In their revised submission, the authors have addressed my previous remarks satisfactorily, and the manuscript has improved considerably. The value of this study and its contribution to the field are now clearer. I recommend acceptance after addressing the minor issues noted below. Line numbers refer to the marked-up version of the paper.

Line 47: This sentence is not grammatically correct. Consider starting with e.g. "The fact that…"
The sentence has been corrected accordingly to the reviewer suggestion.

In the introduction of the revised manuscript I'm missing a high-level description of the ground-based aerosol data and the satellite-based cloud retrievals that the authors combined for their analysis. In fact, the first sentences of Section 3 (lines 281-284) could fit better in the introduction for this purpose.
The section 3 sentence has been moved to the introduction.

Please also consider moving Table 1 before the technical description of in situ measurements (section 2.1.1).
A short introduction to section 2 has been added before section 2.1.1 and citing the table 1 : 'The observational dataset used in this study covers the 2020–2024 period and combines complementary in-situ and remote sensing measurements acquired from ground-based and satellite platforms (Table 1). The instruments and datasets contributing to this observational framework are described in the following sections.'
For editing reasons, the table 1 is now before the section 2.1.2.

Line 197: "and the retrievals are therefore more suitable for co-location with ground-based observations." I would say that the main advantage of geostationary observations is their high temporal frequency. With the term "suitable", I assume that the authors mean that colocation of ground measurements with geostationary observations is computationally faster, since it is computed only once?
Yes the reviewer is right and this sentence has been replaced by : "On the other hand, instruments on geostationary satellites generally provide lower spatial resolution but offer a fixed viewing geometry and high temporal sampling, enabling frequent observations of the same region and facilitating the temporal co-location of satellite retrievals with ground-based measurements. »

Line 216: It is Bennartz and Rausch 2017, not 2003.
Yes you are right and it has been corrected

Line 324: "stability filter" should read "stability filters".
Done

Line 401: "4% of the original cases". Table 2 and line 396 mention 3%. Please check.
This has been corrected.

Lines 413-415: It is mentioned that ~40% of the filtered dataset comes from 2023, but the corresponding number in the supplement is "~45%". In fact, visual inspection of Fig. S2f

suggests that this number should be even lower than 40%. Please check and consider providing numbers in the S2f plot.

The reviewer is right. The figure presented in the reviewed version corresponded to unfiltered data. The proportion of the unfiltered data from 2023 is 29% while it increased to 44% for filtered data. We apologize for this mistake. It has been corrected in the manuscript and in the supplementary material.

