

Dear Reviewer,

We thank you for doing this review and for your suggestions that helped to improve our manuscript. Below, please find your original comments in blue and our responses in black. When referencing page and line numbers, we are always referring to the old version of the manuscript.

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.

We prefer to keep the manuscript as a research article. We think this manuscript reported substantial new results of ice-nucleating particles in the southernmost Patagonia regions, where

long-term measurements are scarce. We present that continental biological aerosol, driven by raindrops, is the main source of INPs in this region, as the reviewer summarized. Additionally, in-situ data were combined with lidar data to examine the extent to which particle (and with this INP) concentrations can be derived from lidar data. This research article includes substantial advances and general implications for the scientific understanding of INP sources and properties in the Southern Ocean. It fits the scope of research articles.

[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.

Thanks for the clarification. We removed "above -38 °C" in the text.

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.

Thanks for your suggestions. We modified this paragraph and added the following in P2L57.

"Many long-term measurements in the past years were done with off-line techniques (such as Schrod et al., 2020; Schneider et al., 2021; Testa et al., 2021), offering a comparably low time

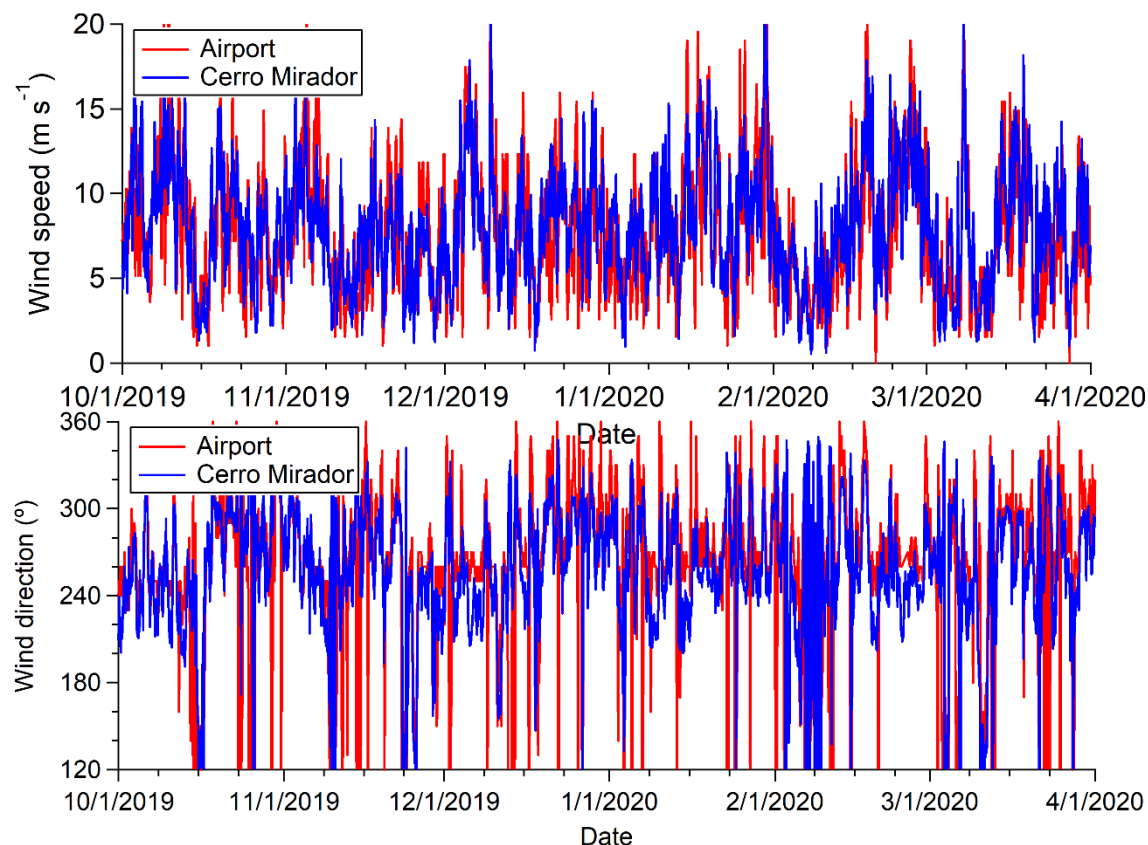
resolution. Nevertheless, highly time-resolved data are also important. However, high time resolution INP measurements are challenging, because they often require the presence of an operator during the measurement. Also, they can only detect comparably high INP concentrations, due to optical detection, and therefore often only yield data at low temperatures or in environments where concentrations are high. Recently, newly developed INP instruments with a higher degree of automation make long-term and highly time-resolved INP measurement more easily available (Bi et al., 2019; Brunner and Kanji, 2021; Möhler et al., 2021)”

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.

