Review Response: The complex response of Arctic CCN to sea-ice retreat
(Now: The complex response of Arctic aerosol to sea-ice retreat)
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We thank the reviewer for their comments. Our response is below, original review comments are shown in italic, while suggested alterations to the manuscript are highlighted in bold. Line and page numbers will reference our manuscript submitted in response to review.

Response to Anonymous Referee #1

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.

Competition between nucleation and condensation processes in GLOMAP is handled by sub-dividing the aerosol microphysics time-step (t=450s) over which these processes are calculated (5 operational calls per time-step, τ = 90s). This technique (operator splitting) is widely used by large-scale atmospheric models and our order of operations and choice of τ was extensively tested during the development of GLOMAP (Spracklen et al., 2005). Discussion of this method is now included in section 2 (page 3, line 13):

GLOMAP utilises multiple sub-time-steps to account for the different time-scales over which processes operate. For example, competition between nucleation and condensation is represented by sub-dividing the microphysics time-step (450 s) by 5. During development of GLOMAP both the order of operations and length of operational time-steps was tested and found to alter global aerosol number concentrations by less than 5% (Spracklen et al., 2005).

Section 2 stands out as being less clear than the rest of the manuscript. Specifically, the following points would benefit from being made clearer:

We agree with the reviewer that section 2 has difficulty with clarity and have amended the text to separate this section into:

1) Model Description
2) Modelling Arctic Emissions
3) Experimental design

Additionally we now discuss sea-ice (see below)

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.

Sea-ice is prescribed in GLOMAP using a monthly mean year 2000 fraction of sea-ice (FSI) climatology from the British Atmospheric Data Centre (Rayner, 2003). Discussion of our sea-ice climatology (including a figure [**Fig.1**] showing August sea-ice fraction) and treatment of leads and marginal ice is now included at line 25, page 3:

Sea ice cover follows monthly mean data (Rayner, 2003) taken from the British Atmospheric Data Centre (BADC) defined in terms of a grid-box mean sea-ice fraction (F_{SI}) (Fig.1). All marine aerosol and precursor emissions are scaled to 1.0−F_{SI}, which we assume encompasses areas of open-ocean, sea-ice leads and open polynas.

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

Emissions of sea-salt, DMS and marine organics are emitted over open-ocean only and thus are scaled to 1-F_{SI}. We assume that this scaling encompasses open areas of ocean, sea-ice leads and open polynas. Discussion of modelled sea-ice is now included at line 25, page 3 (as discussed above).

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.

Arctic marine organic emissions are emitted into an existing insoluble Aitken mode along with OC from wildfire (emitted at 150nm geometric mean diameter) and anthropogenic emissions (emitted at 60nm geometric mean diameter). This is now stated at line 23, page 5:

Marine OC particles were assumed to have a geometric mean diameter of 40 nm and geometric standard deviation of 1.4 based on measurements in Heintzenberg et al. (2006) and Covert et al. (1996). The best agreement with the observed accumulation mode came from inclusion of a Arctic OC flux (emitted into the Aitken insoluble mode) of 2.85μg/m^2/d OC (per unit area of open water), which when scaled to 1.0−F_{SI} (~10%) resulted in an August median primary OC flux north of 85°N of 0.3 μg /m^2/d OC.
In addition we have extended **Figure 1** to include MOC emissions before and after ice-loss.

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?

Liu et al., (2012) report a 0.3-0.5% increase in Arctic cloudiness in response to a 1% decrease in sea-ice cover. We have now moved this discussion to line 26, page 2:

An increase in cloudiness is supported by a recent study (Liu et al., 2012) showing that over the period 2000-2010 each 1% decrease in Arctic sea-ice cover resulted in an increase in cloudiness of 0.3-0.5%.

Page 17089, line 27: Indicate that the increase in natural AOD is over the Arctic only.

Text has been amended (page 2, line 24)

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 meteorology both for the year 2008?

Emissions are prescribed for the year 2006 (this being the closest year available), while meteorology is for the year 2008 (to match the ASCOS data). The inventory year is now defined in the text and we have amended the AeroCom citation to Diehl et al., (2012) (page 3, lines 20 and 24).

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.

We chose not to include a visual comparison of the integrated particle concentration. However, the reviewer is correct that we seem to refer to this figure in section 6 and have now re-written parts of this section (page 7, lines 23-26, 33-34, page 8 lines 14-20, 28-30). See referee response 2 for further discussion.