Review of "*New particle formation leads to enhanced cloud condensation nuclei concentrations at Antarctic Peninsula*" by Park et al.

This study tackles the result of continuous size distribution and cloud condensation nuclei (CCN) measurements in the King Sejong research station located North of the Antarctic Peninsula for the whole year 2018. The work presents a consistent and continuous set of data for the physical characterization of aerosol particles which enabled to assess the significant occurrence of NPF, principally during the summertime. Authors addressed the new particle formation events observed (97 in total), as well as the source point – specifically looked out among marine, sea-ice, a multiple-origin study cases -, and discussed possible chemical drivers of the observed new particle formation events. Finally, the study focused on the CCN result in relation to the observed NPF. The paper is generally well written, and I suggest the publications in ACP after the revisions/clarifications on the following points:

General comments:

The study undeniably complements the knowledge on NPF and its occurrence in the remote Antarctic field. In the context of polar region, NPF statistic based on seasonality would be an asset while showing occurrence during summer / winter and the transitions period of the melting and refreezing of the ocean. (Suggestions: Examples of 'typical' event, with size distribution surface plot would introduce nicely the discussed topic).

The source attribution was thoroughly investigated and well described in the study case subsections. Without direct measurement of precursor gases at site, potential source for NPF can only be discussed rather than undeniably explained, which clearly expressed in the manuscript. However, one big argument is the local fauna, and whose emission surely influence the observation, and this possibly independently of the air trajectory due to its close vicinity. Discussion could be developed in that regard (Suggestion: the use wind data could then be relevant).

The contribution of NPF to CCN would find benefits in a strong(er) linkage rather than with observations of increased CCN after occurrence of NPF solely, possibly using the hygroscopicity factor in relation to growing particles. Alternatively, the authors could rise conscience in the missing link between the two datasets and the need for dedicated studies/measurement in the future.

Specific Comments (RC):

RC$_1$ (**Abstract**) Since the study present a year-long dataset of size distribution, one could expect general information on the seasonality and frequency of NPF observed throughout the year. This would bring context and significance to the abstract.

RC$_2$ **(§1, L.53-L.55)** Have the authors considered possible anthropogenic activity as well as the newly emerging land vegetation as a possible source of NPF to be mentioned in introduction as well.

RC$_3$ **(§1, L.57-L.58)** I suggest adding Sipilä et al. (2016, https://doi.org/10.1038/nature19314) as a key reference for showing the role of Iodic acid in NPF in Marine & Polar environments.

RC_4 **(§1, L.71-L.75)** Let me bring to your attention the publication of Quélever et al. (2022, https://doi.org/10.5194/acp-22-8417-2022) reporting new particle formation at the Marambio station during the early 2018 (incl. measurement period of the present study), also reporting particle characterization based on size distribution. This reference could also bring context in the discussion presented later in **(§3.3.1 and §3.3.2)** regarding the relationship between NPF occurrence and meteorological parameters and potential (chemical) source of NPF.

RC_5 **(§2.1, L.107-L.108**) Please clarify for each CPCs if this corresponds to measurement incl. ultrafine particle (corresponding data $CN_{2.5}$) or particles larger than 10 nm (corresponding data CN_{10}), if mentioned already in the method, there is then no need to specify it later (e.g., **L.233 – L.234** or in the caption of Fig.2).

RC_6 **(§2.2, L.155-L.159 & L.165-L.168)** Could you explain the reason for the BC comparison between North Atlantic Ocean and Southern Ocean (where northern hemisphere is usually more influence by anthropogenic factors)? The environment being significantly different, I would suggest to clarify the context or to revise the relevance of this additional information to the manuscript.

RC_7 **(§3.1)** I would suggest to re-assess the structure of the subsections within 3.1 in order the easily follow the story line of the analysis by, for example, adding a subsection for the meteorological parameters influencing the aerosol particles and their formation (L. 248).

RC_8 **(§3.1.1, Table 1 & L.230)** Please reformulate the caption for Table 1: e.g., "*Monthly median for total particle number concentration >10 nm (CN10) ….*". I also suggest to add, in the caption, that the data are filter for pristine / clean conditions only (i.e., data when BC <50 ng.m-3). Finally, review the sentence **L.230** accordingly as **Table 1** does not show the time series for one-hour average but it recaps monthly median values for the year 2018.