We performed wind direction and speed measurements at the Cerro Mirador station, but the measurement only started at end of September 2019. The figure below shows the time series of wind direction and speed at Cerro Mirador station (in red) and the airport (in blue). Wind direction and speed are comparable at both stations. When doing the comparison of meteorology parameters, we always show the probability density function over 7-14 days.

Moreover, we used the cumulative precipitation along the backward trajectory (P_{BT}). Considering the spatial resolution (1-degree latitude-longitude) of the HYSPLIT model, P_{BT} should not show a difference between the two stations. Temperature and pressure are considered to be uniform on a larger scale. Therefore, we assume that met data at the airport can be used for this study. At the end of Sect. 2.1, we included the following:

“These data were used as there was no meteorological data available at Cerro Mirador at the time of sampling. However, based on more recently installed meteorological measurements at Cerro Mirador we know, that wind speed as well as wind direction there are similar to those at the airport.”



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).

Indeed, there was a Nafion dryer installed in front of these two in-situ particle size measurements which we explicitly mention now in the text.

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.

The choice of the pore size was based on a balance between resistance and collection efficiency. It is important to note that 800 nm pore size does not mean that particles smaller than 800 nm are not collected. There is a size-dependent collection efficiency of different pore sizes. We based our choice on the study by Soo et al (2016), who, for the here used type of filter and flow, give a collection efficiency of 96%. Moreover, we compared results for the use of different filter pore sizes (200 nm and 800 nm) on different occasions and got the same results wrt. INP concentrations. This was presented e.g., at EAC 2020 (Abstract title: “Variations in off-line filter sampling and analysis for Ice Nucleating Particle measurements”) and will be included in a publication which is currently in preparation.

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.

Our past (unpublished) experience showed that samples degraded over the course of a couple of weeks when they were not stored frozen (both, at room temperature and also in a refrigerator at +4°C). Related issues were described by a publication by Beall et al. (2020), who recommend storage at -20°C as the method causing the least changes in INP concentrations. We mention this in our text now.

Admittedly, sampling degradation may occur while the filter is being sampled. This, as also Beall et al. (2020) state, may cause artifacts. However, we see rather unexpected high concentrations, so the main tendencies reported in our study are very likely not affected.

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.

First, it is important to mention that the lower the amount of water, the higher the INP concentration in the suspension. High concentrations in the suspensions are better for the INP measurements due to the generally low airborne INP concentrations, in combination with the instrument detection limit. Second, for LINA and INDA measurements and the additional heating test, a minimum amount of water is needed. When starting with our analysis for this study, we considered the INP concentration may be pretty low at Punta Arenas. For the first few test samples, we used the minimum amount of washing water, i.e., 3 mL. However, all of the samples are used up if only 3 mL are used. After testing a few samples, we realized the INP number concentration is high enough to use 4 mL of washing water. From then on, we used 4 mL of water, as this makes handling during the measurements much easier.

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?

We feel that the trajectory analysis does not need an extra section, but added the following in Line 329:

“Backward trajectory analyses were performed with the HYSPLIT (HYbrid Single-Particle Lagrangian Integrated Trajectory) model (Stein et al., 2015), based on GDAS (Global Data Assimilation System) meteorological data.”

We also did an analysis as suggested by you, deriving the frequencies that air mass spent at different altitudes during their passage to our measurement station. These are now included in the manuscript, amending the earlier Figures 8 and 11.

We added the following in L361: “This difference in air mass origin can also be seen in the right panels of Fig. 8, clearly showing that collected air masses spent a higher fraction of time in the free troposphere for sample 11.”

