Dear Reviewer #2,
We thank you very much for carefully reading our manuscript and for your comments and suggestions. In this reply letter, your comments are given in **black** and our answers are written in **green**. The given line numbers refer to the revised version of the manuscript (without markup).

Major comments:

Maximum diameter is generally one of the most ambiguously defined parameters for highly complex particles, such as ice or snow. I am of course aware of the fact that Dmax is widely used in retrievals, model parametrizations, and in-situ observations. But I am missing in the paper a discussion that Dmax has been defined in the past in very different ways. For example, most airborne in-situ probes derive Dmax as the size of the circumscribing sphere or spheroid.

> —> We thank Reviewer #2 for the comment regarding our selection of dmax. It convinced us to review the existing 2DVD-based dmax retrievals, to determine and mention the dmax approach in the remote-sening-based retrieval LIRAS-ice, and, most importantly, to conduct a comparative study between the maximum Feret diameter and the circumscribing sphere diameter. The revision work that was put into this topic is elaborated on in the following comments of this reply letter, as well as in the reply letter to Reviewer #1, who raised similar points. In the introduction, the following sentences were added: “However, the particle size has been defined in the past in very different ways. Alone in 2DVD studies, various definitions of the diameter such as equivalent sphere diameter (e.g. Lee et al., 2015), maximum diameter ('maximum value of width and height seen in both cameras'; Bernauer et al., 2015), or area diameter (defined by the area with smallest circumscribed ellipse; Huang et al., 2015) were used. From a physical perspective, the maximum dimension is the distance between the two outermost points of the particle.” (lines 65-69)

You decided in your manuscript to use the Feret diameter instead. I would like to see a comparison of your “Feret-Dmax” to more common methods, for example, the maximum size of a circumscribing ellipse.

> —> We decided to introduce dmax describing the maximum dimension between the outermost points of a particle (from a certain perspective) because we see this as the physical maximum extent. A circumscribing sphere diameter, on the other hand, would for example exceed dmax in case of an equilateral triangle. Nevertheless, we now introduced the circumscribing sphere diameter dc and compared it to dmax in Fig. 5. It was found that dmax and dc are indeed nearly identical in case of spheres. We also compared dmax against dc for both case studies and found that dmax and dc are nearly identical (Fig. R1).

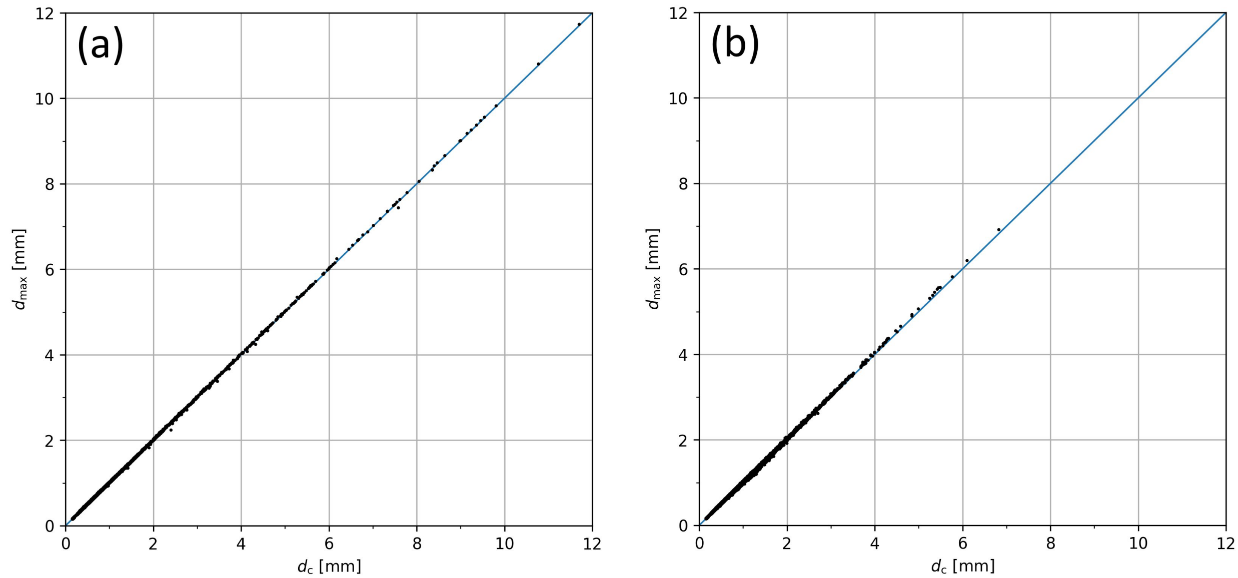

Figure R1: 2DVD dmax versus dc (a) on 17 January 2023 during PolarCAP/Cloudlab in Eriswil, Switzerland and (b) on 10 November 2019 from 11 to 15 UTC during MOSAiC.

