The comments from the authors are written in bold font. All page numbers and line numbers refer to the original draft.

Reply to comments by reviewer #2

General Comments

This paper presents a detailed comparison of polar mesospheric cloud (PMC) albedo and ice water content between two different satellite instruments. The instruments are the Optical Spectrograph and InfraRed Imager System (OSIRIS) on the Odin satellite and the Cloud Imaging and Particle Size (CIPS) instrument on the Aeronomy of Ice in the Mesosphere (AIM) satellite. Because OSIRIS typically views PMCs on the limb whereas CIPS typically views PMCs in the nadir, the authors have carefully considered coincidence criteria, scattering conditions, observation geometry, and instrument sensitivity in the uniquely coordinated study between the two instruments. As part of the study, the authors present the first thorough error characterization of OSIRIS tomographic cloud brightness and ice water content.

This is an important paper and establishes a valuable precedent for subsequent comparisons between PMC limb viewing instruments and PMC nadir imagers. The results show good agreement, particularly given the diversity of data included in the study. Most importantly, however, the authors provide an exhaustive error analysis that will be a useful reference in future PMC correlative studies.

The Reviewer recommends the paper for publication provided that the authors address the comments below. The "Specific Comments" are relatively minor but important, particularly in providing context of their results with the existing body of work on this topic.

> **Reply: We thank the reviewer for this very kind comment. We also want thank the reviewer for providing much valuable advice and recommendations of how to improve the manuscript. Your specific comments and ideas for the section "Conclusions" is something we are very grateful for.**

Specific Comments

(1). Abstract. Please indicate latitude range and years used in the analysis. Also, if PMC frequency is not compared between OSIRIS and CIPS within the common volume, the authors should explicitly say so in the abstract.

> **Reply: We have added this information to the abstract.**

(2). p. 8. Lines 9-11. Did Benze et al. [2011] use the operational CIPS product to compare directly with SBUV? The Reviewer looked at this paper and it appears that the good agreement with SBUV as stated here arises because a separate CIPS retrieval was developed to simulate the SBUV PMC retrieval. This is not a validation of the CIPS or SBUV data, which is what is suggested by this statement. How do operational SBUV and v4.20 CIPS PMC albedos, IWC and frequencies compare for the same volume and the same time at these high latitudes (78-80 N)? If the authors do not have a ready answer or if it is beyond the scope of this work then they should be explicit about what was done previously to find agreement between CIPS and SBUV (i.e. a separate CIPS algorithm). They could also delete these sentences entirely without loss of content to the paper.

Reply: Good point. As the reviewer writes, Benze et al (2011) used a separate "SBUV-type" algorithm. To make this clear in the text, we changed the lines 9-11 on p. 8 to:

CIPS cloud detections and albedo values were previously compared to the solar backscatter ultraviolet (SBUV/2) instruments [Benze et al., 2009; 2011]. This was accomplished by applying a ''SBUV-type'' algorithm to the CIPS level 1A data to make the two datasets comparable. Cloud frequency and brightness from CIPS were shown to be in good agreement with SBUV/2 retrievals.

We do not know how SBUV and CIPS v4.20 compare for the same volume and time, and it is beyond the scope of this work to analyze this.

(3). p. 16, end of section. Please include a paragraph here explicitly indicating what is done with pixels where there are no clouds at all. If the authors have set all pixels less than 2e-6 sr-1 to zero, then they need to explicitly say here whether they have averaged the zeros into their calculations of albedo and IWC or not. This distinction has historically been a source of great confusion in the field of PMCs. It may be that at these high latitudes there is always a cloud within the common volume for their limited dataset and if so they need to say that as well. This does not appear to be the case from looking at Figure 2. However, the authors winnow the dataset to 788 total observations (p. 17, line 9) so it is not clear how Figure 2 evolves with the study.

