Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-1181-RC1, 2018


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "New particle formation events observed at the King Sejong Station, Antarctic Peninsula – Part 2: Link with the oceanic biological activities" *by* Eunho Jang et al.

**Anonymous Referee #1**

Received and published: 18 December 2018

General comments: This manuscript (ms) is the second part on new particle formation (NPF) events observed at the Korean Antarctic Station King Sejong. The objective of this remarkably brief accompanying study is to identify the interrelation between NPF and biological activity as well as phytoplankton taxonomic composition in the most dominant source region for this site, the Weddell and Bellinghausen Sea. The authors present authentic and original scientific material that potentially has relevant implications for understanding atmospheric processes in Antarctica and is an important contribution on this field of research. Hence, the subject is appropriate to ACP and I recommend accepting the paper after some (major) revisions. My principal concern is that the main conclusion of this study, i.e. the link between phytoplankton taxonomic



composition and NPF is not convincingly presented and the scientific approach is not clearly outlined (see my comments below).

Specific comments:

1. The discussion presented in Chapters 3.2 and 3.3 is not conclusive. I am confused about the described approach deriving DMSP concentrations and DMSP/Chl ratios. The Galí et al. algorithm presented in the Supplement tells me, that DMSP concentrations are dependent on Chl concentrations, SST, MLD and PIC. What is the impact of SST, MLD and PIC compared to Chl? Did you resort to the phytoplankton taxonomic composition derived from the PHYSAT database to weight the results? In this case: How representative is the PHYSAT database for the period 1997-2010 with regard to your observation period?

2. However, even agreeing that DMSP/Chl ratios were (generally) higher in the Bellinghausen Sea area, from this fact alone, you cannot conclude that DMS production was higher. For this purpose, one has at least to compare the absolute DMSP concentrations, not just the ratios as presented in Fig. 2c. Mean chlorophyll concentrations are much higher in the Weddell Sea; hence, a systematically lower ratio in this region is just comprehensible, even in the case of DMSP concentrations being higher compared to the Bellinghausen Sea.

3. In addition, it is not clear in which way you generate the pixels shown in Fig. 2b: What is the threshold value for a given phytoplankton contingent to be depicted as dominant? In the same way, the assertion on page 5 line 36-37 is equivocal: "...35% of the satellite pixels were dominated by diatoms...". What does this mean and what is about the remaining 65%?

Chapter 3.1, line 12-14: About 38.2% of the hourly mean number concentration of nanoparticles complied with the >90% criterion. Did the remaining 61.8% of the data indicate any significant link to the origin of the air masses?

Chapter 3.3, first passage and Fig. 3: MSA data are only available for one summer season (2013/14). Due to the fact, that MSA concentrations around Antarctica have proven to be extremely variable on every timescale (see e.g. Minikin et al. 1998), the representativeness of the data may be questionable. How many individual filter measurements are represented by each bar shown in Fig. 3? It would be much more informative presenting here the original (daily) MSA concentration data.

Technical correction:

Page 3, line 34: "...below the detection" should be "...below the detection limit."

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