



EGUsphere, community comment CC1
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Review of egusphere-2022-310

Øystein Hov

Community comment on "Modelling wintertime Arctic Haze and sea-spray aerosols" by Eleftherios Ioannidis et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-310-CC1>, 2022

Title: Modelling wintertime Arctic Haze and sea-spray aerosols

Author(s): Eleftherios Ioannidis, Kathy S. Law, Jean-Christophe Raut, Louis Marelle, Tatsuo Onishi, Rachel M. Kirpes, Lucia Upchurch, Andreas Massling, Henrik Skov, Patricia K. Quinn, and Kerri A. Pratt
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MS type: Research article

Overall review based on the principal criteria:

Does the manuscript represent a substantial contribution to scientific progress within the scope of Atmospheric Chemistry and Physics (substantial new concepts, ideas, methods, or data)? FAIR

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? GOOD

Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)? FAIR

The findings and conclusions are not clear. In my opinion the approach taken in the paper precludes clarity as references to literature findings are continuously introduced interrupting the flow of a consistent argument based on the data material of the study to attempt to answer a few scientific questions (which need to be posed), like what are the relative importance of the removal processes and dispersion for the aerosol concentrations calculated at the observational site?

Some comments:

On l. 85-87 the purpose of the paper is stated: «In this study, the performance of the Weather Research Forecast model, coupled with chemistry (WRF-Chem), is examined with regard to its ability to simulate Arctic Haze composition as well as SSA components, including ss-SO₂– 4 and marine organics.» This is done by comparing model results for two five-day periods in January-February 2014 with observations taken close to Barrow in Alaska. This means that the paper takes the "model for science"-approach, while in a model evaluation requires more of a "science for model" approach. How is the model

diagnosing the processes that affect the calculated concentrations?

The paper is to a large extent a review of literature of observations of aerosols in the Arctic. The number of references is very large, while the understanding communicated from them in the paper is more limited. It does not present the picture of the mechanisms and processes – and their variability with time and in space - that modify the aerosol amount and composition from the source to the receptor, no discussion of lifetime regimes for aerosols of different size and age, even though the field of aerosols is quite old with numerous studies of processes that influence aerosols also in the Arctic and including SSA, since the 1970s. The model results are only discussed in terms of the concentrations calculated, while the diagnostics - why the results ended up in the way they did, is not known. This limits the learning from the results.

One would think that anthropogenic aerosol (fractions) and SSA have quite different lifecycles in the Arctic. In particular super-micron SSA is probably rather local and depending in a quite non-linear way on the upwind wind speed, and depending on the the air masses passing over open leads in sea ice upwind of the observation site. While the concentration of anthropogenic aerosols at a surface site like the one used here, would depend strongly on the synoptic weather situation. Was the site inside or outside of the polar vortex? Are there anthropogenic sources that can emit aerosols or precursors that can be transported close to the surface to the site? To what extent is the calculated aerosol concentration at the Alaska site a small number which is the difference between two much larger numbers? (The concentration field calculated with really slow loss mechanisms compared with a concentration field with a realistic loss processes.) (In this case a factor of 2 or even 5 "error" in calculated concentration near Barrow would be quite a success.)

Even though the agreement between observed and calculated wind speed is very high at the measurement site (Figure D2) (is that due to the nudging?) one would think that in particular for super-micron SSA the concentrations are quite sensitive to the upwind wind velocity close to the ground, as well as the sea ice conditions when the air passes over the ocean. This calls for high resolution limited area modelling of a coupled NWP-sea ice model data assimilation, of the type now available at some meteorological centres. 2-3 km resolution is often routinely available. The 20 and 100 km resolution used for atmospheric physics and dynamics and no mention of data assimilation, seem to inject a fairly large uncertainty into a decisive part of the data set that the WRF-chem calculations are based on?

The illustrations are quite straight forward comparisons of calculations and observations while the text in quite long sections is a description of what the figures show. Perhaps it is possible to convey more understanding through the illustrations? Model diagnostics?