Review for “Modeling the contribution of leads to sea spray aerosol in the high Arctic”, by Lapere et al.

This study investigates the contribution of leads on sea spray emissions in the high Arctic. The authors first are using a parametrization based on a field work study and they are incorporating it with current state of the art sea spray functions to estimate sea salt emissions from leads. Then they are applying their parametrisation to Weather Research and Forecasting (WRF) model coupled with Chemistry for a case study. This is an innovative work as such parametrizations are missing from regional transport models. However, the manuscript can be further improved. I therefore recommend this manuscript to be published in ACP once the minor comments below will be addressed.

First, I have some general comments. Please address:
Move the figures closer to where they are mentioned in the text, otherwise it is difficult to read your manuscript and look for the figures.
Also, throughout the text you either discuss Gong et al. 2003 or both Gong et al. 2003 and Salter et al. 2015. Please be consistent and always discuss both.
You are using abbreviations throughout the text that they have not been defined. Please define.

Minor comments:
Line 16: Use abbreviation for sea spray aerosols (e.g. SSA)
Line 23: Use abbreviation for sea spray emissions (e.g. SS emissions)
Lines 23-29: You are discussing about the main source of sea spray aerosols, as well as for Arctic related sources. You do not mention frost flowers as a potential source. Please mentioned them in your discussion in the introduction to be complete as there are papers looking into that.
Line 40: Define sodium as Na^+ and use throughout the text.
Line 81: Define abbreviation “WRF-Chem”.
Lines 96-104: You are mentioning that you are using Gong et al. 2003 and Salter et al. 2015 but you are not providing a satisfactory explanation. Explain better why you chose these two since there are plenty more source functions. Barthel et al. 2009 for example, tested more than five source functions. It would be interesting to see results for one-two more source functions. Also please add the source functions (e.g. Gong et al. 2003, Salter et al. 2015 or if you use more) in your manuscript (with and without Nilsson correction).
Line 117: Explain why you are choosing Vignati et al. 2010?
Line 125: You are using Vignati et al. 2010 in your WRF-Chem simulations. However, Archer-Nicholls et al., 2014 already included a source function for marine organics following Fuentes et al. 2010 which was later corrected for Arctic as shown in Ioannidis et al. 2023. Please mention why did you decide to change the source function and how does Vignati compare with original Fuentes 2010 and/or Ioannidis et al. 2023?
Line 147: Mention what is HYCOM-CICE and TOPAZ
Line 156: Define the abbreviation “NEMO”
Line 157: Define the abbreviation “NANUK” and move the definition of neXtSIM in the previous line.
Line 168: Define the abbreviation “MODIS”.
Line 194: Define the abbreviation “ERA5”.

Line 195: Define the abbreviation “MERRA-2”.
Line 197: Use Na^+ for sodium and define once first appearing in the text.
Line 215 and Figure 2: You say you are focusing on the high Arctic. However, have you considered to use the radar data for sea ice at Utqiaġvik, Alaska to address the uncertainties in the different products you are using near the coastlines and even correct your parametrization for coastline regions. May et al. 2016 and Kirpes et al. 2019 are discussing radar data so they might be available.
Figure 3 and Figure 4: Show results for Salter et al. 2015 not only Gong et al. 2003. Or at least justify why you are showing results only for Gong et al. 2003. In Figures 5, 6 and 7 you are showing results for both so it’s really confusing.
Figure 7: Replace sodium with Na^+. Also mention if observations are sub-micron, super-micron, PM2.5 or PM10.
Lines 269-272: Add a reference.
Lines 290-291: Provide more information about the observations, are they sub-micron? Super-micron? Unclear.
Lines 307-310: Compare/discuss why you didn’t choose Fuentes et al. 2010 or Ioannidis et al. 2023.
Lines 325-327: Please re-read Kirpes et al. 2019. They did not measure atmospheric concentrations. They measured elemental fractions. Also be more precise. They used ratios as an indicator of local influence or transported and they found a local influence on sea spray aerosols. Re-write this part.
Line 332 and Figure A1: Again, you only show results based on Gong et al. 2003. Please decide and show only Gong et al. 2003, and justify why, or both Gong et al. 2023 and Salter et al. 2015.
Figure 9: Compare with Fuentes et al. 2010.
Line 376: You are using WRF-Chem model which uses a different source function to estimate sea spray aerosols compared to your analysis so far. Why did you not include Ioannidis et al. 2023 source function in your analysis so far? You could add the source function in Figure 2 and in the text, as the other two (see earlier comment). It would make much more sense to include the source function you are going to use in your WRF-Chem simulations in your earlier analysis and apply there the Nilsson et al. 2001 parametrization/ratio you got and trying different sea ice products. Maybe you will get different results. Or if you already tested it then please discuss it/mention it in the text. Would be interesting to see how Ioannidis et al. 2023 compares with the rest in your analysis, since it was tested for simulations over the Arctic.
Define what SSA is in the WRF-Chem model version you are using.
Line 380: You are using NCEP FNL reanalysis data. I assume sea ice fraction is coming from there. Did you consider using NEMO-netXtSLM sea ice concentration in your WRF-Chem simulations, which gave you the best lead estimations? Can you use NEMO-netXtSLM in your simulation instead of NCEP FNL sea ice fraction? Or did you compare NCEP FNL from the three sea ice products you used to estimate potential uncertainties you might get in your model results coming from NCEP FNL?
Also, for your simulations are using NCEP FNL while for your earlier estimations you used ERA5. ERA5 and NCEP FNL have some differences. Try to explain the inconsistencies and the uncertainties you introduce to your parametrization by switching reanalysis data.
Line 370 and after: You add a new source in the model. How does this affect dry deposition in the model? How does this influence other inorganic aerosols?

Line 380: You just mention the name of the model options for your simulations in the APPENDIX. Add text where you explain briefly the most important options for your simulations (such as aerosol scheme). Add citations that WRF-Chem has been used for Arctic simulations and that can be trusted.

Line 418: Did you test how different will be your results if you use Fuentes et al. 2010 or Ioannidis et al. 2023 with adjusted Fuentes?

Figure 10: You present your parametrisation implemented in a regional transport model. How do you make sure that your parametrization is trusted? How does the model compare against observations?

Please compare against observations. How do you know your results make sense and can be trusted? Compare against sub-micron, super-micron and PM10 Na^+ observations at least in a few Arctic sites. Also include comparison against Cl^-.

Did you estimate ratios, such as Cl^-:Na^+, SO_4^{2-}:Na^+ as Kirpes et al. 2019 or did you calculate depletion factors as Frey et al. 2020 to be able to provide more robust conclusions about your model results?