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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of sea ice loss on sea salt aerosol concentrations and the radiative balance in the Arctic" *by* H. Struthers et al.

Anonymous Referee #1

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General comments

In this paper, the authors bring our attention to a very important aerosol-climate feedback mechanism - sea ice extend and SST change on the sea salt aerosol production - in the future Arctic sea ice loss scenario. They quantify the feedback by running perturbation simulations to quantify the direct and first indirect radiative forcing.

The paper is well written and structured. The main strength of the manuscripts is that it identifies the aerosol-climate feedback from the several other feedback mechanisms relating to the decreased sea ice extend in Arctic, such as the snow/ice albedo effect. My major concern is that the model's uncertainty in the representation of Arctic cloud,

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the cloud-aerosol feedback, as well as other uncertainties will influence the confidence in the conclusions about the first indirect effect. However, since the authors are quite aware of those uncertainties, and because of the innovative contribution on this research area, I recommend it to be published if the authors present the uncertainties more clearly and consider the points listed below.

Specific comments 1. The Måttensson scheme cut off at dry radius of 1.4 μm, while the optical depths are usually dominated by sea salt particles with larger sizes. I suggest the authors considering extending the sea salt spectrum to larger sizes using Monahan source function. It could result in different optical depth thus influence the estimation of the direct climate effect.

2. Since also important for the sea salt burden, the removal and hygroscopic growth scheme used in the model should be described in more detail.

3. Wind speed is the dominant effect of sea salt emission. Suggest highlighting in the text ahead of the conclusion session that the influence of wind speed on sea salt emission under 2100 climatology is negligible as implied in Figure 5 e,f.

4. I think some clarification is needed about the climatology used in the perturbation runs. For example, P1-ICE is "identical to CTL but with northern hemisphere sea ice fraction replaced with 2095-2100 climatology ...". As a reader, I might wonder if P1-ICE run uses 2000 climatology with nothing different but 2100 sea ice extend, or with feedbacks of the climate system due to the sea ice extend change? As I interpret from Figure 5 and the aim of the simulation, should it be the latter?

I brought this to attention because although the sea salt emission is not related to sea ice extend change in P1-ICE, changes of wind speed, SST, cloud fraction, and boundary layer height due to the snow/ice-albedo feedback could still influence the sea salt emission, though the magnitude, I believe, is trivial compare to the sea ice extend contribution.

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5. P28870, L10. Since the model sets the sea salt emission linearly change with the sea ice extent, the ∼100 factor is not reflected in the model, these comment is somewhat problematic.

6. Could the authors mention more about how the uncertainty of CAM3.1 in polar-ward transportation effects the simulations?

7. In section 5.1, the authors constrain the model performance of U10, turbulent heat flux, boundary layer height, and the low cloud fraction with observations. However, the model performance on sea ice extend and SST against observations are not provided. Is there any reason such as satellite data not available for the model years? It might be more comprehensive investigation if uncertainty in the modeled ice extend and SST can be mentioned.

8. P28876 L27 "The increase in magnitude of the aerosol forcing in the P2-ICE-SALT and P3-ICE/SST-SALT simulations arises due to the local increase in sea salt aerosol emissions, and is of the order of 50% of the ice-albedo effect as measured by the difference in aerosol forcing between the P1 and CTL simulations." I think this is a very important conclusion relating to the purpose of this investigation – identifying the relative contribution of ice extend-aerosol effect to snow/ice-albedo effect. Maybe more analysis for P2 and P3 analysis will make the argument more interesting. In the abstract, the authors mention that the direct forcing is estimated to be between -0.2 to -0.4 W/m2. I think it is worth mentioning again in this section.

9. P28877 L3. Could the author explain why P3 is closer to P1 than P2?

Technical corrections 10. P28867, L5. Is the 6 year integration forced by sea ice fraction of a period of 6 year during 1980-2000?

11. Figure 5, caption: (a)(b) should be "open ocean area", instead of "sea ice area"

12. Is the ERA-Interim reanalysis the same as ECWMF? Should use the consistent terminology.

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13. P28869, L27, indicate what SHEBA stands for.
14. P28870, L15, compare with Fig. 1 -> refer to Fig. 1
15. P28876, L13, suggest move (0.7 to >0.9) after high albedo.
16. P28877, L2, “already mentioned” -> “as already mentioned”

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28859, 2010.

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