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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The summer aerosol in the Central Arctic 1991–2008: did it change or not?" *by* J. Heintzenberg and C. Leck

Anonymous Referee #2

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This paper documents the physical aerosol particle size distribution in the Arctic north of 80 degrees latitude during four summers over an 18 year period.

I consider the results to be important. While the coverage of the dataset is not perfect, it is substantial and of high quality, and the absence of a clearly significant difference among the distributions across the years does suggest little change in this aerosol property over that time in this region. Also because the distributions are so similar, it indicates that the processes contributing to this aerosol are reasonably reproducible from year to year. The increase of the Hoppel minimum with number concentration is also interesting; albeit perhaps consistent with our expectations of this.

On the other side, it is fortunate that there is a title and abstract as it is hard to guess what this paper is about from just reading the introduction. Sea ice has declined in the

Arctic, and yes that decline can me affect surface ocean-atmosphere exchange etc, but this can be told more simply than done here. I like the discussion of the ice, but it goes beyond the subject of this paper. The overall discussion could stand to be a little more objective. As it is now, there is repeated and distracting discussion about microcolloids released from the ocean water being responsible for the particle number concentrations. Maybe they are, maybe they are not or maybe they are part of it, but that is not what the paper is about. In fact, it could be argued that the significant change in sea ice and the absence of a significant change in the particle distributions opposes that as the source. The paper needs to be trimmed so that the paper focusses on the data that are presented.

Detailed comments: 1) Abstact, line 4 – "do" rather than "to"

2) Abstract, lines 5-6 – as Reviewer One suggests, it may also be insufficient data. Therefore, it is premature to suggest "causes".

3) Abstract, lines 18-19 – Another supposition is that this will remain "the only aerosol information from this region... for some time... orbiting satellites do not cover the area...". First, there are many aerosol data from the Alert GAW baseline station, past and coming, and from past airborne measurement programs in the region. So your data are not the only aerosol information, and more will be available soon. How will satellites provide the data you present in this paper, even if they do reach the North Pole?

4) Page 888, line 23 – why is "Table 2" here?

5) Introduction – The introduction does not convey a sense of the main purpose of the paper until the very last paragraph. Rather than start by telling us why it is important to know more about the atmospheric aerosol in the high Arctic, it talks about sea ice change and possible impacts of sea ice change. On lines 26-27 of page 891, you discuss using data from nearly two decades, but we don't know what data are being discussed until the next paragraph.

6) Page 890, line 3 – You say here that “clean air”... “reduces the albedo of the clouds...”, but relative to what? What was there before clean air?

7) Page 890, lines 5-6 – The sentence is unclear in part due to grammar.

8) Page 890, line 24 to page 891, line 2 – This discussion is too simple. DMS may be less likely to produce new particles if the condensation sink is significant, but in the high Arctic that sink is relatively low (as you indicate on page 896, lines 10-11) and DMS can produce new particles in the near-surface atmosphere in the Arctic (Chang et al., J. Geophys. Res., 116, doi:10.1029/2011JD015926, 2011).

Then you haven't discussed what the precursor is that leads to the growth of the biological particles that you claim are responsible for the numbers of CCN. How does that growth happen in the absence of DMS? Or if there is an influence from DMS and the sulphuric acid generated from DMS condenses on the biological particles of a few nm's in size, then why can it not also nucleate? In other words, is the condensation sink offered by these biological particles sufficiently high?

Also, since you focus on the Hoppel minimum in this work, why is there no discussion of the importance of DMS in that context? Afterall, S(IV) oxidation in cloudwater is likely the most important process contributing to the advancement of particle size in cloud residual particles.

9) Page 891, lines 20-24 – Even with satellite coverage over the North Pole, how are satellites going to help with this problem? Can they accurately measure any properties of such a low concentration aerosol? What about when there is low cloud?

10) Page 894, line 24 – “in”

11) Page 897, lines 7-14 - Why is this here? I don't see the relevance of this discussion to the paper, but if there is a good reason then it belongs in the introduction with references.

12) Page 904, line 7 – I don't understand the term “structural” in this application. It is

of course used in a number of places, and it needs to be defined at the beginning.

13) Page 906, lines 24-26 – Or, if this is truly a "Hoppel" minimum, it could also be a product of feedback of increasing N on supersaturation for clouds with low cooling rates; the latter is apparently your situation.

14) Page 906, line 27 to page 907, line 1 – It is not evident that this statement is correct. Figure 6 is a log-log plot with scaling factors used to separate the data. Why should it be any different than your thick curve with the open dots in your Figure 9?

15) Page 907, lines 7-8 – what do you mean by “limited”?

16) Page 910, lines 10-11 – But Chang et al. JGR 2011 (mentioned above) was able to explain nucleation in the Arctic using more conventional nucleation based on DMS as the precursor.

17) Page 911, lines 21-23 – This is neither a conclusion nor, as above, do I believe it is a factual statement.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 887, 2012.