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Interactive comment on "Albedo-Ice regression method for determining ice water content of Polar Mesospheric Clouds using ultraviolet observations from space" *by* Gary E. Thomas et al.

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Interactive comment on "Albedo-Ice regression method for determining ice water content of Polar Mesospheric Clouds using ultraviolet observations from space" by Gary E. Thomas et al. Anonymous Referee #2 Received and published: 14 January 2019 This manuscript describes the retrieval of the ice water content (IWC) of mesospheric clouds based on measuring cloud albedo. This retrieval method has been developed with particular focus on AIM/CIPS since changes in orbit have made the original IWC retrieval based on phase function analysis impossible. However, the method is also applicable to other mesospheric datasets like SBUV. This gives the method potential

importance for inter-comparisons of mesospheric datasets and for the analysis of long-term variability. The approach is straight-forward, and the results are convincing. I recommend the manuscript for publication after some minor revisions. I mainly would like to see a number of clarifications.

New and old text are in "".

One issue I would like the authors to discuss more: Why is the AIR method not even better than described in the manuscript? The AIR method is based on finding a linear relationship between ice water content and cloud albedo. For typical mesospheric clouds, one can argue for such a relationship even on a theoretical basis, as long as scattering coefficients depend on the particle size to a power in the vicinity of 3. The authors show that the AIR method works well on a statistical basis. However, the AIR results presented in the manuscript show much scatter in the relationship between albedo and IWC, and the authors point out in several places that we cannot expect AIR to work for individual IWC retrievals. I would like to see more discussion on why the method is "not better than this". A reference is e.g. Hultgren and Gumbel (J. Geophys. Res., 119, 14129-14143, 2014). That paper shows many examples of a close relationship between cloud scattering coefficient and ice mass density, which works well even for the altitude-dependent quantities, not only for the column-integrated quantities considered in the current paper. Can I dare the authors to make a more quantitative statement: Can we take the AIR results for real and apply the method to individual retrievals, by providing a suitable statement about the error bar of such individual AIR IWC retrievals? How large (in percent) would such an error bar be?

Apart from the question of why one would be interested in only one cloud measurement, we have responded by calculating the overall percent error in single "measurements" (model simulations) of albedo, given the scattering angle. We looked at all albedos>1G and SA=90o. The distribution of AIR errors is given below. The std deviation of the Gaussian fit is 19%. For most applications this is probably too large an error to be useful. The dispersion of particle sizes leads to a distribution that is quasirandom. This means that averaging will improve the mean by the square root of the number n of measurements. We separated the data into sets of 100 and 500 randomly chosen iwc-albedo pairs, and calculated the % errors. The errors in the means of the AIR distributions [sigma/sqrt(n)] were 2.16% and 0.9%; these are equivalent to a standard error of ∼19% for a random data set. These numbers will vary with scattering angle, and with specific albedo values, but this provides an example of how AIR would work with real data, on a time interval when only a few hundred measurements are available, e.g. one month.

We added the following sentence in line 225 (now line 225): “For the conditions in Fig. 1(c), the mean error of AIR for a single model simulation is 19%. The error can be reduced substantially by averaging. For example, for 100 measurements, the AIR error is only 2%. Figure 1 also shows. . ..” We also added the reference Hultgren and Gumbel to line 130. See Supplemental Figure 1:”singlemeasurementerrors.jpeg

Section 2 (Theoretical basis): The major result of this study is that IWC is linearly related to cloud albedo. Therefore, the description in line 156-160 is confusing. In line 156-157, the authors refer to "the results of this study that IWC is linearly related to the column density of ice particles". The fact that IWC is linearly related to the column density of ice particles is somehow trivial (although dependent on the details of the particle population). In line 158-159, it is also stated that "As pointed out by Englert and Stevens (2007), such a relationship exists for certain SA values..." However, the relevant finding by Englert and Stevens is about the relationship between IWC and albedo. I therefore suspect that this paragraph should be about the relationship between IWC and albedo, not between IWC and column density. Please clarify and reformulate. We agree that we should have stated albedo rather than column density. We changed the sentence from “Anticipating the results of this study that IWC is linearly related to the column density of ice particles,” to “Anticipating the results of this study that IWC is linearly related to cloud albedo, ..” Section 2.1 (Model results): Please describe in more detailed the processes included in the model simulations and the resulting variability

in mesospheric clouds. Is gravity wave activity included in the simulations? Does the cloud database include multiple layer clouds or other conditions that may lead to clouds deviating from a straight growth/sedimentation scenario?