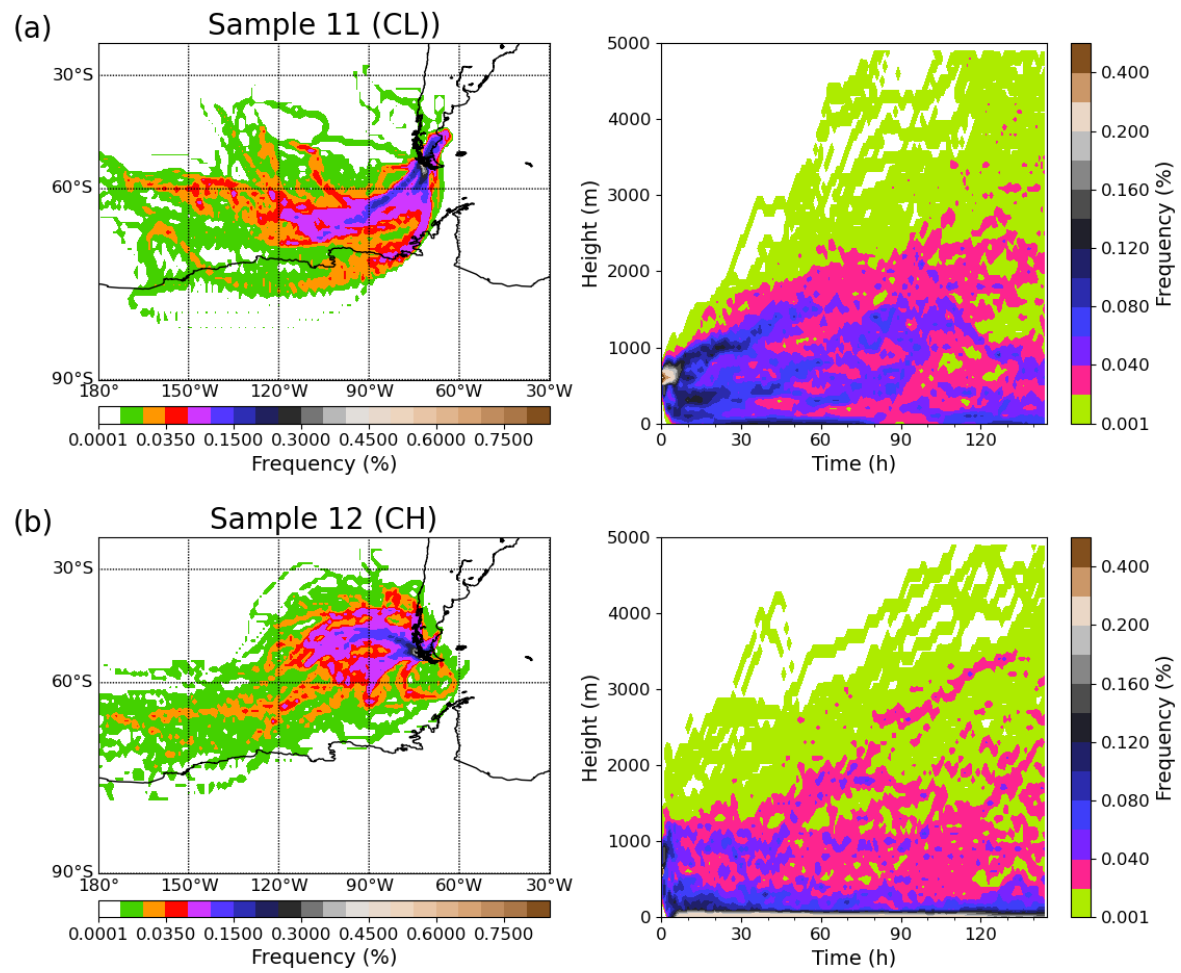


Figure 8. Relative frequency of backward trajectories ending at Cerro Mirador site (600 m a.s.l.) with 1 h resolution, based on a 1° by 1° grid size (left) and the corresponding relative frequency of backward trajectory height (right) during sample 11 (a) and sample 12 (b).

