

## ***Interactive comment on “Sea ice as a source of sea salt aerosol to Greenland ice cores: a model-based study” by Rachael H. Rhodes et al.***

### **Anonymous Referee #1**

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This manuscript uses the p-TOMCAT CTM to examine the role of sea ice sources of sea salt on atmospheric concentrations of Na<sup>+</sup> in the Arctic and in ice core measurements in Greenland. The authors propose that the model including sea ice sources can reproduce the winter maximum of Na<sup>+</sup> in the atmosphere and in ice core records. Furthermore, the authors show that the model reproduces some of the interannual variability at Summit, which could provide a way to use Na<sup>+</sup> concentrations in ice cores as a proxy for sea ice extent.

This study builds on previous work by the authors in developing a parameterization for blowing snow and applying it to examine halogen activation as well as sea salt emissions over polar regions (Yang et al., 2008; Levine et al., 2014; Legrand et al., 2016). The authors further refine their assumptions on snow salinity and snow age.

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### General Comments

I have 3 main areas of concern regarding this study. The first is that the description of the different tuning parameters is confusing in the manuscript. In particular, after the sensitivities studies it is unclear what the final choice is for the standard simulation used in the figures. The second area of concern is that many parameters are changed compared to the 3 previous simulations using p-TOMCAT (Yang et al., 2010; Levine et al., 2014; Legrand et al., 2016). It appears that depending on the problem at hand (Arctic vs Antarctic sea salt; sea salt vs bromine), different parameters are tuned to different values. This again tends to be confusing. It would be very useful to the reader to get a sense of what the different assumptions were in the different studies and how they affect total emissions of SISS. Furthermore, OOSS formulations also seem to be different in each of these papers. Finally, the comparison between Greenland ice core measurements and the model show that the open ocean source of sea salt is sufficient to explain the observed seasonal cycle at Na<sup>+</sup>, thus the authors' conclusion that Na<sup>+</sup> concentrations in ice cores is influenced by sea ice sources is not supported by their comparison.

These comments and other specific comments are detailed in the next sections.

### Specific Comments

1) Snow salinity. After reading section 2.3.2, it is unclear to me what salinity is used for Arctic snow on sea ice. The authors mention the BLOWSEA project with 0.3 psu for Antarctic snow salinity. Is that the value used in the standard simulation shown in Figure 3? In section 3.3.1 (page 8), the authors mention a sensitivity simulation with 2-fold and 3-fold salinity. What is that with respect to? 0.3 psu? This is confusing, and it would be clearer to directly specify the actual numerical value of the salinity used. Is the 2-fold salinity 0.6 psu and 3-fold salinity 0.9 psu? Which one is used in Figure 4? I suggest that the author discuss the different salinities used in section 2.3.2 and then refer to them in the sensitivity studies

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2) Sea salt emissions. Can the authors compare their emissions (in TgNa/yr) for both OOSS and SISS to Huang and Jaegle (2017)?

3) This is the fourth paper using the Yang et al. (2008) blowing snow parameterization in P-TOMCAT (Yang et al., 2010; Levine et al., 2014; Legrand et al., 2016). In each of these papers different assumptions are made in terms of OOSS source functions, as well as blowing snow parameters (salinity, snow age, gustiness, etc...). It would be useful to discuss the overall impact of these different assumptions on emissions. In particular, I suggest adding a table that lists Arctic and Antarctic emissions for Na for both OOSS and SISS (this could be used to address my comment 2) above). This table should also include mean surface concentrations or tropospheric burdens of Na.

4) Snow age. Page 5, line 22. The choice of 24 hour snow age seems arbitrary, especially as a previous study with the same model used a snow age of 5 days. A better justification of this value would be to use the meteorological fields to infer a mean time between snow precipitation over the Arctic.

