Atmos. Chem. Phys. Discuss., 13, C6003–C6006, 2013
www.atmos-chem-phys-discuss.net/13/C6003/2013/

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 #1

Received and published: 20 August 2013

Review of "The complex response of Arctic cloud condensation nuclei to sea-ice retreat" by Browse *et al.*, submitted to Atmos. Chem. Phys.

This study uses a global aerosol microphysics model to suggest that sea-ice retreat in the Arctic could locally decrease the number concentration of cloud condensation nuclei, in spite of increased aerosol and precursor emissions from the newly-opened Arctic ocean. The authors explain this counter-intuitive response by the increase of gas-phase condensation onto the new aerosols to the detriment of the nucleation of new particles, and by a more effective wet deposition as particles grow in size.

The authors do not exaggerate when they characterise the response as complex, yet the manuscript convincingly supports their interpretation and its implication, which

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

states that aerosol indirect effects in the Arctic may not be strengthened by increased aerosol emissions due to sea-ice retreat after all. It will be interesting to see whether other microphysical models exhibit a similar behaviour, and whether observations will eventually support the response mechanisms suggested here.

The discussion is clear and exhaustive, with section 6 in particular being very good. I recommend publication after minor revisions are made to address the comments below.

1 Main comments

- The CCN response strongly depends on the competition between nucleation and condensation processes. Are the authors confident in that particular aspect of their model? Arguably, implementation choices such as the order in which processes are applied, or simplifying assumptions in the representation of condensation, may unduly favour condensation over nucleation. A short discussion of those issues would be useful.
- Section 2 stands out as being less clear than the rest of the manuscript. Specifically, the following points would benefit from being made clearer:
  - Sea ice: the reader is never told where the sea-ice distribution comes from, and how it represents the open leads and marginal ice zone that play a role in the calculation of aerosol emissions (for sea-salt at least, according to lines 10–11, page 17091). In a model with a resolution of 2.8 by 2.8 degrees, this must surely involve some kind of sub-grid parameterisation.
  - Fraction of the gridbox where aerosols are emitted: I guess that the scaling of emissions (page 17091, lines 25 and 28) is in fact by the *complement* of

ACPD
13, C6003–C6006, 2013

the sea-ice fraction, that is, OC and sea-salt aerosols and precursor DMS are emitted over open oceans only?

– Marine organic aerosols: are they emitted into an aerosol mode added to GLOMAP for this study, or into an existing mode (page 17091, lines 21–23)? Also, please clearly state that the new source function is applied over the Arctic Ocean only. It would be useful to extend Figure 1 to also show emissions of marine OC aerosols in the PD-MOC and no-ice-MOC runs.

2 Other comments

- Page 17089, line 1: The word "*however*" comes too soon, as we are only told of forcings of opposite signs from sea ice retreat and cloud cover increase in the next paragraphs. By the way, does cloud cover increase as a consequence of the sea-ice retreat, or is it coincidental?
- Page 17089, line 27: Indicate that the increase in natural AOD is over the Arctic only.
- Page 17090, line 26: Dentener et al. (2006) describe the emission dataset used in AeroCom 1 (Kinne et al., Schulz et al., Textor et al., 2006) to simulate years 2000 and 1850. Hindcast emissions refer to the dataset by Diehl et al. (2012), used for AeroCom 2 Hindcast simulations. It sounds like the authors are in fact using Diehl's emissions. Also, are emissions and meterology both for the year 2008?
- Page 17095, lines 12–13: Is Figure 2 missing a panel? Lines 10-11 and 25–26 of page 17096 seem to refer to a panel showing the slopes and intercepts of model v. observation.

ACPD
13, C6003–C6006, 2013

- Page 17095, lines 23–25 and Figure 2a: I'm trying to understand the PD-noDRIZZ distribution: because wet removal is decreased, there is more time for particles to grow to larger sizes through various processes, thus shifting the distribution towards larger diameters, and more time for particles to coagulate, thus apparently decreasing total number concentrations (although integrating the size distribution by eye may have misled me). Is that correct?
- Page 17096, lines 13–15: Over the Arctic, or in general?

3 Technical comments

- Page 17091, line 20: The unit should be μgm^{-2} here.
- Page 17092, lines 3 and 15, and page 17097, lines 6 and 26: Figure 1a is DMS, 1b sea salt. The text lists the panels in the wrong way around.
- Table 1: Simulation MOC-no-ice is called no-ice-MOC in the text.

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