Response to Reviewer:

We thank and appreciate the reviewers for their valuable scrutiny and feedback on our manuscript and the opportunity to submit a revised version. All the suggested changes and feedback have been duly addressed and implemented, with our responses for each comment presented as follows.

The major changes are:

1. A supplementary material has been added alongside to the revised manuscript containing the validation of the estimated cloud base and LWP calculations.

2. The CFAD analysis is completely revised.

3. The wording of the manuscript has been significantly improved to remove redundancies and increase clarity. Additions have been made as per reviewer suggestions.

Note: line numbers must have changed in the revised manuscript.

Reviewer 2 Comments:

1. The authors should specify the purpose of this study, for example to identify cloud boundaries in the absence of lidar. In order to provide more clarity on the paper's objective.

Response: The objectives of the study are as mentioned in the introduction section: we aim to use a combination of both in-situ and radar-based measurements during SOCRATES to develop a new method to classify the MBL cloud phase and determine cloud boundaries over the SO for low-level clouds, to answer the following questions:

- What are the dominant cloud types, their associated cloud phase, base and top heights, and their vertical distribution?
- What are the phase-specific macrophysical properties for SO low-level clouds?

In this study, however, the radar + lidar signals has an overlap region of only ~11% (as observed from this study based on a total of 15 research flights) when considering the entire 3D time-height profiles where lidar attenuates easily along the vertical. Radar signals contribute to ~84% of the entire valid cloud signal retrievals (3D profiles) while lidar contributes to ~16% (this is based on the valid cloud reflectivity and backscatter measurements as defined by Shupe., 2007). However, along the time axis the overlap is almost 100% hence a direct comparison of our dominant phase results and Shupe (2005), Initrieri (2002) method based on median lidar PLDR (and Backscatter coefficient) applied to the SOCRATES dataset as in Table 4b was possible (since both are retrieved as 1D time-series profiles).

2. I would advise authors to estimate uncertainties more accurately, simply by adding statistical parameters (standard deviation on figures), for example. For in situ probes and parameters, refer to more articles highlighting measurement uncertainties and uncertainties related to derived parameters. Above all, to show that in situ accurate and real, but is nevertheless marred by uncertainties.

Response: The values presented in Table 3 and Table 5, along with the higher degree of spread of the frequencies in CFAD indicate the greater level of uncertainties that exists in the measurements during SOCRATES. Furthermore, the cited papers in Table 1 (reference column), also present these uncertainties in measurements to some level for each in-situ probe and remote sensing measurements, some of these cited studies in Table 1 are entirely on estimating the level of uncertainties of measurements from the respective instruments. The cited literatures throughout the paper on classifying cloud-type, cloud phase and hydrometeor-type detection over the SO region, including Xi

(2022), Desai (2023), D'Alessandro (2021, 2019), Romatschke & Vivekanandan (2022), Atlas (2021), Schima (2022), Zaremba (2020) also indicate the level of uncertainties existing for measurements over the SO. We do compare the levels of uncertainties with the results in this study with other similar studies like Xi (2022) and the uncertainties show decent agreement with previous studies. Even for a greater precision calculation of constrained height-resolved LWC as in Huang (2009) and Hogan (2005) using dual-frequency radars, the retrieved LWP still showed a mean difference of 70-120 g/m^2 with microwave radiometer (MWR) measured LWP.

3. The authors should explain the LWP calculation a little more clearly. This is an important point in the paper, linking in situ and phase discrimination. I would advise the authors to try to validate their methods cloud boundary detection and LWP : For LWP, they should try to compare the LWP calculation with a sawtooth portion of flight in a cloud. In order to see qualitatively whether the LWP is "globally" of the same order of magnitude. For cloud boundaries, you could once again use thresholds on your contents (LWC and/or IWC) to estimate cloud top and base on sawtooth leg (as in the portion on your Fig. 1 between 26 and 27 UTC for example).

