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Atmospheric Measurement Techniques Discussions

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 #1 Received and published: 23 December 2018 General comments: This manuscript deals with an alternative approach to retrieve the ice water content (IWC) of PMCs from satellite-borne UV-backscatter observations with the CIPS instrument on the AIM satellite. Due to orbital drifts CIPS is since 2016 operated in a different way and the original approach to retrieve IWC is not possible any more for a large part of the measurements. The novel approach estimates IWC from

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backscatter measurements at a single scattering angle. Overall, the approach works well. The paper is generally well written and is suitable for publication in AMT. I ask the authors to consider the comments listed below. In my opinion the paper should be accepted, once the (mainly minor) issues listed below were addressed.

Specific comments and responses: Our responses are in italics. When we respond "OK", it means that we are following your recommendation. We also include a revised pdf document with our tracked changes, where all additions are in red type. Line numbers are referred to "new" if they refer to the new document, which has been reformatted.

Line 16: "265nm" -> "265 nm"OK

Line 114: "..it is necessary to derive scattered radiances from the same algorithm used by CIPS." I don't really understand this statement "derive scattered radiances from the same algorithm used by CIPS." CIPS measures scattered radiances and the algorithms are used to infer physical properties of PMCs, right? I guess I'm missing a point? No, we were not clear. We replaced the sentences (lines 113-117) , (now 91-94) with the following: "Since the SOFIE technique uses near-IR solar extinction in ice-water absorption bands, the primary measurement is ice water content. As shown in Sec 2.2, we reversed the process, to derive radiances from IWC, and then applied the same regression method to the results." [I suggest an alternative wording for the last sentence (and note the change from section 2.2 to 2.3)]: "As shown in Sec 2.3, we inverted the retrieved SOFIE IWC to derive the equivalent 265-nm albedo, and then applied the regression method described above to the results." Line 138: "the monochromatic scattering cross-section" It actually is the "differential" scattering cross section. Perhaps this can/should be added. OK. Line 140: "number density of ice particles" Perhaps better "column density"?

in new line 107 we changed "number density" to "column density".

Equation (2): I suggest using a slightly different symbol for the albedo than in equation

(1). The left hand sides of equation (1) and (2) are the same, but the right hand sides differ.

Agreed. We placed the letter “m” as a superscript, and identified it as such in the previous sentence (new line 117).

Line 167: add space before “denotes”. OK

Line 209: I don’t fully understand the meaning of this sentence. Is the microphysical model a 1-D model?

No. It is fully 3-D.

“(2) radiance and IWC may be calculated accurately, so that effects of cloud inhomogeneity are absent;”

We added the following sentences after (new) line 172.” With regard to the latter point, we describe in more detail the model calculations. The model grid is 4o in latitude, 5o in longitude, and variable in the vertical. Ice particles of varying sizes fill many of these cells, but the density of particles within each cell is, by definition, constant. For a given model cloud, the integration is made through a vertical ‘stack’ of all ice-filled cells generated in a given computer run, and within each particle size grid. The total radiance is the sum of contributions from the size range 20 to 150 nm. The observation angles are always assumed to be zero, in other 175 words, the integration is performed in the vertical only. Thus cloud ‘boundaries’ in the horizontal plane are not an issue. This contrasts with real heterogeneous clouds where these approximations would not hold.” As you will note in the response to Reviewer 2, we added two more clarifying statements.

Line 216: “thick.)“ -> "thick)“

Same line: I suggest adding "resolution“ to read "The model height grid resolution is variable, being highest. ”, otherwise “being highest” doesn’t make sense.

We changed the sentence to read (now line 186) “The model height grid is variable, so that the smallest layer thickness is 0.26 km, which resolves the narrow ice layers” ... Line 220 and Figure 1: I’m wondering how the linear regressions are actually done. Is it a single linear fit of y vs. x, or do you fit both y vs. x and x vs. y and determine an average slope and offset? Alternatively, there are routines taking both differences in x and y direction into account, when minimizing chi-square. There may be a large difference between fitting y vs. x or x vs. y.

This is an interesting point. However it is largely a mathematical issue, irrelevant to the desired objective. The albedo is the measured quantity, and IWC is the derived quantity - the albedo is the natural independent variable. Certainly, there are circumstances where the opposite is true, when IWC is the measured variable (as in the case of SOFIE) and one desires to know the albedo- (however one would also have to specify the scattering angle in that case). As the reviewer notes, in this case the fitting process would have to be x vs y, but our regression results (y vs. x) apply to the question being asked.

