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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Extending the SBUV PMC Data Record with OMPS NP" *by* Matthew T. DeLand and Gary E. Thomas

Anonymous Referee #1

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Review of "Extending the SBUV PMC Data Record with OMPS NP" by DeLand and Thomas [2018].

This manuscript describes how the new Ozone Mapping and Profiling Suite (OMPS) Nadir Profiler (NP) can observe Polar Mesospheric Clouds (PMC). PMC results from OMPS can furthermore be combined with existing results from a similar suite of observations by the Solar Backscatter Ultraviolet (SBUV) instruments to create and extend a 40-year record of PMC observations. The authors argue that this multi-decadal record can be used for long-term trend studies of the Earth's mesosphere by splitting the record into two segments with a break point imposed in 1998 and analyzing each segment separately.

The OMPS observations are valuable as they complement and could extend the mul-

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Discussion paper

tidecadal PMC observations by the SBUV instruments, which will be discontinued in 2019-2020. The similarity between the instruments furthermore allows for relatively minor modifications to existing PMC retrieval algorithms currently in place for the SBUV data. However, there are many details lacking in how the authors produce their results and the reviewer requests that the authors satisfactorily address the following concerns before the manuscript can be recommended for publication in ACP. These are divided into Specific Comments and Technical Corrections below.

Specific Comments:

1. Section 1, p. 3, last sentence. The authors need to be more explicit about what they are presenting in this manuscript. To this end, Section 1 needs an additional paragraph at the end motivating what is to come, instead of the final sentence. This paragraph should indicate that in Section 2, the authors compare PMC frequencies and ice water content (IWC) from the two nadir-viewing datasets (SBUV and OMPS). In Section 3, the trend study is done only for IWC (if that is the case) and is split into two different periods, with a break point at 1998. Also, if frequency is not included (i.e. inclusion of IWC values that are zero in the averages shown) then they also need to explain why either here and/or at the beginning of Section 3.

2. Section 2. The native OMPS data are not shown. The reviewer requests an additional figure between Figures 2 and 3 showing a sample day of the total observed OMPS albedo, indicating which data points are PMC and which are not. A figure analogous to DeLand et al. [2003] Figure 2 for the NOAA-9 SBUV/2 data would be appropriate here. In supporting text, a discussion of the detection threshold and systematic uncertainties resulting from the separation of the PMC signal from the bright background signal (perhaps referencing Figure 2 of the present paper) would provide valuable context for future comparisons with more sensitive limb sounders or nadir imagers.

3. Section 2. The comparison between the two very similar nadir viewing instruments

(SBUV and OMPS) does not “validate” (line 114) the results since the instruments, the observational approach, and the retrieval algorithms are quite similar. There are now many observational studies that compare nadir viewing PMC observations, including Bailey et al. [2015], Benze et al. [2018] and Broman et al. [2018]. There are also modeling studies that show variations of cloud frequency and IWC as a function of instrument sensitivity over the diurnal cycle and at a variety of latitudes [e.g. Bardeen et al., 2010; Stevens et al., 2017; Schmidt et al., 2018]. Curiously, none of these studies are discussed or even cited by the authors. Even if the above studies do not represent identical conditions of the SBUV and OMPS shown, the authors could compare their average IWC against average results for similar conditions. This discussion should necessarily include the particle sizes to which OMPS and SBUV are sensitive.

4. P. 4, Lines 95-104. Is the scaling factor applied to the operational SBUV product? If so how would the user go about reproducing the results in the manuscript given that there are additional tests performed to identify PMC (please provide a reference for these tests on line 104). Also, is the SZA dependence due to ice particle scattering on solar scattering angle? If so, the authors should say this and if the solar scattering angle is controlling the variation shown in Figure 2 then that quantity should be on the x-axis rather than SZA. In addition, if this dependence is determined from a single phase function the authors need to state this as well as well as any other assumptions that go into Figure 2.