Response: This was an excellent suggestion, and we appreciate the reviewer pointing this out. The LWP calculation is simply based on method used in existing studies like Oh (2018), Mioche (2017) which integrates the LWC across the cloud thickness. Validating the calculated LWP as in eq. 3 in the manuscript with the LWP measured across the sawtooth regions where the sampling height is the flight altitude and LWC is measured along that same height as well (i.e. LWP = LWC*flight_altitude for the sawtooth regions) shows excellent agreement. The correlation value (r) is 0.85 with a strong statistical significance ($p<<0.001$). Cloud Top values are consistent to the maximum height for the reflectivity profiles of the sampled clouds, this method seems very accurate for estimating LWP that also presented in Kang (2024). However, this method does not apply accurately for cloud base estimation, as only one or two data points at the extremities of the sawtooth legs are indicative of the base and top heights, resulting in very few comparable data points and makes the statistical comparison insignificant. We have tried to compare and validate our cloud base and LWP calculations with existing geographically and spatio-temporally collocated measurements during the MARCUS campaign which provided radiometer measured LWP values and, ceilometer and micropulse lidar estimated cloud base over the SO. We also compare the LWP and cloud base height values by creating a lat-lon grid box with overlapping measurements for low level clouds between the two campaigns SOCRATES and MARCUS for the similar climatological months (Jan, Feb and March), and found excellent agreement in the resulting values. The mean cloud base height difference was found to be around 159 meters where SOCRATES mean cloud base height (derived using the method described in this study) was 1055 meters and for MARCUS was ~900 meters. This is expected as MARCUS being a ship campaign sampled more lower clouds (closer to the surface) than SOCRATES, and the mean difference is within a decent agreeable range for cloud base heights. LWP measurements also show an excellent agreement with mean values: 110 g/m^2 (SOCRATES, as calculated in this study) and 127 g/m^2 (MARCUS, radiometer-measured), and a difference of ~17 g/m^2.

The comparisons and validations of cloud base heights and LWP measurements are presented in the Supplementary as will be attached with the revised manuscript.

4. The overall weather conditions for the campaign were not presented.

- ECMWF or ERA weather

I think it would be interesting to add a paragraph highlighting thermodynamic conditions.

In particular, to link with cloud structures and the strong presence of low-level clouds.

- Temperature to map cloud properties

Response: Currently this is beyond the scope of this study however we have added some lines describing the thermodynamic and weather conditions in the revised manuscript. Previous studies on the SOCRATES campaigns cited throughout the manuscript have already covered extensively on the thermodynamic conditions prevailing over the SO region during the austral summer months. The SOCRATES campaign specifically targeted low-level clouds with greater liquid concentrations hence our results that estimated a >90% occurrence of low-level clouds and liquid-dominant cloud phase was expected. The ERA5 temperature was used for the phase-analysis which was already available as a time-height profile matched to the HCR-HSRL datasets in the SOCRATES EOL data archive, developed by NCAR/EOL HCR Team (2023). SOCRATES campaign was carried out during the austral summer months of Jan and Feb 2018 hence there was not any seasonal differences observed between the datasets as the months of NDJFM present relatively stable and similar cloud cover over the SO (Dong et al., 2024). The linkages of temperature (entire cloud, cloud top and cloud base) with the retrieved cloud phases are presented in Figure 8. Maciel (2024) showed extensively on the cloud phase partition linkages with the thermodynamic temperatures (cited in the revised manuscript). Furthermore, a typically windy weather condition was observed along the cloud base, which is a characteristic of the SO, also indicated by the greater level of turbulence observed in the datasets captured by the profiles of reflectivity, spectrum width and doppler velocity (Figure 7) and the CFADs (Figure 10).

5. Part 4 (Results and discussion) is interesting, with a detailed phase classification algorithm. The sub-sections are coherent, and the addition of the comparison of the different methods is an essential and clear point. However, I find the explanations of the CFADs linked to figure 10. a little complex and difficult to follow. The links between Doppler velocity, or Spectrum Width, and size distributions are sometimes complicated to make. Moreover, add definition of mixed phase; please provide ref. It is important to clary what is considered as a mixed phase cloud or layer here. (see specific comment).