Reply: The zero pixels are included in the average. The reviewer is right that there is often cloud within the CV element in our combined dataset, but not always. As can be seen by figure 2, for several hundred observations CIPS observe no cloud in the CV element, and for many observations CIPS observe only partly cloudy pixels in the CV element. As suggested by the reviewer, we add a section in the end of p. 16 to discuss this. The following text section has

been added:

To summarize, the following two sensitivity adjustments are applied to CIPS pixels: (1) All pixels with an albedo < 2e-6 sr^{-1} are set to zero, and the zeros are included in the horizontal average of CIPS albedo (2) A filling factor of 95% is required, meaning that only observations where CIPS observe at least 95% cloudy pixels in the CV element are included in the comparison.

To make this clear also earlier in the paper, we added the following sentence to section 3.3, p. 13, line 14:

As noted in section 2.2.1, we manually set all dim CIPS pixels < 2e-6 sr^{-1} to zero in the qualitative comparison. These zero pixels have been included in the average of CIPS mean albedo.

(4). Conclusions. This section is lacking a summary of relevant conditions under which the comparisons are made. This includes (but is not limited to) the years studied, the latitudes used and the local times of the comparisons. This should also emphasize that the authors are comparing albedo and IWC and not PMC frequency. This section is also lacking a summary of previous related work by Bailey et al. [2015] using common volume observations of SOFIE and CIPS on the same AIM satellite. Bailey et al. state that CIPS IWC is a factor of two smaller than SOFIE IWC, differences that are generally larger and go in the opposite direction of the present work with OSIRIS. Although the authors have done a thorough analysis of their two datasets (OSIRIS and CIPS), the reader should be made aware of these differences of CIPS IWC with the limb viewing SOFIE IWC. This is all the more important because IWC is the native measurement quantity for SOFIE. Differences in the method of observation, calibration, coincidence criteria, latitudes of the comparison, the years studied, the solar zenith angles and the local times of the comparison may all play a role in reconciling these differences and could be included to raise awareness with the reader. The above could be done with two paragraphs and if the authors prefer they could rename this section “Discussion and Conclusions”.

Reply: The authors agree, the difference in IWC compared to Bailey et al., (2015) needs to be addressed. We are aware of the differences between our CIPS/OSIRIS study and the CIPS/SOFIE study of Bailey et al. In our study we compare two instrument that have similar measurement technique. They both measure scattered light in the ultraviolet from a common volume, and therefore a direct comparison between CIPS and Osiris IWC is instructive. Despite this, the observed difference between the CIPS and SOFIE remain.

We changed the title of the section to "Discussion and Conclusions" and added the following paragraph to p. 23, line 14:

Bailey et al. (2015) compare albedo, radius, and ice water content in a CIPS-SOFIE CV that is located at high SZAs. These clouds occur right at the edge of visibility, where the Rayleigh background is small and has a large slope vs. SZA. This is a difficult region to observe PMCs, as even a small error in the CIPS Rayleigh background is expected to have a large impact on the retrieved cloud products. Bailey et al. (2015) apply a justifiable correction to the CIPS background removal which brings the SOFIE and CIPS observations, specifically albedo and ice water content, into much better agreement than without (their Figure 11). The present study uses cloud observations at a much more favorable SZA range (59-71 deg). Here, CIPS background changes such as found by Bailey et al. (2015) are negligible, and no CIPS background correction is necessary.
Bailey et al. (2015) show that CIPS IWC is ~30% smaller than SOFIE IWC (their Figure 11). In the present study, CIPS observes significantly more IWC than OSIRIS in the common volume. This difference between the two studies is important because ice mass density is the native measurement quantity for SOFIE, which can thus be seen as a reference measurement. SOFIE uses the technique of satellite solar occultation to measures vertical profiles of limb path atmospheric transmission, and as such is much more sensitive to small ice particles than OSIRIS. The two studies use different methods: Bailey et al. (2015) model what CIPS should observe based on SOFIE observations, whereas this study corrects for sensitivity differences and compares the horizontally and vertically averaged quantities. Another difference is that this study uses observations at more favorable SZAs and higher latitudes, therefore avoiding uncertainties of the CIPS background and containing generally brighter clouds with more ice.