5) Comparison to atmospheric observations (Figure 3). The observations at the different sites are for different time periods (Alert: 1990-1995; Summit: 2003-2006; Barrow: 1997-2000; Zeppelin: 1993-1999; Villum: 2001, 2002, 2008-2013) but the model simulation is the average for 1991-1999, which in the case of the Greenland sites doesn't overlap with the observations. For the other sites, there is some overlap, but the model years are not selected to match the observation years. Given the large interannual variability in Na observations (and in the simulations) can the authors justify this approach? I suggest that at a minimum the authors select the model years that match the observations for Alert, Barrow, Zeppelin. Extending their simulation by a few years would also allow them to have a more rigorous comparison to the Greenland sites.

6) Section 3.3 and figure 4. The sensitivity studies shown in Figure 4 are conducted for a single year (1997), while the observations are for multiple years – at least this seems to be the case based on Figure S4. How representative is 1997 compared to

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the 1991-1999 simulations? At some sites, such as Villum (Figure S4) there appears to be significant differences between 1997 and the 1991-1999 average. Is panel A in Figure 4 for 1997 only or for 1991-1999 (corresponding to Figure 2)? Based on this single year simulation, my understanding that authors choose the option with multi-year sea ice emissions decreased by 50% (panel C) for subsequent simulations (page 9, line 15). The authors should justify this. If this is the simulation they choose, it should be the one they show in Figure 3. To clarify the assumptions for the various simulations, the authors should include a table in the supplementary material with the actual assumptions that are made. For example what salinity (over what sea ice) and snow age are used in Figure 4E?

7) Page 10 line 15. Do the authors have any potential explanations for why the observations at Barrow are reproduced by the SISS simulation during the first part of the year, but not the second part? Are the meteorological conditions (windspeed) not captured as well?

8) Seasonal variability of Na in ice cores. The authors compare the p-TOMCAT simulation to ice core observations over Greenland, finding that the model captures the observed seasonality with a winter maximum (section 4.3.2). Figure 5 shows that this seasonality is mostly due to the open ocean SS aerosol (dashed red line), with little influence from the sea ice SS sources. This is contrast to the open ocean (OOSS) simulation of atmospheric Na at ground sites in the Arctic (Figure 3). Can the authors explain the reason for this different modeled seasonality in the atmosphere and in ice cores for the OOSS simulation? Also the comparison between p-TOMCAT and ice core measurements is a little difficult to follow as different sites are shown in different figures. For example, Tunu is missing from figure 5, but is shown in Figure 7. I suggest that the authors add Tunu in Figure 5, especially as it appears that the modeled influence of sea ice sources might be large at this site.

9) Section 5. Based on the comparison shown in Figure 5, it seems that the sea ice sources do not really lead to a better simulation of the ice core measurements.

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At most sites the influence of sea ice sources is small. The largest modeled sea ice influence is at the NEEM site, where the model does not capture the observed seasonal cycle. Thus this comparison is inconclusive in terms of the role of a sea ice source in influencing ice core measurements.

#### Technical corrections

- Page 2 line 24: add “the” before “principal source of sea salt”
- Page 2 line 30. Please add a reference for these field-based observations in the Weddell Sea.
- Page 3 line 27. “age range” is a strange term. Do the authors mean “year of collection”?
- Page 8 line 20-22. This sentence is confusing. The Weddell sea salinity (0.3 psu) multiplied by two is 0.6 psu, while this sentence implies it is 0.12 psu. The Mundy observations of 0.1 psu of surface snow over the central Canadian Arctic thus imply that the salinity used by this study (0.6 psu?) is too large.
- Page 9 line 10. Can the authors be more specific about the region chosen to calculate these emissions?
- Page 15 line 16. “SISS contributes to the winter maxima observed in all the ice cores, but that in some cases, OOSS alone can produce winter maxima and summer minima in sea salt in ice cores” There is no evidence of this in the manuscript. Figure 5 shows that OOSS reproduces the observed seasonal cycle at all sites except for NEEM. At NEEM, adding the SISS source doesn’t lead to a better simulation.

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