Response: We have added a definition for the mixed phase cloud as per Korelov (2022) in the revised manuscript (Section 4.1). We appreciate this comment as it helped us identify certain key issues in the CFAD presented, which led us to reanalyze the CFADs and the new CFAD is presented in the revised manuscript. The key changes are that since a direct pixel by pixel mapping of the phase profile was tampering the 2D time-height structure of the reflectivity, spectrum width and doppler velocity profiles. In the new CFAD we carefully remove all the surface values and only retain the profiles from the estimated cloud base height to the cloud top height eliminating near-surface contamination or noise. Furthermore, the time averaging is increased to 1 minutes from the previous 10 seconds to further smoothen the data to handle outliers and remove significant noise from the volatility of the remote sensed measurements. This helped to significantly smoothen and polish the dataset prior to plotting the CFADs. The revised CFAD highlight the dependence of dBZ on particle size, while Vd and WID profiles reveal particle motion and turbulence, with findings showing turbulence-induced broadening near cloud bases and transitions in particle sizes and phases from cloud tops to bases. Mixed-phase and ice clouds show distinct patterns, with turbulence and particle growth playing critical roles in their vertical evolution. The new findings suggest a median dBZ of around -26 for liquid phase with a slight shift towards -30 from lower mid to cloud base (normalized height, $H_i<0.2$), dBZ for mixed-phase remains significantly high at around -20 dBZ for mixed phase displaying a greater spread which can be attributed to the wider variability in particle size distributions, ice phase displays a median dBZ of around -27 from cloud top to mid-cloud level but increases to -25 at the cloud base due to incidence of larger particle sizes as ice crystals grow from the cloud top to cloud base by accumulation and aggregation processes. The doppler velocity (V_d) shows similar trend across all the three phases increasing significantly from the mid to cloud base level ($H_i<0.2$) due to significant downwelling motion. The spectrum width (WID) of liquid and mixed phase cases are similar exhibiting sharp increase from $H_i<0.2$ indicative of significant downwelling motion and greater turbulence at the cloud base, for ice phase the median WID is significantly higher at the cloud top (0.7 m/s), decreases at mid cloud levels and again increases to ~0.6 m/s at cloud base which is indicative of a broader spread in the ice particle size distribution at the upper levels and significant turbulence and downwelling motion at the cloud base. The CFADs also confirm the irregular shape or morphology of ice particles along with the higher incidence of larger drizzle and ice particles at the cloud base, but not enough liquid samples at the lower bottom regions. The results from Maciel (2024) have shown the presence of stronger in-cloud turbulence and updraughts in the clouds sampled during SOCRATES, especially for cases when supercooled liquid droplets are surrounded by ice crystals and/or mixed phase cases. This is demonstrated by the nature of the CFADs where a stronger turbulence causes an increase in Vd and WID values near the cloud base (Hi<0.2). Detailed analysis is presented in the revised manuscript.

6. You can make comparison with other study, it looks like a statistical comparison and not a pixel/cloud layer by pixel comparison (which could be more relevant). It could interesting to carry out a pixel by pixel phase comparison. Phase comparison with other in situ probes such as the PHIPS which can provide an independent assessment of the cloud phase. Provide in the appendix the results of the D'Alessandro method applied to your dataset.

Response: We did not identify any existing phase retrieval study that could offer a pixel-by-pixel comparison with this study. Hence an aggregate comparison is the best we could do here as is presented in Table 4a and 4b. PHIPS dataset do not provide a relevant phase-specific dataset that is comparable to our estimated time-height or dominant phase profiles, such comparisons though relevant are not apple-apple comparison. The presented comparison of our results with MLR algorithm in Table 4a is the results of D'Alessandro phase product applied to this dataset, we however cannot independently do what D'Alessandro method did as it was an extensive multinomial logistic regression analysis and beyond the scope of our study. We used the final cloud phase product from D'Alessandro (2021) method which is available at the SOCRATES EOL data archive for the comparisons with our study by matching the time stamps and found decent agreements (Table 4a). The significant difference between the methodologies in this study and D'Alessandro (2021) have already been highlighted in section 4.2.