An additional reason for why OSIRIS IWC is biased low to CIPS IWC in this work is considered to arise from how ice mass density (IMD) is calculated in OSIRIS PMC retrieval. When calculating IWC from the vertical integration of OSIRIS IMD data, we currently only take into account retrieval pixels that are bright enough so that a spectroscopic size retrieval is feasible. It is possible that OSIRIS miss ice from the pixels where a mean radius > 20 nm are reported. How much ice do we miss in this way by ignoring weak cloud pixels that cover typically an altitude range of 1 km in the upper part of the cloud? This we can estimate by considering how much ice we can produce when converting a typical concentration of water vapour at 86 km (e.g., 3 ppm water vapour in 2e14 cm-3 air) into ice. Calculating this, a 1 km thick layer contributes to the overall IWC with 18 g km-2. This is more or less the difference that we see in Figure 7 between OSIRIS and CIPS IWC.

In the updated manuscript, we will add this explanation of the difference. One could think about quantitative ways of assessing the ice water in the weak upper parts of the cloud even though an actual size retrieval may not be possible in these weak pixels. One possible way is to establish a direct relationship between cloud brightness (scattering coefficient) and ice concentration, as recently suggested by Thomas et al. (Atmos. Meas. Tech., 2019) in terms of an "Albedo Ice Regression". This is beyond the scope of the current paper, though.

To p. 24, in beginning of section Discussion and Conclusions, the following sentence was added:

The analysis is performed for northern hemisphere 2010 and 2011 for a total set or 180 coinciding orbits at latitudes from 78N to 80N for local times ~ 15.45.

To p. 24, line 21, the following sentence was added:

Due to the limitation of a total of 18 days of observations during the seasons NH 2010 and NH 2011, we have not performed a detailed comparison of cloud frequency.

<u>Technical Corrections</u>

p. 1, line 17. “ice” should be “ice water content”. Similarly, on line 20 “ice content” should be “ice water content” to avoid confusion.

Reply: This has been corrected.

p. 2, line 17. “larg” should be “large”

Reply: Corrected.

p. 2, line 30. “. . .reason for the increasing visibility of PMCs at mid-latitudes” should be “. . .reason for the increasing visibility of PMCs at mid-latitudes in the modern era.” To the Reviewer’s knowledge, decadal-scale trends of mesospheric clouds observed from the ground since the late 20th century are weak or non-existent.

Reply: This is certainly true, corrected.

p. 2, line 36. “advantage” should be “advantages”.

Reply: Corrected.

p. 10, Figure 1. This figure has a geographic range that is much larger than the region of interest and could be improved dramatically. On lines 9-10 the authors say that the cloud albedo is variable but the region of interest is drawn over the data so the reader cannot see this. By reducing the latitude and longitude ranges of the image, only the borders of the region of interest can be drawn and the boxed region can remain unfilled so that the reader can see the structure within. If the geographic range is small enough, the red area could also be drawn with borders rather than filled. If the authors prefer, the figure could be drawn with two panels: Panel "a" could be the current figure and panel "b" could be the zoomed in version. Please also include a color bar showing the range of cloud albedo in the figure(s).

Reply: Good point. We did as the reviewer suggested and updated with a two – panel plot and updated the figure text to:

Figure 1: Example orbit showing a CIPS/OSIRIS coincidence on a polar map plot for CIPS orbit 50777 and OSIRIS orbit 17098. The top panel shows the CIPS orbit strip. The white line on top of the CIPS orbit strip indicates the overlapping ~660 CIPS pixels in the CV. Each CV is composed of ~10 CV elements. The bottom panel shows an example of only those 66 pixels contained within one CV element.

p. 11, Figure 2. Please include tick marks on the x and y axes to better guide the reader. Also, please indicate the total number of detections either within the Figure or in the caption. Since this is the first figure quantitative showing CIPS data, it would also be instructive to indicate that this is CIPS data, and include average latitude, local time, year of the data and a CV frequency either within the Figure or in the caption. Thank you.

Reply: The figure and caption has been updated according to request.

p. 13, line 15. Please include here the ranges of Cspectral and Cphase used in the analysis so that the reader can appreciate the impact of these adjustments in the context of the data.

