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Atmospheric Chemistry and Physics Discussions

Interactive comment on "New particle formation events observed at King Sejong Station, Antarctic Peninsula – Part 1: Physical characteristics and contribution to cloud condensation nuclei" *by* Jaeseok Kim et al.

Anonymous Referee #1

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General comments:

The manuscript at hand characterizes new particle formation (NPF) events observed at the Korean Antarctic Station King Sejong. As the authors state, this is the first NPF investigation from the Antarctic Peninsula. To my knowledge it is based on the longest observation period actually measured in Antarctica regarding this topic. In addition, the authors discussed in particular NPF events along with cloud condensation nuclei (CCN) data. The article is written in a straight and concise way and presents invaluable results to elucidate NPF and its impact on CCN availability at this site and (coastal)

Antarctica in general.

However, the article in the present appearance has some weak points. Especially the regrettably scarce discussion in general is not commensurate with the unquestionable valuable data set. In addition, description of the used instruments and data evaluation procedures are often insufficient (see specific comments below). I think this outstanding data set is worth the effort addressing this weakness and considering a more in-depth discussion.

Notwithstanding, I am confident that the data and their evaluation presented here are of high quality and on the whole, the subject is appropriate to ACP. Hence, I recommend accepting the paper after revisions according to my specified suggestions from above and listed below.

Specific comments:

1. The authors should provide specification and operation details for the SMPS and CCN instruments even though they were comparable to Kim et al. (2017). Furthermore, I miss an adequate presentation of the SMPS results! In any case, it would be advisable to show some figures (e.g. the typical Dp vs. time contour plots), at least for the two case "A" NPF events.

2. Another concern is the lack of an appropriate CCN data presentation. For instance, the authors showed no figures regarding the CCN spectrum. I would appreciate a thorough description of the performed data analysis.

Chapters 2.2.1. and 2.2.2: The authors based their definition and classification of NPF events on the criteria compiled by Dal Maso et al. (2005) and Yli-Juuti et al. (2009), which are widely accepted by the community. According to these previous studies, an NPF event must show signs of growth (see Dal Maso et al., 2005, p. 326). Therefore, NPF events can only be identified by size distribution (here SMPS) data, but clearly not by sole CN2.5 minus CN10 data. The latter just indicate a potential NPF, which may be

better termed as particle burst.

Chapters 2.2.3: I guess that CN and especially CN2.5 – CN10 concentration data based on 1 s resolution are highly fluctuating, making the FR evaluation somewhat arbitrary. Please specify in more detail the way you extract dNnuc/dt from the data (maybe by showing a representative figure?).

Chapters 2.3: Please specify, in which way/procedure you have characterized air masses (by cluster analysis or just "manually")?

Results and Discussion chapter and Tables 4 and 5: The authors observed just two type "A" NPF events, from which growth can be determined with confidence according to Dal Maso et al. (2005). For type "B" events, the authors state that growth was not clear (see caption of table 4). I am confused about this: Does this mean the bottom line is that the reported growth rates were based on merely two events? Please clarify this point!

Chapter 3.3: From my point of view the presented discussion is inadequate. Evaluation of the CCNC data demands a more detailed description and discussion. Especially: A systematic analysis along with SMPS data would be crucial and should be presented. Are your CCNC results consistent with SMPS data?

Chapter 3.4, lines 12 through 14: The authors argue that higher GR observed in air masses emerging from the Bellinghausen Sea area are due to higher source rates of condensable vapour. Unless I am very much mistaken, this is a typical case of circular reasoning, because regarding eqs. (4) and (5), the source rate Q is linearly dependent on the GR, isn't it!

Technical corrections:

Page 1 (abstract), line 16: Misleading phrase. Change to ". . .during particle bursts (not during a particle burst). . ."

Page 1 (abstract), lines 23, 27, and throughout the text: Please present measured

values and values derived from data just with their relevant/meaningful digits.

Page 3, line 3: Misleading phrase. Change to "A NPF event occurring in the period between December 1998 and December 2000. . ."

Page 4, line 15: Modify to ". . .raw data measured during the following conditions. . ."

Page 4, line 21 and throughout the manuscript: Delete "value" in "value difference".

Page 5, line 24: Use a unique consistent term for particle number concentrations between 2.5 nm and 10 nm (either CN2.5-10 or CN2.5 – CN10) throughout the text!

Page 6, line 15: "speed" should be "loss rate".

Page 8, line 15: "should speculate" should be "indicates".

Page 9, line 18: Delete "(which is undefined days)".

Page 9, line 23: Delete "whereas".

Page 11, line 18-21: I do not understand the meaning of this phrase - please clarify: What is meant with "undefined case" here? Delete this line in Table 5.

Page 12, line 2: ". . .a indicate decline. . ." should be ". . .indicate a decline. . ."

Page 12, line 3: ". . .discussed. . ." should be ". . .discussing. . ."

Page 12, line 4: ". . .simulation. . ." should be ". . .model. . ."

Page 12, line 7: Misleading phrase. The term "estimates of the biological characteristics" is somewhat vague, please specify.

Page 12, lines 8-9: DMS oxidation to sulphuric acid occurs in the atmosphere but not in the ocean – please correct!

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