Specific comments and technical corrections

7. Line 18 : Please and microphysical to "macrophysical properties " as the in situ measurements (CDP and 2DS) are also used to characterize the microphysical structure of the clouds

Response: Corrected as suggested

8. Line 24-25 : Please specify the temperature range when you mention higher temperature and lower temperatures

Response: Corrected as suggested (as it is a range, hence T>-2.5 ºC for liquid and <-2.5 ºC for ice).

Introduction :

9. Line 33 : In the beginning of this introduction, all that's missing is a latitude scale to define the limits of the Southern Ocean globally, not just for the campaign region.

Response: Corrected as suggested

10. Line 47 : The are other papers focusing on Arctic mixed-phase clouds such as Jackson et al., (2012), Järvinen et al., (2023) or Moser et al., (2023) that describe the structure of these clouds from in situ data.

Response: We have cited and referenced these studies appropriately in the revised manuscript.

11. Line 50-52 : Cloud you be more specific ? What do you mean by "most algorithms are tuned for specific …. climatic regions" ? Does it concern satellite retrieval algorithm where the cloud phase identification is performed priori to cloud property retrievals ?

Response: Refer to Shupe (2007) for clarification on this part. It concerns both satellite and ground-based retrievals of cloud properties where phase is estimated apriori as a necessary pre-requisite for cloud properties evaluation. We have slightly modified the writing in the revised manuscript to make it clearer.

12. Line 57 : For SO another recent paper Bazantay et al., (2024) and for Arctic region there is also Mioche et al., (2015) or Matus and L'Ecuyer, (2017).

Response: Cited and referenced these studies appropriately in the revised manuscript.

13. Line 79 : In 1-2 sentences, the authors could propose an example of cloud-type classification based on satellite instruments. In order to get an overall idea of the different methods (in situ, spatial,…). Additionally, the authors should also state that the mixed phase identification depend on the observation scale. In the literature, different approach are used and for instance a mixed phase cloud can be composed of a combination of liquid phase pixels and ice phase clouds with no mixed phase pixels. It also depend of the instrument used to detect the cloud phase layers.

Response: We have tried our best to add couple lines addressing this in the revised manuscript, studies like Korelov (2022), D'Alessandro (2021) have been cited which cover the phase- and platform-specific information extensively especially for mixed phase partitioning in SO and Arctic clouds and for comparisons between ground, airborne and satellite-based measurements Dong (2024) can be referred. These information has been added suitably in Section 1 (Introduction) and Section 4.2 where they best fit.

14. Line 82 : This sentence is really long.

Response: Corrected as suggested.

15. Line 88 : Perhaps add instrument names for remote sensing as in line 92 for in situ probes.

Response: Corrected as suggested.

16. Line 104 : Authors can also add "and near-surface contamination problems related to echo".

Response: Corrected as suggested.

17. Line 107 : However, lidar is still useful for determining cloud tops if they are made up of liquid phase (strong backscattering).

Response: Our analysis showed that while HSRL lidar backscatter signal was efficient for estimating cloud base but only when the flight flew below the cloud base offering a zenith-viewing direction for the lidar, which is also presented in Kang (2024), but not for estimating cloud tops at those same zenith-viewing direction as the lidar signal attenuates for thicker cloud layers. However, the HCR radar reflectivity and spectrum width profiles were found to be comprehensive enough for estimating both cloud top and cloud base height at both the zenith and nadir-pointing directions. Similarly cloud base estimation using lidar at the nadir pointed view was found to be erroneous and we estimated an offset of ~400 meters between the cloud base estimated by the two instruments (HCR and HSRL).

Data and methods :

18. Line 131 : The authors should also state that the mixed phase identification depend on the observation scale. In the literature, different approach are used and for instance a mixed phase cloud can be composed of a combination of liquid phase pixels and ice phase clouds with no mixed phase pixels. It also depend of the instrument used to detect the cloud phase layers.