In response to reviewer 1, we added more detail on the model calculations (lines171-onward). In response to your specific request, we added the sentence (now line 177):“ The model contains variability due to waves of various sorts, including tides and gravity waves. However, it does not capture all known details of PMC, such as double layers. Since we are dealing with integrated quantities, this should not be an important issue. Furthermore, we don’t place full reliance on the model, which is why we also use two independent data sets.” Section 2.2 (AIR results from CIPS): Figure 8 shows the AIR method applied to the CIPS from the Northern Hemisphere 2011. When it comes to demonstrating that the AIR method works, this choice of season is unfortunate. CIPS data from the years 2010-2013 has been used in the regression analysis to "train" the model. When subsequently investigating the ability of the model to retrieve IWC, a season should be chosen that has not already been used to train the model. I suggest to choose another season. Since we ‘trained’ the AIR method to literally hundreds of thousands of individual cloud data, showing how it works for a single day does not detract from illustrating its usefulness.. Some other details: Line 84: The notation "meteor ’smoke’ nucleation" may be misleading. It is better to write "nucleation on meteoric ’smoke’".

Agreed: We replace this phrase by “The processes treated by the model include nucleation on meteor ‘smoke’ particles,..” Line 274: To avoid confusion, please clarify what is meant by "mean ice particle volume evaluated at rm", i.e. make clear that you refer to an integration over the Gaussian particle size distribution.

Agreed. We replaced the sentence in (now) line 231 with “V denotes the ice particle volume, averaged over the Gaussian distribution with a mean particle radius value rm.” Line 287: Clarify that by "simulated CIPS retrieved IWC" you mean the AIR result. We apologize for this misconception. We added the following sentence (line 243): “We emphasize that this is not an AIR result, but is an attempt to assess how particles that are too small to be visible to UV measurements affect the accuracy of the CIPS IWC results.” Line 394: The authors refer to n = 3-5 as typical exponents for the size-dependence (sigma âĹij rËĘn) of the scattering cross section for typical mesospheric clouds. Can this be motivated better? Otherwise a larger range may be appropriate from n = 2 (geometrical optics limit) to n = 6 (Rayleigh limit). The reference to Hultgren and Gumbel (2014) has also been mentioned above. This reference is interesting even here as it discusses ideas underlying the relationship between cloud brightness and ice (including e.g. rËĘn dependence of the scattering coefficient, dependence on particle size distribution) that are also discussed in the current paper.

See below for our response to why AIR works as well as it does. Figure 1: In order to better understand the behavior of the model data, it would be instructive for the reader to see the data points all the way down to the zero point (small albedo, small IWC). I do not see a reason not to show these points.

For your curiosity, this supplemental figure 2 shows the results down to very small albedo. This information is not relevant to the CIPS data, which has a detection limit of 1G. The behavior of the relationship is different, with a different slope and more dispersion. We believe that showing this behavior detracts from our message. The blue dots show results from effective particle sizes < 20nm. Red dots show results for reff between 20 & 30 nm and the black dots show the contributions from larger sizes. The light blue line is the AIR result, which is curved because of the log-log scale.

See Supplemental Figure 2, “IWCvsfaintalbedo.jpeg’

Figure 2: To avoid confusion, I suggest to mention the units of the contour lines (g km-2) in the figure caption.

Again, we apologize for the misconception. We added the sentence: “Contour lines are labelled as percent errors relative to the accurate model values.”

Figure 11: In the two plots, there is an obvious lower limit to the data points (in terms of a straight line nearly parallel to the red line). I (and possibly other readers) do not understand why there is such a well-defined lower limit. Please add an explanation.

This was a very helpful suggestion, and we looked further into this matter. The ratio of albedo to IWC shows a clearly defined lower limit to this ratio, which is nearly independent of the effective particle size at least for the larger particles.. This behavior is due to the fact that the ratio of IWC/albedo (which is independent of column density) asymptotes to a straight line for the larger values of particle size (30 <reff<50 nm) which are those responsible for PMC. Below is a plot of this ratio versus effective radius, showing this behavior, for SA=90 deg. The constant ratio at large reff is due to the fact that the r-dependence of the cross-section in this range is r3, and cancels the IWC dependence on r3. The same behavior occurs for the other scattering angles, but the asymptotes are different. The near-constancy of the ratios means that there will be distinct lower limits to the regressions of IWC vs Albedo, which slant upwards in the plots due to the linear variation of albedo on column density. This is evident in Figure 1 where the lower boundary of the scatter is quite linear. These points refer to the largest particles in the population. The other reason for the sharpness of the lower boundary of the ratio is that the largest particles are the ones responsible, and these have a very steep fall-off in the size distributions.