You also don't comment on how Dmax is defined in the remote sensing retrievals that you are using in the intercomparsions later. In the worst case, discrepancies in your comparison of the retrieval results and the 2DVD might at least be partly due to different definitions of Dmax. I think this aspect should be discussed much more.

—> Thank you for pointing out this important aspect. In LIRAS-ice, the maximum diameter is defined as diameter of the smallest enclosing sphere. We added the sentence "The LIRAS-ice-retrieved maximum diameter is defined as the maximum distance between two points in the ice crystal from the zenith-pointing perspective and can therefore be directly compared to dmax from the 2DVD." in lines 313 to 314.

I have some problems with Fig. 5 and the discussion of it in the text: You have steel spheres with well-defined diameters as calibration objects. Wouldn't it make sense to produce two sub-plots with D(sphere) vs Deq and a second one with D(sphere) vs. Dmax? You say in L. 307 that "Deq agrees better than Dmax with the true diameter". But how can the reader judge that if you don't show the true diameter? With two sub-plots you could also plot the data as box-and-whisker plots that can much better illustrate the distribution of points (in your current Fig. 5 it is hard to estimate how many particles are clustering together). You write in L. 307 that Dmax overestimates the true diameter by 0-10% for diameters larger than 2mm. If I look at Deq=6mm or 8mm I can see several points being 2-3mm in Dmax away from the 1:1-line (those points exceed your 10% a lot). Why does Fig. 5 show a general (systematic) overestimation for Dmax in comparison to Deq? For perfect spheres I would expect the points to be around the 1:1 line. You say that Dmax is defined in a way that it has to be larger than Deq, but this is not clear to me in the case of spheres. Finally, are you applying a correction to the 2DVD data based on your calibration results? Maybe you wrote this in the text and I missed it.

—> We agree that the plot was not optimal for judging about the two variables. We applied major changes to that part, also following the comments from Reviewer #1. Firstly, we did the calibration again under very calm conditions meaning no wind at all. We also excluded around 15 data points from this new dataset for two reasons: 1. because either spheres were sticking together (although the calibration was done very carefully, seems one can hardly avoid that with the designated method) and 2. because some spheres were not fully detected (very few spheres seemed to be partly detected on the edge of the measurement area although this should not happen, according to the manufacturer). Further, Fig. 5 was modified in a way that, following your suggestion, box and whisker subplots were introduced. Additionally, the new variable Dc was implemented. Eventually, we did not apply a correction to the 2DVD dmax data based on the calibration results because the results of the (second) calibration were, on the one hand, fluctuating, and, on the other hand, regarded as sufficiently accurate with arithmetic means not differing by more than 3-5% from the actual sizes. Moreover, we think that an additional construction from which the spheres (of all sizes, not just the 10mm ones) can be dropped would improve the results even more, as the spheres can still get a horizontal motion component when being dropped by hand. We majorly modified the results and discussion part about the calibration (Section 4.1, lines 330-339 and Section 5, lines 436-441).

Minor comments and typos:

1. L 137: “a one-dimensional particle shape” Shouldn’t this be two-dimensional?

—> Yes, sure. “one” was changed to “two”.

2. L 187: “For the display of single particles, an own programme was written”. By whom? The authors or the manufacturer? How is the programme called and where can the reader find the programme? It is not mentioned in the Code availability section.

—> The programme was written by us and is now available on Zenodo. This statement was shifted to the caption of Fig. 4 and is also contained now in the code availability section.

3. L 202: Ice density should be 916 kg/m³. Shouldn’t the equivalent volume actually taking the much lower density of snow into account? I mean, if you assume the snowflake volume to be composed of pure ice, the precipitation rate will be huge, or?

—> This is true. We now implemented the formula of Zhang et al. (2021) who investigated the particle bulk density in dependence on median volume diameter (Deq,0). This procedure is now explained as well in Section “2.3.4 Precipitation properties”. We recreated figure 9 with an updated precipitation rate. However, the results are only slightly differing from those with an assumed rho of 0.9. This is because the density derived from the formula of Zhang et al. had to be capped at 0.917 g/cm³ for very small particles. This value is approximately reached if particles have a Deq of 0.1mm and smaller which was the case during the seeding events.