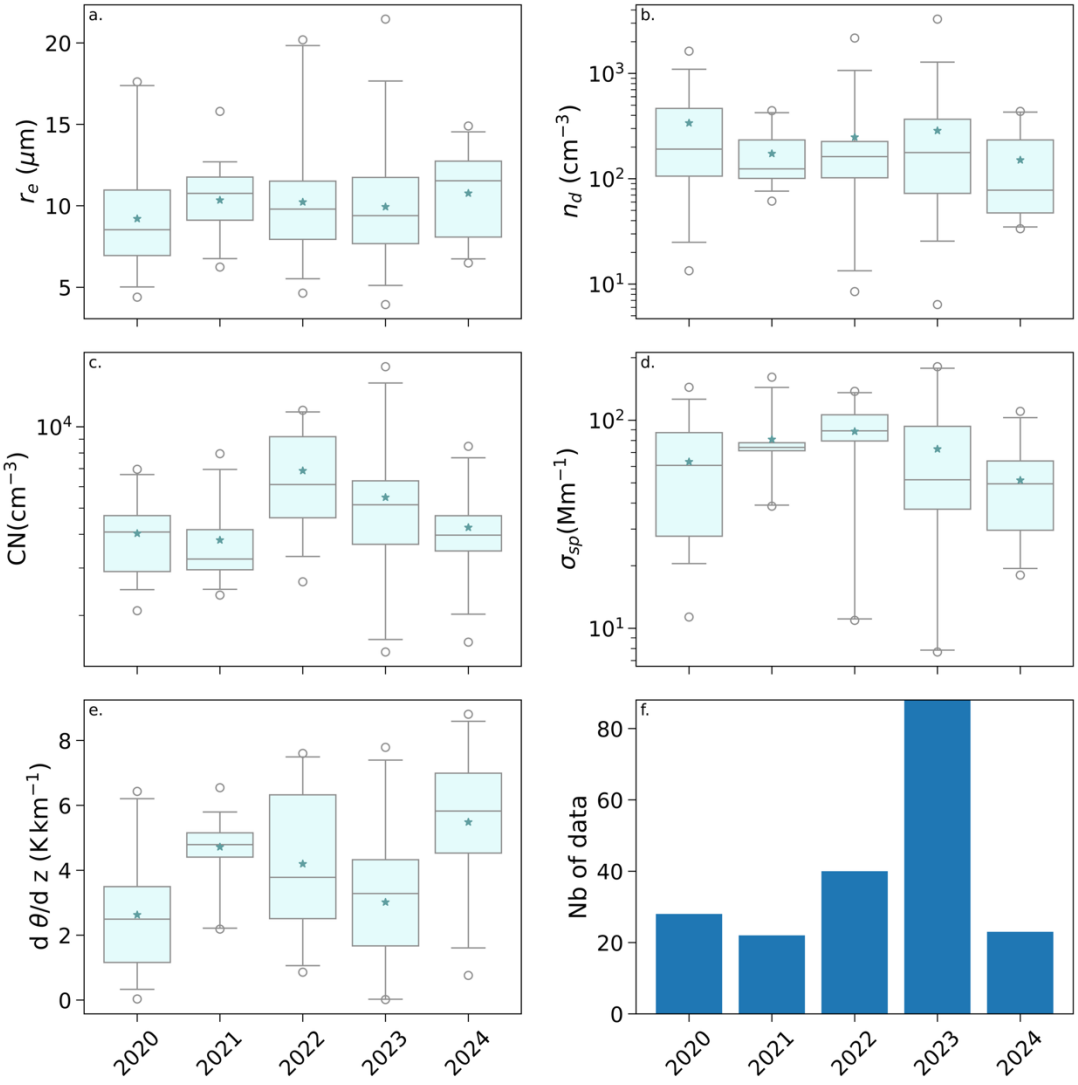


Figure S2 : Interannual variation of (a) cloud droplet effective radius (r_e), (b) cloud droplet number concentration (n_d), (c) particle concentration (CN), (d) dried aerosol light scattering ($\sigma_{sp,525nm}$), (e) potential temperature vertical gradient ($d\theta/dz$), and (f.) the number of observations (Nb of data) over the ATOLL station when all filters described in section 3 are applied. For subfigures a to e, the box represents the interquartile range (25th–75th percentiles), with the median indicated by a horizontal line and the mean by the star. Whiskers extend to the 1st and 99th percentiles and outliers of the whiskers are shown as individual points.

Line 448: "is estimated" should read "are estimated".

This has been corrected.

Figures 5 and 7: The authors mention a vertical dashed line but there is only a horizontal one.

We removed both sentences from both captions. Indeed, we at first wanted to highlight the physical limits of both ratios. However, in this study, as aerosol loading is characterized by the aerosol scattering coefficient rather than by CCN number concentration, values greater than 1 or 0.33 are physically possible and should not be interpreted as indicative of an error in the calculation.

Line 611: “similar cloud scenes” should read e.g. “cloud scenes are similar”.
The sentence has been corrected accordingly to the reviewer suggestion.

Reviewer 1 :

The authors mostly responded well to my comments. Three points need to be clarified further in the revised manuscript:

1) The authors claim they constrain the data to shallow stratiform boundary-layer clouds. However, none of their criteria singles out stratiform clouds. For the statements to be correct, "stratiform" needs to be omitted.
We thank the reviewer for this comment. We agree that we do not apply a direct mask for stratiform cloud type. However, we believe the term remains justified given the combination of criteria used: the clouds are embedded within the boundary-layer height in a stable atmosphere, and are observed at coarse satellite spatial resolution with low retrieval uncertainty on the cloud properties. This low uncertainty indicates that cloud properties are homogeneous within the pixel, which is consistent with a stratiform structure. We nonetheless agree that this should be stated, and propose adding the following clarification at line 267:
“Only cases with positive gradients, indicative of thermodynamically stable stratification with respect to a saturated adiabat, were retained for further analysis. By selecting cases that were convectively stable, and additionally excluding all clouds with a geometric thickness greater than 1 km, it is assumed that cumuliform clouds are removed from the sample.”
We also remove the term stratiform before this is mentioned.

2) The authors explained to me in their reply why the statements about fractions of remaining data are diverging (since they are reported individually). This needs to be clarified for any reader in the revised manuscript as well.
This explanation has been now added by the end of the methodology introduction : « Each criterion was applied independently to the complete dataset to quantify its individual effect on the available sample and to identify the most restrictive selection criteria. This approach was adopted to assess separately the impact of each physical constraint—cloud-type selection, boundary layer height, and satellite–ground consistency—without conflating their respective effects. Independent filtering thus provides a transparent assessment of the sensitivity of the resulting dataset to each selection criterion and the underlying physical assumptions. Once the individual impact of each criterion has been assessed, all filters are applied simultaneously to define the final dataset retained for the subsequent analysis. This resulting dataset constitutes the observational basis for the investigation presented in this study. »

3) The authors pretend $S_n > 1$ might make sense if one uses an aerosol proxy that is not well related to CCN concentrations. This explanation needs to be added to the revised manuscript.

This paragraph has been added to the section 4.1 when Sn is defined : « When defined as the ratio of cloud droplet number concentration to CCN number concentration, the activation fraction is physically bounded by unity, as the number of activated droplets cannot exceed the available CCN. However, in this study, aerosol loading is characterized by the aerosol scattering coefficient rather than by CCN number concentration. As an optical proxy for aerosol loading, the scattering coefficient does not represent a particle number concentration and therefore does not impose an upper bound of unity on the resulting ratio. Consequently, values greater than 1 are physically possible within the formulation adopted here and should not be interpreted as indicative of an error in the calculation. »