RC_9 (**§3.1.1, Figure 2**) $CN_{2.5}$ is visible only from Jan. to Mar., it would be worth to comment on that in the text as well. Also consider to use color set that are color blindless-friendly (e.g., other than green and red in the same plot). (**L.244-L.247**) The correlation analysis CN_{10} vs N_{NUC} could be more relevant with a bigger data set rather than with monthly averaged values, why not using the one-hour data?

RC_{10} (**§3.1.1, L.270-L.272**) Although the authors focus first on specific meteorological parameters influencing NPF, I suggest to strongly insist on the combination high PAR, high temperatures, low RH altogether rather than finding explanation with one parameter alone. Furthermore, high windspeed would enhance mixing of the emitted compounds and accelerate transport (possibly from further away). Please add reference for enhanced VOC in high windspeed conditions.

RC_{11} (**§3.1.2, L.276 -**) The section 3.1.2 depicts the statistics on the observed NPF events. I would suggest to first mentions the numbers of event observed with a brief description of representative event types (if such grouping can be done), e.g., burst events, nucleation transported + local growth, etc.… incl. example with size distribution surface plot.

RC_{12} (**§3.1.3**, **L.301 -)** The start of the section reintroduces the NPF / nucleation presented in the earlier section, I would recommend restructuring the section 3.1. in order to follow a coherent path on the descriptions of NPF events, without reintroducing NPF observation on every subsection.

RC_{13} **(§3.2)** For each case study, the manuscript shows the result of CCN data for one supersaturation ratio only, could the authors develop on the reason for using this data only rather than comparing with the information brought with the other super saturation ratio.
RCx (**§3.3.2, L.426 -**) Here as well, I suggest adding Sipilä et al. (2016, https://doi.org/10.1038/nature19314) as a key reference for showing the role of Iodic acid in NPF in Marine & Polar environments.

RC_{14} (**§3.3.2, L.452 -**) The presence of Penguins close (~2 km) to the measurement location is a determinant parameter impacting the frequency of NPF. Earlier in the section, the authors discussed extensively on the air mass origin, however this now bring a strong local source for chemicals bases such as ammonia that can trigger NPF. This, however, could only be validated by direct measurement of precursor gases. Hence, I highly recommend discuss this further, as well as the need for gas phase measurement. As such, a close emission point will likely interact with the result interpretation incl. from most / all the air mass origin. Furthermore, if pinguins / birds are only present in the surrounding seasonally, I would further suggest a comparison on the frequency / intensity of the NPF observed between e.g., breading season and start of the spring.

RC_{15} (**§3.3.3, L.469 -**) Could the authors bring clarity on the connection between the size distribution data and the CCN data. It is a critical point of this study, as NPF and CCN do not occur at the same time, it would be interesting to account for parameters associated with CCN formation in connection to NPF (particle number/diameter, sinks, survival probability …) Have the authors considered CCN transported from another source (i.e., primary particles)? Alternatively, the authors could present the result given with supersaturation ratio and discuss the link with hygroscopicity (CCN data) and growing particles (DMPS data). I then suggest referring to Chang et al. (2022, https://doi.org/10.5194/acp-22-8059-2022) to complement the analysis.

Technical comments (rmc):

TC_1 **(Title)** Suggestion to revise the title by replacing the preposition "*at*" by "*in **the** Antarctic Peninsula*".

TC_2 (**Abstract, L.24-L.27**) Suggestion to reformulate as there is no direct measurement of DMS/DMSP. → E.g. "*Our estimation of DMPS concentration from satellite chlorophyl data suggest that product of biogenic precursor could be a component of marine NPF, …*".

TC_3 (**§2.1, L.129-L.130)** CCNC supersaturations either 20%, 40%, 60%, 80% 100% or supersaturation ratio of 0.2, 0.4, 0.6, 0.8, 1.

TC_4 (**§3.1.3**) "*size spectra*" → "*size distribution spectra*"

TC_5 (**§3.1.3, L.390**) "evets"→ "events"

TC_6 (**§3.3.2, L.413 & L.418**) Please consider the product of oxidation of DMS are those responsible for the NPF. Condensable vapors: Sulfuric acid and Methane sulfonic acid.