Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-1101

Anonymous Referee #1

Referee comment on "Ice-nucleating particles near two major dust source regions" by Charlotte M. Beall et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1101-RC1, 2022

The field data of ice-nucleation particle (INP) concentrations from the studied regions are rare and invaluable. This study covers many topics, including but not limited to:

*Immersion freezing properties of ambient aerosol particles vs. subsurface seawater collected during the study presented vs. surface soil sample nearby the studied region, and

*Heat and H2O2 applications to assess protein denatured INP and organic-stripped INP concentrations etc.

These research topics are relevant to the ACP journal scope as ACP supports many INP-related papers. Though the data coverage is limited to the two-month campaign, the authors present sufficiently detailed results. The manuscript is a bit wordy but it is informative and reasonably well organized. The authors clearly address the necessity of future study (e.g., aged vs. nascent dust in terms of n_INP; P31L700-707), and this reviewer agrees with the addressed outlook. Overall, in the reviewer's opinion, this paper is worth publishing in ACP and worth it to be shared in the atmospheric science community and beyond. This reviewer supports the publication of this paper in ACP after minor and technical revisions.

[Minor comments]

P6L170-174: It appears that multiple inlets were used for this study. Do all inlets come with a similar particle transmission/loss rate? Was it accounted for the size distribution data (or neglected)? Please clarify in the text.

P7L198-204: MARGA measured TSP but it seems other measurements (i.e., FMPS and OPC) assessed particles up to 10 um. Were there any >10 micron diameter particles measured during the campaign? Since the authors are adapting the MERRA-2 reanalysis data for the range of 0.1 – 10 micron (P2L49 & P8L220), further justification in the detected size consistency seems necessary here. Please elaborate.

P7L196-198: How did the authors estimate this overall uncertainty of 30%? The reviewer assumes this is some sort of systematic error. Please clarify in the text.

What were the measurement time resolutions of OPC and FMPS? Were the data time-averaged afterward, and that is what is reported in Table 1 for aerosol surface area concentration?

Were the statistical relative deviations of surface area concentrations within 30% over individual sampling times?

Table 1: Each sampling interval covers several hours. Air masses came from a similar source throughout individual sampling periods? The authors may consider showing the min-max ranges of surface area for each timestamp or some sort of data deviation range. They may help clarify to the readers if air masses were consistent over each sampling interval (or not).

Have the authors analyzed the correlation between INP concentration and ambient meteorological conditions? Were there any precipitations during the campaign, where rain may have washed out the dust and fresh particles (thus, showing low INP conc.?)?

Sect. 2.3. What is the uncertainty involved in the estimation of reported dust & sea salt mass concentrations? The MERRA-2 spatial scale seems big, and the reviewer wonders if such large-scale data can represent the pin-point dust concentration at the sampling point during the authors' campaign. Is the MERRA-2 data sensitive and representative of the surface dust concentration at the sampling height that the authors employed?

Fig. S5: There seem some gaps between x1 and diluted results beyond the CI95% (e.g. f035, f042). Which n_INP (upper or lower bound) would be representative for the overlapping T region (i.e., ~-10 to -15 dC)?

P11L319-P12L325: How was this H2O2 treatment protocol developed? The reviewer thinks that it will be informative for the reader to know how the 1.6:0.8 mL ratio and this particular concentration of H2O2 were selected to be used in this study. It would be nice to have a reference here if the same procedure has been used in a previous study. Otherwise, please explain.

P12L323-325: Does the catalsase-added H2O2 suspension blank have a similar background freezing spectrum as compared to the field blanks? There is no suppression of background INP in pure water by these catalysts, correct? Perhaps, the authors can add the spectrum of the H2O2-treatment blank on Fig. S6 or may consider showing it elsewhere in this manuscript.

Fig. 2: This reviewer encourages the authors to provide the error bars for the observation data (at least to some representative data points at higher and lower ends of T). They do not need to be statistical uncertainties. Can be systematic uncertainties instead.

P20L482-483: How were these 12 samples selected? The author may provide a brief explanation here or in Sect. 2.4 (~L315).

P21L501-504: Interesting. This release/exposure of INA core upon an application of heat can seemingly be a good future study topic in the IN research community. The authors may consider mentioning this somewhere as one of the outlook study topics.

P31L718: The authors may consider adding “potentially” in front of enabling. The proposed tagging would not warrant to link INP and aerosol properties as aerosol composition is not necessarily identical to INP composition. Perhaps, more reasonable

properties to link in this context would be the relationship between ice crystal residual composition, n_s, and n_INP. Knowing that air masses are typically influenced by dust and maritime source (P5L149-150), other physicochemical properties of particles (e.g., mixing state) may play a substantial role over aerosol composition.

Fig. S12: The authors may include the explanation of the difference between open and solid symbols in the figure caption. Please clarify why the solid purple symbol data are not available for s007. Also, why does the highest T data point in s001 at T > -15 dC has a higher c_INP than the next data point at T of < -15 dC?

[Technical comments]

P1L1 vs. P2L35: ice-nucleating particles vs. ice nucleating particles – the reviewer suggests the authors be consistent in this terminology.

P2L47 vs. P11L295: ice nucleation site densities vs. ice-active surface site density – the authors may consider using consistent terminology.

P2L50 vs. P3L72: ice nucleation (IN) vs. ice-nucleating (IN) – please be sure the abbreviation is consistent throughout the manuscript.

P2L55-57: The reviewer is a bit confused here – the point that the authors want to make is that the ice nucleation active (INA) organics are limited in terms of quantity as compared to INA minerals, but it dominates the ice nucleation at T above -15 dC; therefore, they are important, correct? Currently, it sounds like organics are less important as INA component of aerosol particles than minerals by reading this part alone. The authors may rephrase this sentence accordingly.

P6L154: `

P9L256: SIO – abbreviation

P11L306: ice nucleating à ice nucleation active

P19L452: S7 appears after S8 & S9 (P13L372). Please fix the figure number sequence.

P27L611: ??? what does (6-20X)X mean?

P27L615-616: 155,000 in what physical unit?

[Misc. feedback]

P27L620-623: The reviewer likes this statement. This is one of the well-summarized take-home messages.