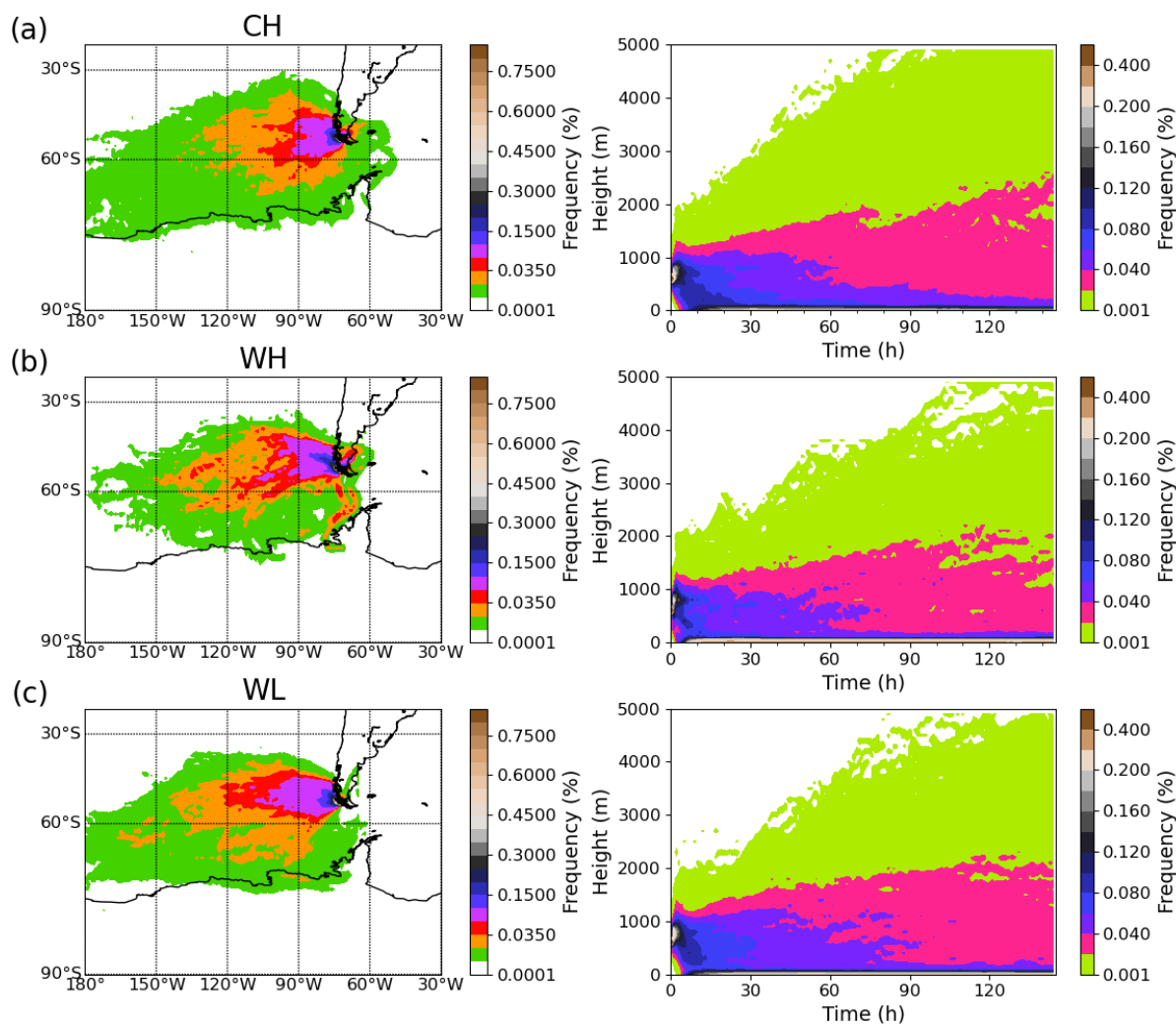


Figure 11. Relative frequency of backward trajectories ending at Cerro Mirador site (600 m a.s.l.) with 1 h resolution, based on a 1° by 1° grid size (left) and the corresponding relative frequency of backward trajectory height (right) during cold season high (CH, panel a), warm season high (WH, panel b), and warm season low (WL, panel c).

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.

The measurement station is at 622 m a.s.l., and it is still inside the boundary layer. We did not mean the aerosol intrusion from BL.

Here we mean the aerosol concentration in the boundary during sample 12 is higher than that during sample 11, which is indicated by the vertical profile of the particle backscatter coefficient.

It is clearly explained in Lines 364-365:

“Also, the particle backscatter coefficient is higher throughout the boundary layer depth and above, indicating a generally higher aerosol load observed for sample 12.”

But to prevent misunderstandings, we replaced “A general enhancement of aerosol concentrations is evident ...” with “Generally, higher aerosol concentrations are evident ...”

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.

We exchanged the word “can” with “may” in the sentence, and added, following this sentence: “However, more research, combining in-situ and offline INP sampling with remote sensing methods is needed before this claim can fully be made.”

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 necessarily the same as the INP composition. How about biological particle composition & rainwater INP & vertical distribution & ice crystal residuals measurement?

We inserted the following:

“Additionally, a combination with measurements of biological particles may be helpful, or examinations of INP in rainwater.”

[Technical comments]

P2L36: INPs – already abbreviated previously.

Changed.

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

It is 600 MSL. Kmsl = 1 when doing HYSPLIT calculation.

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

We changed it to: “ Among above discussed...”

[Misc. feedback]

Sects. 3.2 & 3.4: The reviewer finds the method used to estimate the biogenic INPs contribution (i.e., F_{bio}-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_{bio}-INP can even add/solidify more value to this F_{bio}-INP idea, in the reviewer’s opinion.

Thanks for this comment and hint.

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