Response: See response to point 13. Additionally, a small definition of mixed phase cases along with the type of mixed-phase partitioning observed during SOCRATES has been added in Section 4.2 as per Korelov (2022); Maciel et al., 2024; and D'Alessandro (2021), respectively. The subjectivity of phase retrieval to observational scales and nature of sampling is duly acknowledged as suggested (Section 1) however for this study the observational scales and nature of sampling are very specific to the SOCRATES campaign.

19. Line 137 : I find this sentence not very clear, the authors could add a "small" additional explanation to explain the liquid water/ice discrimination.

Response: Improved the writing as suggested. That's a screening criteria as per the 2DS instrumentation details for SOCRATES, that only particle sizes D>200 µm are considered to be ice. Explained in Table 1 as well.

20. Line 140 : The uncertainties arising from the in situ instruments are reflected in the PSDs, and I know that it's complicated to estimate uncertainties in secondary parameters (such as content (IWC or LWC)). Perhaps add a sentence to effect that these primary uncertainties are reflected in the secondary parameters.

Response: Corrected as suggested.

21. Line 141 : Do you use a weighted average to calculate your merged size distribution ?

Response: They are a continuous dataset at 1 second time interval where CDP contains PSDs for particles between 2-50 µm, and 2DS provides PSD for particles from 40 µm onwards (2DS does provide PSD from 10 µm but the dataset suggests using diameter > 40 um as sizes below that cannot be resolved well). We use the CDP and 2DS dataset as it is, without any modifications, merging them as a continuous size distribution following Zheng (2024). Refer Figure 2 and Zheng (2024), the droplet number concentrations in the overlapping size bin between CDP and 2DS are redistributed, assuming a gamma distribution, and thereby a complete size spectrum of cloud and drizzle can be merged from CDP and 2DS measurements.

22. Line 142 : How did you choose the threshold (40 µm) between cloud droplets and drizzle particles ?

Response: The demarcation was selected based on Wood (2005) and Zheng (2024). The CDP can only resolve for smaller cloud droplets (2-50 µm) while 2DS can sample drizzle particles as well. 2DS dataset mentions that size distributions cannot be resolved for particles below 40 µm. (Refer: Wu and McFarquhar., 2019). Also see: https://data.eol.ucar.edu/file/download/5406994F0D471/Readme_2DS_V1.1.txt

23. Line 152 : Out of curiosity, did you check whether the ERA5 data matched the temperature data measured by the aircraft ?

Response: It is difficult to directly compare them but for segments where the ERA5 height (radar height) is approximately at the flight altitude level, ERA5 and aircraft temperatures had excellent agreements. But we did not scrutinize it in much detail as this is an already published dataset for the SOCRATES campaign. Refer: NCAR/EOL HCR Team, NCAR/EOL GV-HSRL Team. 2023. SOCRATES: NCAR HCR radar and GV-HSRL lidar moments data. Version 3.2. UCAR/NCAR - Earth Observing Laboratory. https://doi.org/10.5065/D64J0CZS. Accessed 11 Nov 2024.

24. Line 197 : The time scale is a bit strange (29 hours UTC ?), maybe change the legend to "Since midnight (15 Jan 2018)".

Response: The time axis has been corrected in the revised manuscript and converted to have the corrected UTC decimal time.

25. Line 230 : I'm not sure I understand the LWP calculation. In the formula, j corresponds to ? In agreement with *h et al. (2018)*, j represents the number of points in your profile ? To have several values of LWC in the profile, means that the plane passes several times in the same column ? Or is your in situ LWC just summed over the thickness of your cloud ? This calculation and method should be described a little more, as it is essential for the study. What does n stand for ? Authors can attempt to "validate" the LWP, using the method in Mioche et al., (2017), on a sawtooth leg.

Response: The LWP calculation is actually done as per Oh (2018), and Mioche (2017) (also see Toledo., 2021). The j is each time interval (1 sec), which is the time frequency for the sampling. The flight sampling for measuring PSD and LWC is along the straight track, it does not pass several times in the same column. n is the last extreme sampling point (upper limit), i.e. where the flight sampling leg ends. i, j and n are all indices or points along the time dimension. In-situ LWC is not summed across the cloud thickness but is directly measured along the flying altitude or the sampling height. SOCRATES do not provide time-height profiles for LWC or LWP as has been mentioned in the manuscript. (note: LWC is also directly available from 2DS and CDP measurements and is validated to be same as the calculated merged LWC, for respective size bins). LWP validation has been done (refer to response to comment 3) and details has been added to the Supplementary.