Reply: Good point. On p. 13, line 9 the following section describing the ranges of the of the conversion factors has been added:
The spectral conversion factor C_{phase} depend on the solar scattering angle and increases with increasing particle size. For particles in the range 1-20 nm C_{phase} varies between 1.0-1.5, for 21-50 nm C_{phase} varies between 0.6-2.7 and for particles in the range 51-100 nm C_{phase} varies between 0.4-5.6. Note that the conversion factors given here range over a large range of solar scattering angles, for a single given solar scattering angle the range of C_{phase} is much more limited. The spectral conversion factor $C_{spectral}$ range between 0.8 to 1.0.

p. 14. The offset and uncertainty in line 17 is a bit different than line 26. This is further modified on p. 17 line 15, but is still not quite the same as reported in the abstract and summary. Please check to make sure the numbers self-consistent throughout. Thank you.

Reply: We thank the reviewer for noticing this. In p.14, line 17 and line 26, and p.17 line 15, the offset and uncertainty should off course be the same and is 2.8e-6 sr^{-1} (±2.4e-6 sr^{-1}). This has been corrected in the manuscript. This result refer to the albedo comparison in the first step of the analysis before we adjust for differences in sensitivity and without restriction on CIPS fill factor.

The offset uncertainty in the abstract and summary (3.4e-6 sr^{-1} (± 2.9e-6 sr^{-1})) is the result from the albedo comparison after an adjustment for sensitivity (described on p. line) and threshold on CIPS fill factor (described on p. line) has been applied. These results have previously also been addressed in results on p. line .

p. 15, Figure 4 caption. “error bars is” should be “error bars are” and “error bar denote” should be “error bars denote”.

Reply: Corrected.

p. 16 lines 2-4. Do the authors mean the error bars in Figures 4 and 6? Please indicate the figures explicitly. Also, the Reviewer only sees black (not grey) error bars in these figures. Are they referring to these? Please be explicit. Thank you.

Reply: Yes, we mean 4 and 6, and the sentence has been corrected. The grey color bars are too dark to be called grey, and we update the text to “black” instead.

p. 20, line 30. A wind of 100 m/s near 85 km seems large. Can the authors provide a reference for this or otherwise justify this wind speed? Is it possible that the cloud could be sublimating and reforming elsewhere? If so the authors should state that as a possibility. To this end, the authors should include the time difference between the two observations in the captions of Figures 8, 9 and 10.

Reply: For reference of the 100 m/s wind speed, the authors referred to the master study by Kåre Backer-Owe from Norwegian University of Science and Technology,“ Behavior of the S1 and S2 Components of the Semidiurnal Tide in the MLT” where observations of wind speeds of 100 m/s are observed by the

Super Dual Auroral Radar Network (SuperDARN) for the station Dragwoll at 63N at 82 km altitude. (fig 2.2)
https://ntnuopen.ntnu.no/ntnu-xmlui/handle/11250/2398890
For wind velocities at higher latitudes, the study by Stober et al (2013) is probably a better source to site. In this paper, wind velocities of ~50 m/s are observed by the Middle Atmosphere Alomar Radar System (MAARSY). We update the text section staring on p.20, line 30 to:

At PMC altitude the horizontal wind is mainly modulated by atmospheric tidal waves and gravity waves. Horizontal wind velocities of ~50 m/s have been observed by The Middle Atmosphere Alomar Radar System (MAARSY) in the northern Norway (Stober et al., 2013). In this CV, the time difference between CIPS-OSIRIS observations is ~5 minutes During this time, a wind speed of 50 m/s would transport a cloud 15 km, which corresponds to the half width of OSIRIS LOS

p. 24, lines 25-27. Please explicitly indicate the version of CIPS data used in this study here (in addition to p. 8, line 8).

Reply: This has been added.

Figures 4, 6 and 7. Please indicate explicitly whether null detections are included in the indicated average. If null detections are ignored in these comparisons then they should say that instead.

Reply: In figure 4, 6 and 7, null detections are included in the average. This clarification has been noted by adding a text section describing this in p.16. To make it clear to the reader, the caption of fig 4,6 and 7 has also been updated with this information.