Line 227: “In fact, we found that the linear relationship breaks down for very small albedo,” The reason is probably, that particle populations with really small particles (< 10 nm) have a non-zero IWC, but the albedo is essentially zero, right?

Yes, we would agree with that. In line (new) 199, We replaced this phrase with “below the detection threshold of CIPS and SBUV, and are a result of the very faint small particles.”

Figure 1: Both the IWC and the albedo values have lower limits. Perhaps I missed it, but what is the reason for this lower limit. For IWC the limit seems to be 20 g/km2.

“The lower limit for the albedo (SA=90o) is 1G, which is the detection limit of the CIPS experiment.” We added this comment to the figure caption. . Figure 2: The caption should clarify that the displayed error is a relative error given in Line 216:

We added the word “relative” to the word ‘error’ in figure caption 2. We added further clarification by adding : “The errors are relative to the model values”

Closing parenthesis missing after “Rapp and Thomas (2006)”. OK

Line new 114: “DIFFERENTIAL scattering cross-section”? OK

Line 289: “The mean difference is -13%. Looking at the Figure, the mean difference seems to be larger.

-13% is an unweighted mean over all bins. If we had weighted the error according to the number of points in each bin, the error would be larger, and positive. We added the sentence (now line 243)” Assuming the microphysical model is accurate, the accuracy of the CIPS UV measurements ranges from over +100% for very small albedo to -60% for high albedos. 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.”

Also, what is the reason for the characteristic variation of the difference with increasing albedo? Is there a simple explanation?

We elaborate further in the Conclusion section (lines 353 -356): “Assuming the microphysical model is accurate, the accuracy of UV measurements ranges from over +100% for very small albedo to -60% for high albedos. The overall accuracy of IWC (averaging over all albedo bins) is -13$\pm$17%. The CIPS algorithm overestimates the small-particle population (20-30 nm) as a result of the Gaussian [this change should also be made in the paper] approximation when the mean particle size is small, and the opposite is true when the mean size is large. Distinct from the more fundamental errors due to the invisibility of very small ice particles and the Gaussian approximation, we also estimated the errors in the AIR approximation, relative to the AIM SOFIE data which apply to larger values of IWC than the model. For the dimmer and more frequent clouds, Fig. 2 shows that the error in ensemble averages is of the order of 5%.

Line 289: " .. subset .. have" -> ".. subset .. has" OK

Line 303: "As previously shown, the AIR data applies to the ice mass of 'UV-visible' clouds, not to their total ice mass." I don't really understand this statement. What is the meaning of "UV-visible clouds"? Was this shown in the current paper? If not, please provide a reference.

In line 112 we added the sentence "As shown by Rapp and Thomas (2006), particles with sizes < 20 nm are not detectable by UV measurements because of their small cross-section values – hence we refer to 'UV-visible clouds'."

Caption Figure 4, line 3: "SA = 40 deg" should read "SA = 50 deg". OK.

Line 366: "AIR overestimates IWC by up to 15% AIR may also underestimate IWC, right? I also think that the overestimation may be much larger than 15%.

See the above additions where we address these issues in more detail (line 243): "Assuming the microphysical model is accurate, the accuracy of the CIPS UV measurements ranges from over +100% for very small albedo to -60% for high albedos. 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 367: "SA = 100 deg" -> "SA = 110 deg" OK

Line 375: "The accuracy of the average IWC results was estimated by removing half the data from an entire season and comparing the two results. " I'm not sure, I fully understand this statement. AIR is applied to SBUV data and then you split the data set in half and compare the results. How does this allow you to estimate the accuracy of the "average IWC results"? I guess I'm missing the point here?

We added (line 101) the following sentence to emphasize that the errors refer to systematic differences between several divided data sets; "For a highly-populated region (more than 1000 clouds per season at latitudes higher than 70°), the differences in

IWC ranged between $\pm 3-5$ g-km-2, thus can be considered typical systematic errors. For a less-populated region ($50^\circ-64^\circ$ latitude) where there were many fewer clouds (<50), the differences were larger, $\pm 5-10$ g-km-2."

Next sentence: You report on "changes" of ± 3 - 5 g km2 etc., but were there any systematic differences? These values were the systematic differences between pairs of divided data sets.

We added the word "systematic" to ± 3 - 5 g / km2. We specifically added the phrase "systematic" errors. See above.

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