frost flowers cannot be entirely ruled out. In particular, they may contribute indirectly to SSA emissions by salinating wind-blown snow (Obbard et al., 2009).”

#### Detailed comments

Page 1, line 18. Here and elsewhere in the paper the authors refer to submicron aerosol, meaning the range which elsewhere they describe as the 0.01-0.50  $\mu\text{m}$  radius range. It would be helpful if they would clearly state this usage, perhaps on page 4, line 17, where after describing the accumulation mode they could add “which we refer to as sub-micron based on its diameter”.

- This has been clarified in the manuscript:

“For this work, we track SSA mass in two size bins: accumulation mode ( $r_{\text{dry}} = 0.01\text{--}0.5\ \mu\text{m}$ ) and coarse mode ( $r_{\text{dry}} = 0.5\text{--}4\ \mu\text{m}$ ), except in the comparison to in situ mass concentrations of SSA for which we use  $r_{\text{dry}} = 0.01\text{--}0.3\ \mu\text{m}$  and  $r_{\text{dry}} = 0.3\text{--}3\ \mu\text{m}$  (see section 2.3). In the rest of the manuscript we will refer to the accumulation and coarse mode SSA aerosol as submicron and supermicron SSA based on their diameters.”

Page 4, mid. I don't quite understand the description that for the blowing snow they treat just two size ranges. My understanding was that the Yang parameterisation that they are following uses many more size bins than that, and relies on this for many of its characteristics. Could the authors explain what they mean? Presumably they maintain different sizes in calculating the mass flux with respect to the number of snowflakes, so in what respect do they not use different size ranges and what are the likely impacts?

- We use the same size distribution for snow particles as reported in Yang et al. (2008), but use a different salinity and number of SSA particle per snowflake. As we track SSA mass in GEOS-Chem in two size bins, we integrate this size distribution and calculate the corresponding SSA emissions for the two size bins.

Page 5, line 5-6. I don't understand at all why they choose a lower salinity in the Antarctic, or why the higher Antarctic precipitation is relevant to that. It reads as if they think the snowfall somehow dilutes the salinity but this of course makes no sense as the salinity is more likely a function of snowpack thickness, which is likely lower in Antarctica. Please explain –at the moment this just looks like a correction factor chosen at random.

-This point was not clearly described in the original manuscript and we have clarified this in the revised version. Yes indeed, it is the snow thickness that controls the salinity of the snow on sea ice. Antarctic is surrounded by ocean, so precipitation occurs more frequently over Antarctic sea ice. As a result, snowpack tends to be thick. The Arctic Ocean is surrounded by land and precipitation is relatively rare, hence the snowpack is thinner on Arctic sea ice.

“Snowpack on Antarctic sea ice is thicker than over Arctic sea ice. Indeed, Antarctica is surrounded by the Southern Ocean which brings moisture, while the Arctic is surrounded by land with wintertime precipitation that is 3 times lower than over Antarctic sea ice (Huffman et al., 2001). As the salinity of snow decreases with snowpack thickness, we assume that the salinity of snow on Antarctic sea ice is 0.03 psu, a factor of 3 lower than over the Arctic following Yang et al. (2010).”

Page 5, line 28. I think you mean “geometric mean diameter” and “geometric standard deviation”. Geometric diameter doesn't seem meaningful.

- Yes, this was a mistake. We have corrected this in the revised manuscript.

Page 7, line 8. Please explain what M and O are; as written your explanation of what NMB is is unclear. I assume it's the percentage mismatch between model and data – why not call it model-data mismatch? The word “bias” seems wrong when you are simply comparing alternative partial sources to the data.

- The normalized mean bias is often used as a metric for model evaluation against observations. As pointed out by the referee, our notation was unclear so we have changed the equation to the following: “ $(NMB = (\overline{Model}/\overline{Obs} - 1) \times 100)$ ”.

Page 9, line 22-25. I assume that you are saying that, for the same size and place the lifetime is the same whatever the source (this must be true), but that the lifetime is longer because the blowing snow sourced aerosol tends to form when it's colder. It might be clearer if you explain it more in this way.

- Yes, the referee is correct. We have changed the wording in the revised manuscript to clarify this point:  
“Open ocean SSA form over lower latitude warmer regions, while sea ice SSA emissions occur at higher latitudes under much colder conditions, with less efficient removal processes in mixed-phased and ice clouds. The current parameterization in GEOS-Chem assumes that in-cloud scavenging of SSA does not occur in cold clouds ( $T < 258$  K) (Wang et al., 2011), thus wintertime sea-ice generated SSA are only removed by below-cloud scavenging (which is slow for accumulation mode aerosols) and dry deposition.”

Page 9, line 24. Just a question from my ignorance: wouldn't we expect sea salt to become an efficient ice nucleus below its eutectic (ie about 250K), when it would become a solid?

- The statement in the manuscript refers to what is currently assumed in GEOS-Chem. Recent laboratory studies have shown that sea salt can act as an ice nuclei by deposition freezing or immersion freezing. This has been added to the revised manuscript:  
“The current parameterization in GEOS-Chem assumes that in-cloud scavenging of SSA does not occur in cold clouds ( $T < 258$  K) (Wang et al., 2011), thus wintertime sea-ice generated SSA are only removed by below-cloud scavenging (which is slow for accumulation mode aerosols) and dry deposition. Recent laboratory studies have shown that SSA could act as ice nuclei by deposition freezing (Wise et al., 2012) and immersion freezing (DeMott et al., 2016), and might thus undergo in-cloud scavenging in mixed and ice clouds. This process is not currently included in GEOS-Chem. “

Fig 1 caption. Please explain here as well what NMB is: the reader should not have to read a quite difficult bit of text to understand the figure.

-We have made the change in the manuscript.

Fig 3 caption. I struggled to understand the text about the coloured circles. Do you mean that the larger circles (which are anyway hard to see) represent the ship's position between 15 and 19 April. If so, why not say this. If not, it needs a new explanation.

-The large circles indicate the locations of the ship between 15 and 19 April, and the color of the circle indicates the SSA mass concentrations observed on ICEALOT. We have added the clarification in the caption:

“The larger circles near Svalbard correspond to the location of the ship on 15-19 April, and they are color-coded based on observed SSA mass concentrations (same color scale as the model).”

Fig 4 and 5 caption, just for clarity please add “submicron” in the phrase “spatial distributions of wintertime submicron SSA”.

-We have made the change in the manuscript.

Supplement, section 1. Much of this text duplicates what is already written on page 4, para 1.

-We have removed the duplicated text in the Supplement.

## Reference

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