

## Rhodes et al. reply to Anonymous Reviewer #2

We thank the reviewer for their comments and address each one directly below.

Rhodes et al. use a chemical transport model to examine the importance of the sea ice source of sea salt aerosol (SISS) relative to the ocean source of sea salt aerosol (OOSS) in the Arctic. They compare their model to observations of sea salt aerosol in the atmosphere and high resolution Na<sup>+</sup> measurements in Greenland ice cores. I found this paper very hard to follow and in the end it wasn't clear what was learned from their modeling exercise beyond what others have published.

As is stated several times in the manuscript (including the abstract and section 6 summary) this is the first study to utilise a chemical transport model to simulate the sea salt concentrations of snow, therefore enabling direct comparison with ice core records. Furthermore, we show that Na concentrations can be simulated to within a factor of 2. As alluded to in the abstract and detailed in the text, this is the first manuscript of a wider study into the viability of ice core sea salt as an ice core proxy. It will be followed by an investigation into the factors driving signal variability (pg. 15, line 23) and studies using paleoclimate boundary conditions (pg. 15, line 6). We trust that our manuscript will be easier to follow once the suggestions provided by reviewers are implemented.

Because the processes responsible for the emission of SISS into the atmosphere are not well understood, the authors "tune" their model to best match the aerosol observations. In the discussion of all of the different parameters that can be tuned, the manuscript would greatly benefit first from an explicit description of the parameterization for SISS (i.e., show the actual equations, and define all of the variables). Without it, it is very hard to follow the discussion of the model tuning.

We choose not to repeat the equations for SISS emission of Yang et al. (2008) because we do not introduce any new parameterisation or variables. We refer to specific equation numbers in the text so that the reader can refer back to Yang et al. (2008).

It seems however that some of the model tuning has to do with the treatment of aerosol deposition in the model, not just the SISS emission parameterization, the discussion of which is also confusing. The parameterisations of sea salt deposition have not been altered since Levine et al.'s (2014) work. What we describe in section 2.3.3 is how the amount of sea salt *deposited* at each time step is calculated. Previously, only the amount *remaining in the atmosphere* after deposition was calculated. We do this in order to make a direct comparison with ice core sea salt concentrations (rather than atmospheric aerosol concentrations) (pg. 6, line 9).

Please see our reply to Reviewer 1 for suggested changes that will make the SISS emission parameterisation easier to follow.

Not all of the terms in Equation 1 are defined. What is  $\alpha_C$  PCL and  $\alpha_N$  PNL? Is this somehow related to  $\alpha_C$ ,  $\alpha_N$ , PNL and PCL? It looks like there must be a mix up of subscripts and superscripts in either the equation or the text.

Yes, this is a typo mixing up subscripts and superscripts that will be rectified.

Does the model calculation of dry deposition include gravitational settling of the larger ( $r > 4 \mu\text{m}$ ) particles? If not, it should.

Yes, see Pg. 6 line 8.

The modeled wet deposition seems to be missing some important processes (Page 13). It's also not clear if the modified snow precipitation directly influences wet deposition, or if the modeled wet deposition uses the "incorrect" precipitation.

The wet deposition scheme does have limitations, as we acknowledge (Pg. 13, line 20). The wet deposition code uses the model-generated precipitation (black line on Fig. 6 B-D) and this can be stated clearly in text.

I think what is new about this manuscript is the comparison of the model with Greenland ice core Na<sup>+</sup>

observations. However, this is probably the most ambiguous part of the paper, and it's not clear to me what they learned from this exercise. They are comparing modeled versus observed seasonality, although it seems that the seasonality of ice core Na<sup>+</sup> is unclear as it was determined assuming constant snow accumulation rates, which is probably not consistent with reality. Also perhaps the seasonality is not well preserved in the observational record because of factors such as snow redistribution (page 14). The seasonality in ice core Na is significant in all the Greenland ice cores shown on Fig. 5 (see green lines and uncertainty bars that denote interannual variability). This seasonality is preserved even though processes such as snow redistribution have likely impacted the ice core records. What may be uncertain is the *monthly timing* of peak [Na] because when an ice core is dated by counting of annual layers in chemistry records [Na] is often assumed to peak Jan 1<sup>st</sup>. Or more accurately, the ratio of non-sea-salt sulphur (nssS) (mostly from sulphate) to Na is used and the minimum is dated as Jan 1<sup>st</sup>. The timing of the [Na] may therefore be artificially fixed as Jan 1<sup>st</sup>, when it could vary by few months either side. Support for the winter timing of peak [Na] comes from sea salt measurements of Arctic aerosol (Fig. 3) and fresh snow e.g., at Summit (Fig. S5). Figure S5 now includes nssS:Na measured in snow at Summit.