4. L 235-236: How exactly are the parallel tangents drawn around the particle image? Are they supposed to be perpendicular to Dmax?

—> No, the parallel tangents are independent from Dmax. They are drawn in a way that their distance (= minimum Feret diameter) is as small as possible. Their locations are also an output from the python feret module.

5. L 239: I would say you need a very trained eye to see a dendrite in Fig. 4

—> From our extensive 2DVD data analysis we concluded that this particle is a dendrite as the six bulges are visible in both camera images.

6. Fig. 4 (caption): There is no thick red line in the figures.

—> “red” was changed to “black”

7. L 267-268: I am surprised about your statement that columnar particles would fall slower than plate-like particles. For example, if you look at Fig. 6 in Mitchell, JAS, 1996 you see that columns fall way faster than plates or dendrites. Please revise.

> —> This is in theory true. However, Bühl (2014, dissertation; Figure 3.4) showed with the experimental-based method of Heymsfield and Westbrook (2010) that the fall velocity differences of rimed long columns, aggregates mixture, and hexagonal plates with sizes of 1 - 2 mm are smaller than 0.1 m/s. The paragraph was revised and some literature was added. It was also included that both oblateness and fall velocity should be used with care because both are bias-prone due to horizontal orientation and small-scale wind effects, respectively. (lines 276-284)

8. L 282: "This dominant crystal shape has to be presumed in advance" Which is a major weakness of the LIRAS-ice retrieval in my opinion.

> —> Indeed, we see it as an advantage of LIRAS-ice compared to similar retrievals such as DARDAR or Captivate that testing different shapes and investigating the impact on the ice crystal number concentration results is possible.

9. L 295: "with further assumed shapes" Do you mean different shapes?

> —> Exactly, this is what we did. We changed "further" to "several other".

10. 296: "require further requirements" Consider different wording.

> —> "suited to conduct such case studies require further requirement" was changed to "suitable for conducting such case studies requires additional criteria"

11. 324: You say that particles with O<0.6 which are shown in Fig. 8d can be assumed to be columns. This is completely unclear to me: If you observe a horizontally oriented plate or dendrite, its height will also always be smaller than its width (so small O). I would argue that you cannot reliably distinguish between columns and plates based on O.

> —> This is true. The reason for the statement was, that during most of our 2DVD analysis, dendrites or plate-like particles could often be identified as such because they are rarely horizontally aligned, likely due to wobbling. This would also increase O significantly. To account for the possibility of horizontally aligned particles (and thus, to soften the statement), the main body was slightly changed:
> L. 347: "may" was added: "clusters which ["may"] represent different particle types".
> L 351: "were also found to fall significantly slower" was changed to "were also found to fall more slowly on average"
> L. 352-354: "many" was inserted: "[many] particles with O ≤ 0.6 can be assumed to represent columnar crystals" and the sentence "Due to the low *v* it is possible that further particle types, such as small horizontally oriented dendrites, are also contained in this cluster." was added.

Even more confusing is the following: The #4 particle in Fig. 8d is identical to your example particle in Fig. 4 which you describe there as a dendrite (!). Also particles #5 or 6 in Fig. 8d look to me much more like aggregates than columns. Please clarify.

> —> L. 356-357: "many plate-like or columnar crystals and also aggregated particles" was changed to "dendrites, plate-like crystals, columns, and also aggregated particles"

12. Figure 7: I stumbled over the 20dB offset between the MIRA and the W-band radar. This offset is huge, so I would like to know a bit more about it. Which of the two radars had the "malfunction" and was it related to the offset? Was one of the two radars properly calibrated and

how? I assume your Dmax remote sensing retrieval depends on the absolute value of Ze, hence a discussion of this aspect is quite relevant.

—> To provide more explanation, the sentences “The offset occurred due to a malfunction of the RPG radar blower, followed by accumulating snow on the radome causing attenuation. The MIRA cloud radar, on the other hand, only measured intermittently during the relevant time period for which reason RPG radar data are shown.” were added in the caption.

13. L 329-330: What is the sampling area of HOLIMO. Up to what maximum size can particles be reliably detected by HOLIMO?

—> HOLIMO’s ability to detect large particles is limited by the relatively small measurement volume of $12 cm^3$. However, 2mm can be considered an approximate upper threshold (see Section 2.4, line 225).

