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Comment on acp-2021-24

Anonymous Referee #2

Referee comment on "Characterization of aerosol number size distributions and their effect on cloud properties at Syowa Station, Antarctica" by Keiichiro Hara et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-24-RC2>, 2021

Review of Characterization of aerosol number size distributions and their effect on cloud properties at Syowa Station, Antarctica

The paper of Hara et al (2021) aims to characterize aerosol number size distributions and their effect on cloud properties at Syowa Station, Antarctica. The work is not well presented and the paper cannot be published in the current state, I suggest rejection, full analysis and resubmission.

The literature review is very poor, most of the references are not cited in the reference list, there is no mention to many Antarctic papers referring to other chemical component driving new particle formation (Sipila et al 2015, Jokinen et al., 2018), as many others discussing NPF in other stations (some of the Dr Weller studies are cited but much more recent work is not discussed). The long discussion of

The analysis is carried out on an old SMPS dataset of 2004-2006. It aims to present a full SMPS dataset but in reality our modal structures (i.e., mono-, bi-, tri-, and quad-modal) were identified in aerosol size distributions during measurements. Particularly, quad-modal structures were associated closely with new particle formation (NPF).

Most of what is stated and reported in the paper seem to be a narrative context, barely supported by the data. I do not find any scientific evidence to show what is reported. Figure 10 seem to report a schematic presenting hypothesis. This seem to be related to the Ito et al., 1993 paper - but poor support is given by the data analyzed.

minor comments

line 12 put year of measurements

line 16 put some %, at the moment paper is Very descriptive

line 24 not sure it is seen in the data

line 40-50 poor literature review and most of the papers are not in the ref list

line 55-70 other chemical components are mentioned and found in other papers (amines, iodine, organics, ect)

line 90-120 check equations with previous papers (Delmaso et al, and others)

line 140 why 500 metres above ground? it is suggested to make a whole new analysis and a new calculation with multi ending point at ground level

line 160 not sure if this is a valid classification, surely I am not familiar with these types of multifitting and not compared with other existing studies (including polar aerosol size distributions and PMF and K means clustering).

line 155 not sure if the classification is valid. Additionally, the title reports a full analysis but in essence this paper only talks about nucleation (and poorly analyzed)

line 150-160 not sure these are nucleation events, not clear from only one size distributions. How many full events, what types, duration, growth rate, examples, case studies).

Again, there is not a % or any number, very qualitative

line 180-190 Blowing snow and sea spray, many studies are available in the literature in

Antarctic regions and not described and compared here.

line 184. "and so on" seem a very poor way of reporting scientific pathways.

line 210 the Na is not measured and not sure it is a valid method to report. It is calculated and not sure valid.

line 280-330 discussion and conclusions are more of a speculation section, not sure the data are showing this, and it is difficult to see SMPS data given the analysis carried out is poor.

line 525 figure 10 is very speculative and does not take into account any paper published since the Ito et al 1993 paper. It also speculate that only FT NPF can make CCN whereas the BL NPF cannot make CCN.

Overall the entire paper try to follow the idea of Ito et al 1993 showing that most nucleations occur in the free troposphere - leaving out most of the recent literature.

It is suggested overall to make a deeper analysis of the SMPS dataset and resubmit the paper, if not to ACP maybe on a more specific lower impact factor journal.