Comments on review 1: Please find below our answers to Review1's comments (answers highlighted in yellow).

First, we would like to thank the Reviewer for the careful reading of our manuscript and for the constructive comments.

Review of "Cloud occurrence, properties, and ice crystal effective dimension parameterization from nine years of far-infrared observations on the Antarctic Plateau", Fabbri et al, submitted to ACP.

This manuscript uses retrievals from the REFIR-PAD data collected at Dome C in Antarctica to develop an ice cloud particle size parameterization. The data used in the analyses are unique, and are an important description of cloud properties in this poorly understood, but climactically important region. I think the topic is quite relevant to ACP, and the work is an important application of the Dome C dataset.

There are a number of issues with the presentation that I think should be addressed before the paper could be accepted. I think these are all minor issues in that I do not think they require much detailed work to address.

-- There is a large amount of overlap with the Donat 2025 (D2025) manuscript submitted to AMT. For example, Figure 4 appears be the same data as Table 1 in D2025; Figure 5 is the same as Figure 4(b) and (c) in D2025.

-- In addition, I also think that the manuscript is rather long, and there are some figures that could be omitted. This, I would suggest substantially reducing section 3, and simply referring to D2025 when warranted.

-- Finally, I also think the primary new work in this paper is the analysis of the cloud properties themselves, and the new parameterization, not the occurrence statistics (as that was done in D2025). Thus, I think the paper can be shortened and focused on the novel aspects.

-- To address the above three points, I would suggest removing Figures 4 and 5 as these duplicate information in with D2025. I would also suggest removing Figure 3, as it is not central to the paper. This would also mean substantial text could be removed from Section 3.

We agree that Section 3 presents some overlap with Donat et al. 2025 (hereafter D2025). We have revised the manuscript to reduce the overlap and to focus the paper on its novel aspects (the cloud property analysis and the new parameterization). We address each specific point of your comment here below.

Figure 4: We agree with the Reviewer. Figure 4, representing the seasonal cloud occurrence, was inserted to introduce the general context rather than a new result. Nevertheless, we agree that it could be considered redundant, and we decided to follow

the Reviewer suggestion and remove it. In the new manuscript we will now refer the reader to Table 1 of D2025. The current text on Figure 4 will be removed accordingly.

Figure 5: We would like to clarify that Figure 5 reports a result that is not present in D2025. The analysis method is the same as in Fig. 4(b) and (c) of D2025, but it is applied to a different dataset: D2025 considered all cases identified as cloudy by the CIC algorithm, while in the present manuscript we restrict the analysis to those classified as ice clouds only. The novel result is that for ice clouds only, the semi-annual cycle is still present, while the annual cycle (which is reported by D2025 for total cloud occurrence) disappears. In the interest of keeping the focus on the central aspects of the manuscript, we will remove the figure and describe this result briefly in the text.

Figure 3: We prefer to retain Figure 3 since it illustrates the temporal coverage of the measurements in both summer and winter and provides a general overview of the mean sky conditions.

-- I generally agree with the methodology in the retrieval setup, but there are several very important simplifying assumptions. One key assumption is that the cloud is a homogeneous layer of solid single ice crystals. It would be valuable to know how sensitive the results are to this assumption, for example repeat the retrievals with a different habit assumption and quantify the difference in retrieved cloud properties (similar to what was done in Maestri 2019, doi:10.1029/2018JD029205). If the authors argue that is out of scope for this paper, that is fine, but I think these assumptions need to be emphasized more clearly. Right now, those important assumptions are described in the methodology (lines 219, 230-234). These should be repeated and emphasized again in the conclusions, probably expanding the paragraph at line 480.

The reviewer is right to raise this point; clearly, the assumption regarding the crystal habit impacts the uncertainty of the retrieval parameters, namely, effective diameter (Dei) and optical depth (OD). This impact has already been evaluated for some specific cases in Maestri et al 2019 (doi:10.1029/2018JD029205) and also described and quantified in a previous work by Di Natale et al. 2020a [1], in which retrievals were repeated on a four-year dataset of REFIR-PAD measurements by varying three crystal habits (complex aggregates, bullet rosettes, and plates). The results showed that for OD smaller than one and Dei smaller than 20 μm , the impact of the shape assumption is small. The difference observed in the retrieved OD is on average -11%, with the largest differences down to -20% for large ODs. The difference in the retrieved Dei is on average -50%, increasing to ±100% for Dei larger than 15 μm.

We will discuss this briefly in the new manuscript. "The effect of varying crystal habits on the retrieval results was previously evaluated for some specific case studies in Maestri et al. (2019) and analyzed and quantified in Di Natale et al. (2020). This last study tested three distinct ice particle shapes, namely complex aggregates, bullet rosettes, and plates, across four years of REFIR-PAD observations. The findings showed that shape assumptions have a minor effect when optical depth is smaller than one and effective diameter smaller than 20 μm. Specifically, mean discrepancies in retrieved OD sat around -11%, though they stretched up to -20% for large OD (> 1). Meanwhile, variations in Dei averaged -50%, with fluctuations reaching as high as 100% for particles exceeding 15 μm.”

[1] Di Natale G, Bianchini G, Del Guasta M, Ridolfi M, Maestri T, Cossich W, et al. Characterization of the far infrared properties and radiative forcing of antarctic ice and water clouds exploiting the spectrometer-Lidar synergy. Remote Sens 2020;12. doi:10.3390/rs12213574.