26. Line 250 : How is the IWC calculated ? Mass/diameter law?

Response: We do not calculate IWC ourselves and use it directly from the 2DS dataset, where IWC is estimated using the mass-diameter relationship. (Refer: Wu, W., McFarquhar, G. 2019. NSF/NCAR GV HIAPER 2D-S Particle Size Distribution (PSD) Product Data. Version 1.1. UCAR/NCAR - Earth Observing Laboratory. https://doi.org/10.26023/8HMG-WQP3-XA0X).

27. Line 253 : How good is the ERA5 data in the Southern Ocean ?

Response: We only use the ERA5 temperature in this study and we found it to be in-sync with the in-situ aircraft measured temperature, at the similar height levels. However, a direct comparison is not possible. The existing literature on SOCRATES can be referred here for further details, like Romatschke (2021); Romatschke and Dixon (2022); Romatschke and Vivekanandan (2022). Further details on the dataset can be found in NCAR/EOL HCR Team, NCAR/EOL GV-HSRL Team (2023).

28. Line 267 : The authors could add the grids on the figures to make it easier to see the values (even if the essential values are quoted in the text). I think a error bar should be added to figs 3.a, 4.a and b. Ilt's always interesting to estimate the statistical error.

Response: Gridlines have been added to Fig 3, 4 in the revised manuscript. The error statistics is mentioned in Table 3. We will add the error bars in Fig 4a in the revised manuscript, it is around 10 g/m^2 (standard error) as described in the manuscript already. The estimated LWP was also compared with reported LWP measurements over the SO for low-clouds, and we found a decent agreement in the values and the uncertainty (std. dev) with results from Xi (2022), Mace (2023), Tan (2023). See Xi (2022) - Figures 3 and 4 which is a similar version of Fig. 3 and 4 of this study. Figures 3a and 4b are occurrence frequency percentages and the occurrence frequencies add up to 100% for all cloud types.

29. Line 271 : I would have preferred the definition of the LWP threshold at 10 g/m^2 to have been explained at the same time. This is explained in the next section (4.1).

Response: Since the LWP thresholding is only carried prior to phase estimation, it was deemed more appropriate to be explained in that section (4.1). We will add it briefly in the referenced section as well as mentioned.

30. Line 285 : Have you analyzed median values versus mean values ? Just to see if the observations follow a Gaussian curve.

Response: We have for this part, however not a perfect Gaussian curve, the mean and median values are very similar to each other for the cloud boundaries and LWP. However, the CFADs (Figure 10) were found to be slightly right skewed.

Results and discussions :

31. Line 291 : 10 seconds at ± 100 m/s (probably more) ≈ 1 km. It is important to note that the main underlying hypothesis is that the cloud is globally homogeneous over ± 1 km.

Response: Correctly identified, and we have added this to the revised manuscript. The temporal averaging is an important constraint in this study. The homogenous nature of SO clouds has already been highlighted in the Introduction section as well.

32. Line 340 : Duplicate sentence
Response: Corrected.

33. Line 352 : In the fig 6.a, the pixels representing the point counts are wider than in fig 6.d. What is the reason for this difference in definition ? Is it just that there are fewer points for these conditions (“liquid cloud droplets, drizzle and rain drops”) ?

Response: Correct**,** number of points for T> 0 °C is very limited hence a lower number of data bins (50 bins only) was selected for constructing Fig 6a for enhancing visual clarity, resulting in wider pixels.

34. Line 390 : I find this paragraph difficult to understand, but it’s important for understanding dimensional segmentation. The authors could perhaps be reworked.

Response: Reworded this section as suggested.

35. Line 432 : It would have been interesting to have 2DS images to represent the morphological environment. But we can’t show everything.

