

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2022-71

Anonymous Referee #1

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Referee comment on "Significant continental source of ice-nucleating particles at the tip of Chile's southernmost Patagonia region" by Xianda Gong et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-71-RC1>, 2022

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In this manuscript, the ambient ice-nucleation particle (INP) concentration and ice nucleation efficiency of aerosol particles collected from the southern tip of South America are presented along with some complementary field observations from three different locations in the studied region. Key findings out of this study are (including but not limited to):

\*A majority of assessed INPs show the indication of proteinaceous component (evidenced by the freezing assay with heat treatment), and

\*the field observations imply that the mixing of large aerosol particles from the continental source with other sources occur in the planetary boundary layer over the study region, and the authors link the observations to their INP data implications (which are detailed in the manuscript).

These findings are relevant to the ACP journal scope as ACP supports many INP-related papers. The INP data from the studied region, especially relatively long-term data as presented in this manuscript, is rare and invaluable. While the field data and offline lab measurements are reasonably well-blended, the manuscript is a bit lengthy, and this reviewer identified some speculative parts (discussed below). For that matter, this manuscript may fit more as the 'measurement reports' manuscript type under ACP, in this reviewer's opinion. While this reviewer does not have a strong opinion to turn this

research article into a different manuscript type, the authors may consider it if the editor and other reviewers address a similar point. Nevertheless, the manuscript is overall well-written, and this reviewer can support the publication of this manuscript (in any manuscript type) in ACP after minor and technical revisions.

### **[Minor comments]**

P1L22-23: Pore condensation freezing can occur below -38 dC as long as the ice saturation condition is met. For that matter, the authors' statement, which sounds limiting the heterogeneous ice nucleation (and/or cloud droplet freezing) to the temperatures above -38 dC seems misleading. The reviewer suggests to the authors to rephrase this sentence to focus on discussing only immersion or clarify it differently.

P2L51: The reviewer agrees with the authors about the importance of long-term INP measurements/monitoring. The authors might want to discuss the importance of highly time-resolved INP data somewhere in this manuscript as well. In the reviewer's opinion, both long time coverage and fine time resolution of INP data are currently missing in the atmospheric science community. The reviewer thinks the time resolution employed for aerosol sampling in this study (P3L77) is valid and the associated offline INP data generated out of a week or two aerosol samples are still invaluable. Some extra discussion regarding the time resolution would add more value to the current manuscript, and the readers will appreciate seeing the discussion of limitations and future outlook.

Sect. 2.1 & P14L330-334: The authors use the meteorological data measured at the airport and connect these to their aerosol measurements and sampling activities carried out at the mountain station, which seems several kilometers apart at least. The authors might want to clarify that the meteorological data were not available at the station in the text. Also, please include a justification and discussion of why they think it is reasonable to refer to the met data at the airport for their studied aerosol properties from the mountain station in the manuscript.

P3L74-76: Please address if any dryers were installed upstream of MPSS and APS. If the authors conducted the 'wet' particle size distribution measurements without a dryer, please address why dry particle diameter measurements, which are typically used for the  $n_s$  estimation, were not carried out and associated limitations (if any).

P3L76: The reviewer wonders why the 800 nm pore size filter was chosen to be used for this study (instead of the one with a smaller pore size). By looking at the particle size distribution figures in Sect. 3, it appears that a substantial fraction of <800 nm particles were measured during the DACAPO-PESO campaign. Later, the authors discuss the key feature of large particles (>500 nm). Therefore, there seemingly is a logical size leap between 500-800 nm. Please elaborate on the discussion/limitation.

P3L85-88: The authors might consider explaining the purpose of keeping samples frozen. What was the typical time span between sampling and offline INP measurements? All INP measurements were completed within a certain amount of time after the sample was taken? The aerosol sampling was done for 1-2 weeks (P3L77), and the filter was exposed to the ambient air and temperature during sampling while the filter is kept unfrozen. Do the authors think such a long sampling interval impacts the degradation of collected aerosol particles over the employed sampling time? Please include the discussion in the manuscript.

P4L91: How did the authors determine the suspending water amount to be 3-4 mL? Based on the set lower detection limit of detectable INPs in the authors' offline assay? Please include the clarification in the manuscript.

P15L346-361: The authors might consider including the specs of the back trajectory analysis method somewhere in the manuscript (even in the method section). It seems that the back trajectory analysis is one of the crucial analytical tools in this study. The

reviewer wonders if the authors can incorporate the air mass height information in Figs. 8 and 11. Do the back trajectory data support that air masses travel through the free tropospheric conditions?

P15L369-370: "A general enhancement of aerosol concentrations ..." This sounds ambitious and speculative. So do the authors are suggesting that the aerosol intrusion from BL bumps up the aerosol concentration? Please detail what's meant by this sentence in the manuscript.

P21L433-435 & P19L403-405: These sentences are provoking but sound very ambitious. While this reviewer agrees that the ground-level INP & aerosol measurements are important, verifying what's stated in these parts (especially on P21) seemingly requires more measurements and evidence to constrain vilifications (e.g., cloud height INP data, vertical distribution of INPs over the studied site, online biological particle concentration measurements, INP measurement in precipitation samples etc.). Perhaps, the authors soften the tone of this sentence and also include the list of limitations and things to do as a future outlook? This reviewer thinks the limitations/outlooks are as important as good scientific results.

P22L472-475: A chemical analysis of the aerosol particles would definitely add some complementary information (at least for the aerosol source identification), but the aerosol composition is not necesarrily the same as the INP composition. How about biological particle composition & rainwater INP & vertical distribution & ice crystal residuals measurement?

**[Technical comments]**

P2L36: INPs – already abbreviated previously.

Figs. 8 & 11 captions: 600 m AGL or AMSL?

P19L402-403: “of all discussed...” The reviewer does not understand this sentence. Please rephrase it.

**[Misc. feedback]**

Sects. 3.2 & 3.4: The reviewer finds the method used to estimate the biogenic INPs contribution (i.e., F<sub>bio</sub>-INP) interesting and useful yet speculative. In future fieldwork in a similar location, the deployment of an online biological particle sensor and linking such online data to F<sub>bio</sub>-INP can even add/solidify more value to this F<sub>bio</sub>-INP idea, in the reviewer’s opinion.