14. Figure 9a: “Z” is defined as the 6th moment of the raindrop size distribution. As you are observing ice particles, you actually show the effective radar reflectivity factor which is usually denoted as “Ze”.

—> “Z” was changed to “Ze” in Figure 9, 7, and as well as at two further positions in the main body.

15. L 372: You mention in the article several times (for example, in the abstract) that LIRAS-ice is evaluated “for the first time” against ground-based in-situ observations. I would assume that a proper evaluation of a remote sensing retrieval is presented in the same publication as the retrieval itself. I have the impression that you want to convince the reader how innovative your evaluation in this section is. I would suggest to leave that judgement to the readers or at least mention it only once.

—> “for the first time” was removed here.

16. Figure 10 (caption): Consider including a reference to Fig. 9.

—> The sentence “Time series of cloud radar profiles of Ze and LDR, as well as 2DVD N and R are shown in Fig. 9.” is now included.

17. Figure 11 and discussion: I see in panel b) a systematic underestimation of at least 15% of N by LIRAS-ice for all assumed shapes. How big is the uncertainty of LIRAS-ice in general? How much would a realistic uncertainty in radar calibration of +-1.5dB affect your N and Dmax estimate? This should be discussed.

—> Thank you for pointing out this aspect. Bühl et al. (2019) are pointing out that determining the uncertainty for the ice crystal number concentration (ICNC) is very challenging and based on different assumptions. For the diameter, no specific uncertainty values are given. We added the following text in lines 317 to 320 Section 3.3): “The determination of uncertainty for the retrieved ICNC is complex and based on a number of assumptions, for example, about particle type, size distribution and mass– or velocity–size relationships. Further uncertainties of Z and E additionally propagate into the final ICNC uncertainty. According to Jimenez et al. (2025), the ICNC uncertainty, considering lidar and radar measurement errors as well as model errors, is about a factor of 2-3.”. Also, we changed in the Results Section “The ICNC derived under the assumption of bullet rosettes agrees with the 2DVD N within one order of magnitude for most of the time.“ to “The ICNC derived under the assumption of bullet rosettes and 2DVD N agree within the given the LIRAS-ice ICNC uncertainty, i.e. a factor of 2–3

(Jimenez et al., 2025), for a substantial fraction of the time." (lines 417-419). In the Discussion Section, we added "ICNC and N values agree within the LIRAS-ice uncertainty for a substantial portion of time." (line 453).

18. L431 + L. 392: In L. 431 you mention a temporal shift of 2 minutes but in L. 392 you say the shift is only 1 minute (60s). Please clarify.

—> 60 seconds is the right value, so "two minutes" is replaced by "60 s".

19. L430: How can you estimate the vertical wind shear from Fig. 11a? If you infer this from the shape of the fall streaks, you have to assume particles with identical vertical velocity over time.

—> This is true. Following a comment of Reviewer #1, we took a look into the radiosonde wind data of this day. We firstly investigated vertical directional and velocity wind shear during the investigated period and we secondly found that the wind direction at the lowest LIRAS-ice height bin (180m) changed by 30° during that time period. We added the data source (Maturilli et al., 2021) in line 424 and in the data availability part. We changed "Figure 11a" to "Radio soundings from that day [...] (Maturilli et al., 2021)" (lines 465-466).

20. L433: "the statistical results (…) are biased by the varying vertical wind shear" Why don't you test the impact of different time shifts on your statistical results? It sounds plausible but it would be more scientific to show it.

—> In fact, the performed cross-correlation worked in a way that every possible time shift for the whole period was tested. The time shift which led to the highest correlation (which is -60 seconds) was then used for the calculation of the statistics. In a reply to a similar comment by Reviewer #1, we elaborate further about potential influences of directional and velocity wind shear on the correlation.

21. L 435-436: "in order to make measurements better comparable to precipitation data by other instruments". I agree, but then your definition should be comparable with the more common definitions of Dmax used by those instruments. Or at least you should show and describe possible differences (see also major comments).

—> As described under the major comments, we showed the similarity of dmax and dc. Therefore, we believe that the statement is appropriate here.

22. L 461: I can imagine that wind deteriorates the 2DVD calibration. But why is no wind-shield used during calibration? The calibration "table" has no side walls that could hold off wind coming from the side

—> As mentioned under the major comments, another calibration was performed under wind-free conditions, which improved the results a lot.