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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A satellite-based estimate of aerosol-cloud microphysical effects over the Arctic Ocean" *by* Lauren M. Zamora et al.

Anonymous Referee #1

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This paper uses estimates of cloud properties from satellite remote sensing (AIRS, CloudSat, CALIPSO, MODIS) and black carbon concentrations from the FLEXPART transport model to study cloud-aerosol microphysical effects over the Arctic Ocean. It is found that combustion aerosols are associated with large changes in surface longwave radiation over sea ice. However, up to 91% of the cloud fraction differences between all and clean conditions is due to meteorological conditions, i.e., the black carbon is essentially a passive tracer in these cases.

This paper is well-written and the results are very interesting. I think this paper is suitable for publication in ACP after addressing my concerns below.

Main comments:

1) There needs to be justification in the introduction for focusing on black carbon. It is not clear why other aerosol sources are excluded in this study. There needs to be an overview of previous studies of the role of black carbon in Arctic cloud-aerosol effects.

2) The measurements are separated into clear and cloudy conditions as a function of height. So different days are used in each of these averages? It would be very useful to have a figure showing what days where used in the different averages. Are the upper and lower quartiles used to make Figure 2 using data from different seasons? Are the averages as a function of height in Figure 1 using data from different seasons? If so, it obscures some of the effects since, for example, if the aerosols cause increased activation of ice crystals and precipitation at high altitudes then it will not be possible to see the impact of these at lower altitudes. I first interpreted Figure 2f as an example of increased ice production at higher altitudes and depletion of water vapor due to deposition at lower altitudes but this may not be the case if the values at different heights are calculated separately.

3) Throughout the paper it is said that this study is focused on regional-scale effects. What is meant by this exactly, that sea ice and open ocean is analyzed separately? For example, page 5, line 17, what is meant by "regionally averaged"?

4) The results indicating increased ice precipitation in MPC at low altitudes and decreased precipitation at high altitudes is very interesting. It would be good to include a more detailed comparison with the results from previous studies in the discussion section.

Minor comments:

1) Page 5, line 4: How does focusing on relative rather than absolute differences get around the issue of misclassification of small supercooled water as ice particles?

2) Page 7, line 21-22: This is a very interesting result but why wouldn't it be the case over open ocean?

3) Page 7-8, lines 33-1: Why is this relationship due to microphysical effects and not meteorology? Can it be concluded that microphysical effects are stronger in stable conditions or just more observable? If it is stronger then why is there no significant difference in the LTS for high and low quartile black carbon (Figure 2e)?

4) Page 8, lines 11-12: This is an interesting result but what would this be the case?

5) Page 10, lines 3-5: Are the results of this study consistent with the deactivation of pre-existing INP hypothesis (Archuleta et al. 2015; Cziczo et al. 2009)?

6) Page 11, lines 15-16: Why would the impact on MPCs be different over sea ice and open ocean?

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