Response: We did try to analyze some of the 2DS particle habit imager images with our retrieved phase results. We found some decent agreement, but an apple-to-apple comparison was not seen possible due to the nature of the habit imager dataset (and our temporal averaging). The 2DS habit image data can be found here (UCAR/NCAR - Earth Observing Laboratory. 2018. NSF/NCAR GV HIAPER Raw 2D-S Imagery. Version 1.1. UCAR/NCAR - Earth Observing Laboratory. https://doi.org/10.26023/9555-DKY0-J604.) and requires the XPMS2D software to be viewed.

36. Line 433 : The authors could add a reference to show the consistency of these remarks with other studies, also in agreement with mixed-phase clouds in the Arctic region. I also think that the PHIPS and SID-3 instruments were deployed during the SOCRATES campaign. Did you check the consistency of your cloud phase identification with asymmetry parameter derived from the PHIPS which could be a good proxy for cloud phase. SID-3 can also provide information on small ice crystals. Was this investigated?

Response: See response to comment 6. Since we were specific to low-level SO clouds during SOCRATES however Korelov (2022) which is one prominent study on arctic mixed phase clouds which was thoroughly cited throughout the paper, we did not carry out any further comparisons with Arctic region clouds. As, mentioned, PHIPS habit imager dataset does not provide a relevant phase-specific dataset that is comparable to our estimated time-height or dominant phase profiles, such comparisons though relevant are not apple-apple. We could not find any SID-3 dataset deployed during SOCRATES in the EOL archive (this needs to be requested from the relevant research team as per Järvinen, 2018). So, this was not investigated, but a future study can be designed to validate these findings. One must note that our proposed phase estimation method is coarser and majorly dependent on radar signals, hence comparisons with particle habits is not straightforward.

37. Line 435 : So yes, the 2DS is better to identifying images above 50 µm, but the term "easily" is a little misleading. Particle with size of 50 µm is made up of ± 5 pixels with 2DS, so it's still difficult to characterize the phase and even more complicated if you're trying to analyse morphology.

Response: Yes, this is true. We have reworded the sentence. Also noticed a typo in this section as ice particles only above 200 um is resolvable clearly.

38. Line 454/503 : Authors could try to explain the difference between the comparison of the 2 methods (this study and MLR). Of course, these differences can be partly explained by the use of very different methods.

Response: A brief description of the MLR algorithm has already been presented in the manuscript, which is a readily available cloud phase product dataset in the SOCRATES EOL data archive, that is also indicative of how the methods are different in nature. As for Shupe (2005) and Initrieri (2002) we used those thresholds and constraints on our dataset to estimate phase according to their methods to compare with our results. Further, see response to comment 6.

39. Line 480 : What could account for the difference in PLDR thresholds between Arctic and Southern Ocean ?

Response: Majorly the nature of the clouds as described in the Introduction section, which also accounts for the different microphysical nature, spatial heterogeneity, and the nature of ice-liquid phase partitioning in SO clouds compared to MBL clouds over the Arctic region. But as observed the PLDR thresholds we propose based on our findings in Section 4.2 are very close if not at all same to those defined over the Arctic. Further scrutiny is necessary.

40. Line 535 : What's not necessarily explained is that these thermodynamic parameters are dependent on the season in which the measurement campaign took place (January/February). Would partitioning phase or phase distribution be the same for identical environmental conditions in summer?

Response: We have added a sentence highlighting the exclusivity of these thermodynamic parameters to Jan/Feb (during SOCRATES). The results should be similar for the entire climatological winter months NDJFM (Dong, 2024) but further scrutiny is needed to evaluate how they change for the summer months.

41. Line 571 : I would have liked to see a reference for the influence of morphology on reflectivity unless this is a hypothesis you're proposing ?

Response: The CFAD analysis has been revised significantly, and the new analytical results and explanations need to be considered here. One key takeaway is that mixed phase which constitutes of intermixed small to large sized liquid and ice droplets can significantly cause higher and a greater spread in reflectivity values. Xi (2022) is a relevant reference to validate this finding.

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