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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The complex response of Arctic cloud condensation nuclei to sea-ice retreat" *by* J. Browse et al.

Anonymous Referee #2

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The Browse et al. paper represents a modeling study, using GLOMAP-mode, tested against ASCOS aerosol measurement data, to assess the impact of complete removal of sea ice on cloud condensation nuclei (CCN) in the Arctic. The main intent is to use this global aerosol physics model to test for the magnitude of the aerosol indirect effect in the Arctic with sea ice loss. This is, I think obviously, an important question to test from a variety of approaches, because of the potential local and global-scale impact of climate change and sea-ice loss in the Arctic, and because it hasn't been well-evaluated to date. The authors use GLOMAP to show that, while removal of sea ice causes very large increases in primary (sea salt, primary organics) and secondary (from DMS oxidation) aerosol emission/production in the Arctic, the impact on CCN concentrations is quite small, and, over the central Arctic, negative. This surprising

result, they show, comes from uptake of condensable gases (H2SO4) onto the existing and larger aerosol surface area (ala the problem with CLAW), and from efficient precipitation scavenging of accumulation mode aerosol. This paper does a very good job with expressing the title, i.e. that the response of CCN to loss of sea ice is indeed quite complex. And, it does what a good paper should do, which is (will be to) stimulate a lot of thought, and, I suspect, lots of response from the community. The results/conclusions of this paper, if correct, are important, and thus I think it needs to be published. The response of CCN to sea ice loss is also more complex than their treatment indicates, in that it assumes that cloud microphysics and precipitation/precipitation scavenging is not changed in response to climate change, which is not true. And, indeed, the authors make an effort in pointing this out in their discussion and conclusions. I feel that this is a good piece of work, and should be published. I really only have two complaints about the paper. First, while this is likely a high quality analysis, the paper is difficult to read, or at least reading it is hard work. Specifically, one has to read the figures, e.g. Fig. 3, and the maps/color bars/details and Figure captions very carefully, while frequently going back and forth between the text (esp. that explaining the four core simulations on page 17093) and the figures, to follow the discussion. To be honest, I am not sure how to fix this, other than to very carefully label the components of the Figures so that they are easy to understand. I will provide some suggestions below. My other complaint is that I think the paper needs to discuss what is known about how well the model actually treats precipitation scavenging of aerosols in the Arctic, with appropriate references. If the answer is that GLOMAP-mode now simulates aerosol mass well in the Arctic, then that isn't very satisfying, and I think some discussion/recognition of uncertainties in that component of the model (and uncertainties in precip.) should be discussed. Again, the authors do a good job in recognizing many of the assumptions made, but some of them could have a large impact on the conclusions. An obvious issue is the very large increase in latent heat flux in a no-ice world, and how that would impact the results; would it represent a large increase in precipitation and precipitation scavenging, so that the surprising result here may be understated? A bit more discussion on this would be useful. More minor comments are listed below, in the order they arose in the manuscript.

1. Page 17092: The paper makes no mention of the importance of secondary organic aerosol; do you think it is unimportant in the Arctic? Is there evidence that that is the case? The importance of dicarboxylic acids in the Arctic makes it seem that SOA is important, and that there should be some discussion about how comprehensive are the aerosol sources in GLOMAP. The sources do not include sea salt aerosol from wind blowing over saline surfaces, e.g. new ice, and frost flowers. Are these known to be unimportant?

2. Page 17092 - do you mean to say H2O2, or is HO2 correct?

3. Page 17094 - what is a "pollution controller"?

4. Page 17096, line 11 - the model-observation slope and intercept are not shown in Fig 2a. In Fig. 2a it is confusing which is the observed ASCOS data - is it the black line? I think the legend in this figure should show colored lines.

5. Regarding the discussion of H2SO4 nucleation - are there any H2SO4 measurements in the Arctic? It is being measured in various places with good sensitivity, I just don't know if it has been done in the Arctic.

6. Last paragraph on page 17096, line 22 - really, your simulations indicate the (likely?) role of drizzle scavenging. I feel that to use the word "confirm", you need specific field observations of that process.

7. Figure 3 is among those that takes some time to sort through. Line 8 of page 17098 says "larger than 100nm diameter", but the Figure 3 caption says 200nm. Fig. 3 should probably have a title line over the top, like, "Aerosol Impacts, No Sea Ice" or something appropriately descriptive. Similarly, Figure 4 could have a caption at the top that says "Aerosol Impacts of No Sea Ice, Drizzle Scavenging Suppressed". The Journal might not like that, but it would help with readability. And, it would be useful to

have a bold label at the left of each row in Figure 3, like: (delta)CCN, (delta)particle # conc. (>200nm), (delta)particle # >3nm. At this page, around line 13, shouldn't the text also state that CCN increases in Arctic coastal regions, i.e. where Arctic people live? At line 24, it is hard to see from Figure 3 what you are referring to.

8. Page 17100, line 13 - do the model uncertainties justify 3 significant figures? Line 11 - it isn't clear what we are supposed to be looking at in Table 2.

9. Page 17101, line 17 - this isn't the number in Table 2, should it be 254? Maybe such numbers need to be rounded anyway, to maybe two sig figs?

10. Page 17103, line 4 - the decrease could well be greater than Voulgarakis et al. suggest, because sea ice loss also will most certainly decrease BrO, which reacts with DMS. At a typical sea-ice covered BL BrO concentration of 10 ppt, the DMS lifetime is ~2.3 hrs. (cf. Brieder et al., GRL, 2010).

Overall, I like this paper and hope to see it published soon!

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 17087, 2013.