-- One minor methodological issue, in the discussion of the parameterization performance, several statistics are computed to evaluate the 'goodness of fit' (Table 7). One of these statistics is the bias - presumably this is the mean of residuals between the data and fit. Note that the mean residual of a least squares fit is zero by definition. The small number reported for bias, in Table 7, is therefore not meaningful and should be omitted. (The nonzero value is floating point roundoff error propagated through the bias calculation).

We thank the Reviewer for this correct observation. Since the parameterization is fitted by ordinary least squares, the bias is zero by construction and carries no information on the quality of the fit. We will remove the bias column from Table 7 and revise the corresponding text accordingly.

-- On data availability: The new and novel data used in this manuscript is not the remote sensing datasets (this is described in several other papers, e.g. D2025 among others), but rather the cloud property retrievals from the REFIR-PAD. My reading of the Copernicus data policy (https://publications.copernicus.org/services/data_policy.html) is that data release is not required, but encouraged. Therefore I would encourage the retrieval data to be shared publicly. The editor could also give their opinion in this case.

We decided to not share the full dataset of products at this time since the writing of a paper to be submitted to Earth System Science Data (ESSD) for the publication of the REFIR-PAD Level 2 data analysis database is already underway. The database will most likely be made public on Zenodo with a DOI and will contain all atmospheric profiles as well as cloud optical and microphysical parameters for the period from 2012 to 2022 for now (10 years). The submission will take place as soon as possible.

The scene classification performed by the cic algorithm for the time range reported in the paper will be shared at the next submission of the manuscript.

Other minor corrections, suggestions, needed clarifications:

Line 45: I think the "broad temperature range" for the ice clouds is incorrect, doesn't Figure 8 show many cases below -40 C? It also seems strange to describe the mixed phase cloud range as "narrower" when it is only 2 C warmer. But, I think the ice cloud range should contain much colder temps.

The Reviewer is correct. As shown in Fig. 8, ice clouds occur down to about −60 °C, so the temperature range reported in the text was wrong. We will correct the text in the manuscript accordingly.

Line 156: "The criteria adopted for the selection of the spectra consist in the individuation of neatly defined cases observed by the lidar."

This sentence is unclear, can you rewrite?

We agree that the original sentence was unclear. The intended meaning was that the spectra were selected through visual inspection of the co-located lidar profiles, retaining only the cases for which the lidar classification was unambiguous. We have rewritten the sentence, merging it with the preceding one to reduce redundancy and clarify the selection criterion.

Original text:
"The spectra are selected by visually inspecting the co-located lidar backscattering and depolarization profiles. The criteria adopted for the selection of the spectra consist in the individuation of neatly defined cases observed by the lidar."

Revised text:
"The spectra are selected using the co-located lidar backscattering and depolarization profiles, considering only cases that can be confidently assigned to a single class by visual inspection."

Figure 1: This would be better represented as a table, as there are not very many values. If the authors insist on keeping this figure, a different colormap is needed, as the Ice Cloud - ThS is not readable (black on dark blue).

We agree that a table is more appropriate in this case, and we will replace Figure 1 with a table in the revised manuscript.

Line 288: Is "surface temperature" the surface air temperature, or surface skin temperature?

The quantity used is the near-surface air temperature, not the surface skin temperature. We will state this explicitly in the revised text.

Figure 7: Please explain the reason for ~25% of the data that lack co-located LIDAR data; is that simply due to instrument outages, or are these cases that the LIDAR fails to detect?

The percentage refers to cases where the lidar measurement was affected by disturbances due to internal reflections within the instrument, which were subsequently resolved but unfortunately made the measurement unusable. A clarification will be added in the revised manuscript.

Line 320: I thought that the ice crystal size was assumed to be constant (cloud properties are homogeneous in the layer); so why does the mean value of D_eff need to be computed?

The reviewer is right, we provided a wrong nomenclature; the effective diameter is not obtained from an average process, so it should not be in parentheses. This will be corrected throughout the revised manuscript.

Line 364: How was the D_eff computed in the mixed phase cases? Is it weighted by the optical depths, or something else?

In mixed-phase clouds, the two phases are kept separated and never combined into a single effective dimension. The SACR retrieval treats them separately, providing two distinct quantities: the effective dimension of the ice crystals, ⟨D_eff, ice⟩, and the effective diameter of the water droplets, ⟨D_eff, water⟩. Their distributions are reported in separate panels in Fig. 9. In the right panel of Fig. 10, the values plotted for the mixed-phase points (triangular markers) correspond to the water droplet effective dimension, ⟨D_eff, water⟩. This point will be stated more explicitly in the figure caption and legend of the revised manuscript.

Figure 13: This plot is confusing to me. It could be omitted entirely, as the only new information is that the residuals do not strongly depend on T_cloud or IWC, and I think Figure 12 already shows this - the spread of the distributions does not vary much across the T_cloud in and IWC variables in each plot. If the authors want to keep this figure:

The boxes with the sample counts are hard to read (please choose a lighter background color). The boxes could also be omitted, as this is not new information, as the count histograms are already in Figure 8. The bin widths are chosen at seemingly random values, it would help to bin by regular values (2.5 or 5 K for T_cloud, 0.25 in log IWC) which would make it easier to visually compare to other plots (e.g. Figure 12). The y-ranges should be significantly narrowed to be able to more clearly see the variation that is there (+/-25 um).

We agree that Figure 13 adds limited new information; we have therefore decided to move it to the Appendix and briefly comment on the negligible conditional bias of the parameterization directly in the text. For the revised manuscript, we have also improved the figure by removing the boxes with the sample counts, binning the data at regular intervals (2.5 K for $\langle T\_cloud \rangle$ and 0.25 in log IWC), and narrowing the y-axis range to better display the actual variation of the residuals.