5. P. 5, Figure 2. Since the quantity relevant to the results reported in this paper is IWC, it would be most instructive to the reader to show that threshold in this figure rather than an albedo threshold. If the IWC threshold is dependent on both the albedo and the solar scattering angle [e.g. DeLand and Thomas, 2015], then this can be done by using a different color and the same line types (solid and broken), referencing new labels on the right-hand axis drawn using the same color.

6. PP. 6-8, Figures 3-5. It is not indicated until the conclusion that the OMPS NP instruments are in sun-synchronous orbits and that information should be indicated in

supporting text for these figures or before. Similarly, the orbital inclination of OMPS NP should be in supporting text for these figures, particularly as it compares to the SBUV suite of instruments. This would help to clarify the latitudinal coverage of each. In Figures 3-5, what are the coincidence criteria in space and time used for the data shown? Furthermore, please indicate explicitly (in the panels and/or the captions) what local times are averaged, whether both nodes SBUV and OMPS are used, the SZA (and/or solar scattering angles) and what days are used to define the season.

7. Section 3. It is curious why the authors compare cloud frequencies and IWC in Section 2 (Figures 3-5) but for the trend results in Section 3 (Figure 6) only IWC is shown. If frequency is not included in the IWC trend results of Figure 6 (i.e. the inclusion of observations for which IWC=0), there needs to be a statement in the text to this end as well as an explanation of this decision. If frequency trend results using the OMPS and/or SBUV data appear elsewhere, then the authors need to cite these studies.

8. P. 8, line 153. Have the authors explored how their IWC trend estimates vary depending on their season duration? Please comment in the text. Similarly, how different are the trends if no normalization adjustment is made (lines 156-166)? Please comment in the text.

9. P. 10, Figure 6, p. 11 top and throughout. Have the authors made any attempt to restrict their IWC trend analysis in local time, as was done by Hervig et al. [2016] and Hervig and Stevens [2014]? If so, how different are there retrieved trends when they do this? If not, they need to state this in the text to help distinguish their results from previous trend studies. Similarly, have the authors made any attempt to reproduce the longitudinally dependent SBUV trends reported by Fiedler et al. [2017]? If not, they need to state this in the text as well.

Technical Corrections:

1. P. 3, Line 73. The local times relevant to this study are those at PMC latitudes in the

NH and SH rather than the Equator-crossing time. Please reword.

2. Figure 2. Additional information is required indicating the data used. This information could be in the figure itself and/or the caption and would include (but not limited to) satellites, seasons, and days used as well as local times (see also #4 and #5 above).

3. P. 5, line 114. Given #3 above, the word “compare” is more accurate than “validate”.

4. P. 5, lines 115-116. This is more accurately stated as “7 NH seasons and 6 SH seasons between 2012-2018” (see also #1 above). If they are using the approach of DeLand and Thomas [2015] then they need to state explicitly in the text that IWC is derived assuming a linear relationship with PMC albedo and with fit coefficients derived from general circulation model (GCM) results.

5. P. 9, line 160. If “each instrument” means “each SBUV and OMPS instrument” the authors should say so.

6. P. 9, line 170. There needs to be a more complete explanation about the cause of this change in the late 1990s as context for the reader.

7. P. 11, lines 192-193. Does the statement “those derived in 2015” refer to DeLand and Thomas [2015]? If so, based on Table 4b of that paper a more useful statement for the reader is something like “. . .although the trends for segment 2 (1998-2018) are smaller than those derived by DeLand and Thomas (2015) over a shorter time period (1998-2013).”

8. P. 11, lines 192, 194, 199, and 204. By “significant” do the authors mean “statistically significant”? If so, the authors should explicitly say this. If not, they need to say what they mean in the text.

9. P. 11, lines 207-208. Please provide the typical number of observations so that the reader has more context for the “10-20 clouds” observed.

10. P. 12, lines 217-218. Is there an explanation for the phase lag in the NH? If not

then a more complete statement is “Both the source of the heimispheric difference in solar activity response and the source of the derived phase lag in the NH are not understood.”

11. P. 12, line 228. “above” should be “poleward of”.

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