

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

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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.

C1

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?

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.

Section 2.1 (Model results): Please describe in more detailed the processes included

C2

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?

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.

Some other details:

Line 84: The notation "meteor 'smoke' nucleation" may be misleading. It is better to write "nucleation on meteoric 'smoke'".

Line 274: To avoid confusion, please clarify what is meant by "mean ice particle volume evaluated at r_m ", i.e. make clear that you refer to an integration over the Gaussian particle size distribution.

Line 287: Clarify that by "simulated CIPS retrieved IWC" you mean the AIR result.

Line 394: The authors refer to $n = 3-5$ as typical exponents for the size-dependence ($\sigma \sim r^n$) 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. r^n dependence of the scattering coefficient, dependence on particle size distribution) that are also discussed in the current paper.

C3

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.

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

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.

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C4