In the end it seems that the model shows little skill at simulating the observed seasonality of ice core Na+... The model simulates summer minima in [Na] and maxima in either winter or spring. This is similar to the observed ice core [Na] seasonality, especially when we bear in mind that the Na peaks may have been used as winter (Jan 1st) markers in ice core dating, thereby artificially fixing the timing of maximum [Na]. The model simulates the relative and absolute amplitudes of the seasonal cycle reasonably well (pg. 13, line 28 & Table 2).

The second paragraph of the summary (section 6) I think attempts to articulate what they learned from the model/ice-core observation comparison, but I still cannot figure out what was learned from this exercise. Given that this is the main new contribution of this paper, the paper should be substantially revised to better articulate their scientific contribution.

We thank the reviewer for their suggestion and will work on this in a revised manuscript.

Page 2 line 30: The last sentence of this paragraph needs a reference.

A publication discussing these results in detail is currently being prepared. Once published, the data set will be available online at the NERC Polar Data Centre. A DOI is currently being generated, where metadata will be made available. The DOI will be included in the revised paper so researchers can access the data easily in the future.

Page 5 Lines 16-17: Provide a justification for the choice of 0.3 psu.

0.3 psu is the mean of the salinity distribution of samples from the top 10 cm of the snow pack on sea ice in the Weddell Sea. This could be made clearer in the text.

Page 5 Line 21 and elsewhere: What does "snow age" mean? This should be defined. It's not clear how this should impact SISS.

The snow age parameter is defined in Box et al. [2004] and introduced by Yang et al. [2008] to represent the efficiency of aged snow in SSA production. The snow age parameter taken by Yang et al. (2008) to be the number of hours since the last snow event. We agree that it should be defined here too. The idea is that fresher snow is more easily lofted up because the individual grains/flakes haven't had time to sinter together. So, a higher snow age decreases the SISS emissions. Please also see reply to Reviewer #1.

Page 9 lines 8-9: How was scenario #3 parameterized? Did you simply reduce salinity by 50%?

In scenario #3 the area of multi-year sea ice in each grid cell used in SISS emission calculation is halved. This has the effect of halving the SISS emissions from the multi-year ice in that grid cell. It is not precisely the same as reducing the snow salinity by 50%. Sea ice area is only halved for this calculation, it does not result in additional area of open ocean. The text has been altered to clarify this.

Page 9 Line 17: Define NRMSD the first time used.

Defined on Pg. 7 line 14.

Page 11 line 1: Unfinished statement. What are you comparing the model simulations to?

This sentence will be re-worded.

Specify “snow accumulation” instead of just “accumulation” throughout the manuscript.

This can be done.

Page 14 line 24: What is a “Greenland ice core simulation”? Do you mean model simulation?

This sentence will be re-worded.

Page 15 line 25-26: Be sure to specify that this is for today’s climate. Perhaps it would be different in a different climate.

The sentence can be changed from “in the Late Holocene” to “under present day conditions”.

Figure S1 should be in the main text.

We disagree. Fig. S1 presents comparisons of aerosol Na measurements and simulations at low latitudes and is not integral to the manuscript or its conclusions.

When Figure 3 is presented in the text, it is not yet clear what your “base case” simulation is, which I think is what the blue line is in the figure. This information should be presented in order.

Yes, we agree, this is confusing and it will be changed. An additional small table will summarise the parameters used in the base simulation and this table will be referred to early on.

Figure 7: What are the yellow and other 3 green colors? The acronyms should be restated in the figure caption.

The caption of Figure 7 will be changed. The other colours are other ice core records located within the same grid square in p-TOMCAT.

Figure 8: The model-observation comparison appears good here probably because of the large (2 order of-magnitude) range in the color bar. The observations themselves cover a much smaller range, so the color bar should be scaled according to the range of the observations. Also I’m not sure this is the appropriate figure type to show because of the uncertainties in the SISS parameterizations. It would be best to have a figure that communicates the full model range using all of your sensitivity simulations.

Both scales are log scales to incorporate the wide range of values. Using a linear scale provided no useful information to the reader. The log scale means the variability at low values (most of the observations) is well represented in the colour scale. Readers can also consult Table 2 for the [Na] values of both model and ice core data. We understand the reviewers concerns about the sensitivity of our SISS:OOSS results (Fig. 8D) to the SISS parameterisation. We have designed two experiments intended to produce extreme SISS:OOSS in order to provide a range of possible SISS:OOSS ratios. These experiments are currently running and results will be included in the supplement.

#### References:

Box, J. E., D. H. Bromwich, and L.-S. Bai (2004), Greenland ice sheet surface mass balance 1991 – 2000: Application of Polar MM5 mesoscale model and in situ data, *J. Geophys. Res.*, 109, D16105, doi:10.1029/2003JD004451.