Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2022-21

Anonymous Referee #2

Referee comment on "Predicting atmospheric background number concentration of ice nucleating particles in the Arctic" by Guangyu Li et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-21-RC2, 2022

Review of "Predicting atmospheric background number concentrations..." by Li et al.

The authors report INP measurements at Ny-Alesund (Svalbard) at temperatures between 0 and -30 °C for 12 weeks (Oct-Nov and March-April). They did not see a significant difference in INP concentrations between Oct-Nov and March-April. In addition, the results fall within the range of INP concentrations previously reported for the Arctic. Also, they show that parameterizations developed for mineral dust, sea salt aerosol, and bioaerosols over pine forests do not predict the measurements well. Also, other parameterizations developed from measurements in other regions do not describe the measurements well. In addition, they fit their data as a function of temperature and suggest that this parameterization could be used to describe background number concentrations in the Arctic.

The measurements at Svalbard are important, and I congratulate the authors for generating an important dataset. I think it is also useful to show that these measurements cannot be reproduced with previous parameterizations, even though the disagreement is expected since the parameterizations were developed for other regions or for specific types of aerosols. I support the publication of this part of the manuscript, although publication as a Measurement Report may be more appropriate than publication as a Research Article.

Like Referee #1, I have major concerns about the section that describes the new parameterization of the authors' data. The authors are suggesting that their measurements from one location in the Arctic and during only 12 weeks can be used to predict background concentrations for the entire Arctic. To me, it does not make a lot of sense to use measurements from one location in the Arctic and during only 12 weeks to develop a parameterization for the entire Arctic, especially when some studies have shown a seasonal dependence on INP. If one wanted to make a parameterization for the entire

Arctic, it would make more sense to use all the currently available INP data collected in the Arctic. I am not suggesting doing this, since INP concentrations are expected to change with season and location in the Arctic (at least in some cases), and one parameterization using all the previous measurements would miss this variability.

Below are specific comments:

- Figure 7 shows that the parameterization developed by the authors is off by approximately 2 orders of magnitude in some cases. To claim the new parameterization is doing a good job for the entire Arctic seems to overstate the power of the new parameterization. Furthermore, if you fit all the previous Arctic data, you would get a different parameterization and one that would be likely more applicable to the whole Arctic, since it would be based on measurements for different locations and many different times of the year. Why develop a parameterization from just one location and for a very limited time?
- Page 1, line 10. Consider changing "not feasible" to "not successful".
- Page 4, lines 61-63. "This parameterization will help evaluate the role of cloud phase interactions in Arctic MPCs, and contribute to the progress on accurately estimating cloud influenced climate predictions in the Arctic". Like Referee #1, I think the authors are overselling their work here. They have done measurements at only one location for 12 weeks, and then they used this data to develop a parameterization for the whole Arctic. A more reasonable approach would be to take all the previous measurements in the Arctic and then generate a parameterization based on this combined data set. Even then, I do not think this is a very useful approach since previous work has shown that concentrations can vary with season and location in the Arctic (at least in some cases).
- Page 3. Line 84. The Coriolis impinge has a cut-off of 0.5 μm. Does this mean the technique is missing a large fraction of INPs? Could this explain the lack of agreement with the parameterizations based on particles above 0.5 μm? Please discuss. Did the HINC have a similar cut-off?
- Page 13. Line 259-261. "Our INP parameterization promotes future modeling studies via a more realistic microphysical representation in the Arctic MPCs, especially the vertical profile of primary ice distribution (Hawker et al., 2021), thus, improving the predictions for the future Arctic climate." Again, similar to Referee #1, I think the authors are overselling their results here.
- Page 13, lines 251-253. "Note that the presented INP parameterization is specified for the Arctic environment, where the atmosphere is well-mixed and transient effects average out." What is the evidence for a well-mixed atmosphere in the Arctic? I think there is a lot of field data that shows that the Arctic is not a well-mixed system. Please correct me if I am wrong.
- Page 13, lines 254-256. "The presence of well-mixed INP air masses is exhibited by the absence of a relationship with aerosol properties and further by the inability of previous aerosol-based INP parameterizations to reproduce the observations from this study." I do not understand the authors' logic here. The absence of a relationship with aerosol properties may have nothing to do with a well-mixed INP air mass. Furthermore, the inability of previous aerosol-based INP parameterizations to reproduce the observations from this study may have nothing to do with a well-mixed INP air mass. Please correct me if I am wrong.