See Supplemental Fig. 3, “RatioIWCtoAlb.jpeg

We added the discussion of this issue in Sec. 3, Effects of Mean Particle Size, beginning on line 329, “The AIR approximation is based on the notion that particle size effects can be ignored in retrieving IWC from albedo measurements. That is, they contribute in a sense to the ‘noise’ of the measurement, which can be minimized by averaging. In fact, the particle size (or more accurately, the term ðİŚ§3) is a principal ‘driver’ of < ðİŘijðİŚŁðİŘű > itself, so it is not obvious that particle size effects play a minor role in deriving IWC. The dependence of albedo on column density adequately captures this part of the variability (albedo is strictly linear in column density). The

AIR slope term is $\sim$ rˆ3/sigma_lambda(r,phi) averaged over a distribution of particle sizes, r. The size dependence of the cross-section varies as a power of r, within two limits, the geometric-optics limit, r2, and the small-particle (Rayleigh) limit, r6. In the intermediate and realistic conditions of PMC, the exponent has an intermediate value. Fortunately, there is a “sweet spot’ (or better, a ‘sweet region’ of the r-domain) in which the r-dependence of sigma_lambda is $\sim$rˆ3, so that the slope term is constant (for fixed SA). This behavior occurs for all relevant values of SA, and for the albedo values typical of CIPS. It accounts mainly for the effectiveness of the AIR method. The other aspect favorable to AIR is the steep fall-off of the particle size distribution at the largest sizes, which contributes to the sharpness of the lower boundaries in the spread of points in Fig. 1.”

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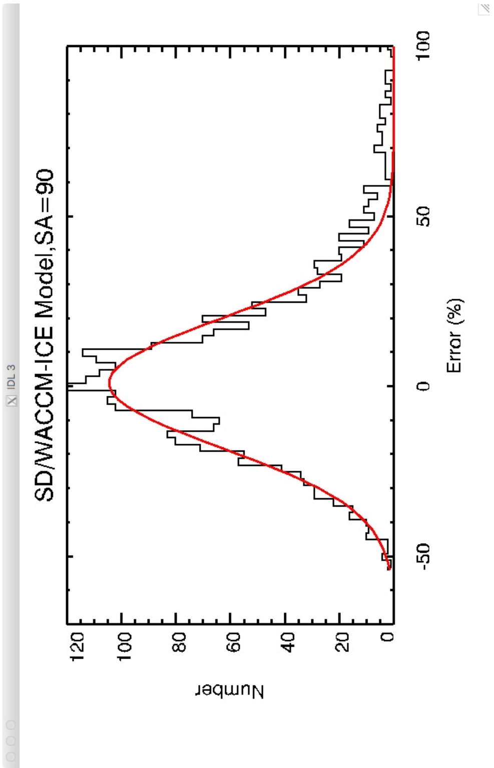

Fig. 1. Supplemetal fig 1

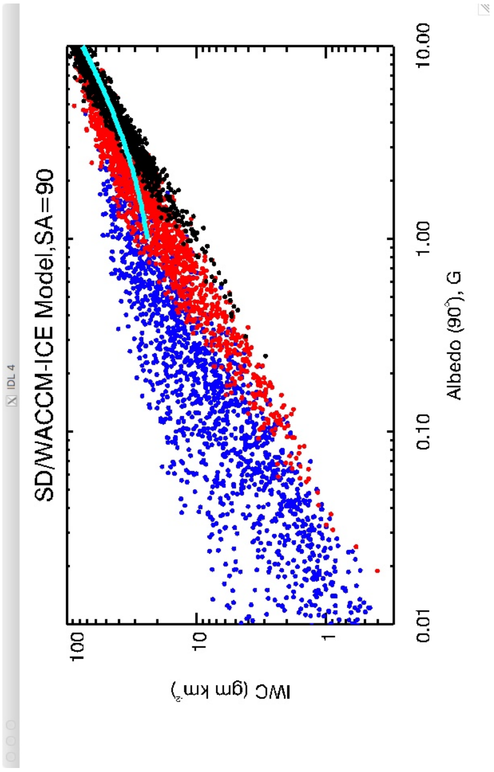

Fig. 2. supplemental fig 2

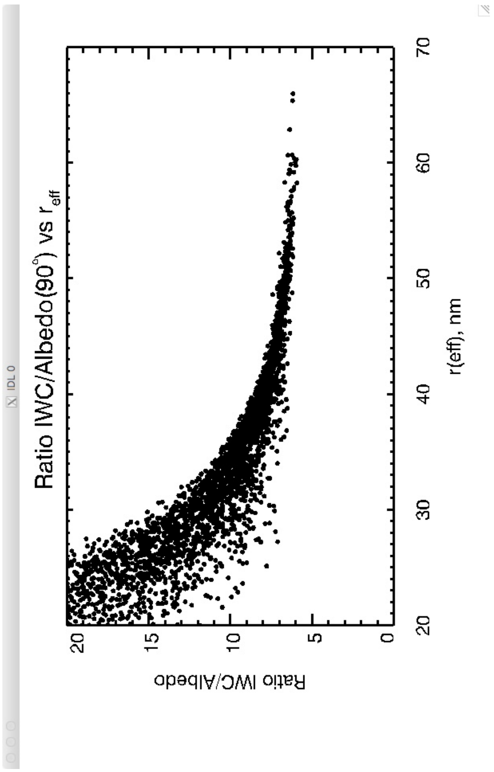

Fig. 3. supplemental fig 3