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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Noctilucent clouds and the mesospheric water vapour: the past decade" *by* U. von Zahn et al.

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The goals of this paper are to quantify the response of mesospheric cloud (MC) brightness to increasing water vapor in the Arctic summer mesosphere, to assess decadal scale water vapor observations in the mesosphere and to explore whether the reported water vapor variation is consistent with the reported MC brightness variation. To reach these goals, the authors present a unique Arctic water vapor data set where observations from 1996-2001 are appropriately confined to the MC season, they include a relatively recent analysis of MC brightness obtained from satellite observations and they employ a 3-dimensional GCM optimized for the study of MC formation. The reviewer finds the science goals relevant and compelling in an evolving and contentious field of study. In principle, the methodology is fundamentally sound, primarily because some of the best available tools are used. However, this paper suffers from the omission of

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important quantitative details, which play a crucial role in support of the conclusions. The reviewer therefore finds that this paper requires major revisions prior to publication in ACP.

The three cornerstones of this work include the cloud formation model, the water vapor observations in the Arctic mesosphere and the MC observations. The reviewer has specific comments and suggestions for revising the analysis and interpretation of each one of these and will address them in order. Other comments and technical corrections are included at the end.

The Cloud Formation Model

1) Results for the analysis of how the cloud brightness changes with increasing water vapor are shown in Figure 1 for 532 nm and a scattering angle of 180°. The authors do not show how the average particle radius at peak β is changing with increasing water vapor, which is an important quantity that helps the reader reproduce their results and allows for the translation or their results to other optical conditions. The reviewer asks that the authors include the average ice particle radius for each of their four cases either on the figure, in a table or in the text.

2) With all due respect to the authors, the argument relating the trend in the 532 nm β value with increasing water vapor (ϵ) to that from the Shuttle Backscatter Ultraviolet (SBUV) instruments is unconvincing. The authors state (p. 3051) that the precise value is not as important as the sign of the trend, but for the relationship of water vapor to brightness β_{max} = const f(H_2O)$^\epsilon$, they quote the exponent ϵ to two significant figures and quantitatively compare the SBUV data to a total of five different calculated trends in Figure 4. The SBUV observations are at 252 nm with scattering angles of 100-140° in general. The wavelength of the observation and the average particle radius determines the size parameter x = 2πr/λ [van de Hulst, 1981], which is significantly different at 252 nm than at 532 nm for typical MC particle sizes of 50 nm. The reviewer asks that Figure 1 be revised to show vertically integrated β values at 252 nm (in sr^{-1}) for a

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scattering angle of 100-140°, since these are the conditions of the SBUV observations being modeled, and to recalculate their ϵ for these conditions.

3) Why are the MC modeled after 5 days of particle growth and not some other time period (p. 3050)? How do the results in Figure 1 change if the MC are modeled for fewer days or more days?

The Water Vapor Observations in the Arctic Mesosphere

These 80 km observations are the new data brought to the paper and are extremely relevant to the conclusions. As for any new data set, they require comparison with previously published observations and with model predictions:

1) The 80 km observation is a very high altitude result for the ground based microwave instrument and must surely be a challenge. The absolute mixing ratios reported at 80 km (4 ppmv) are far less than recently published work of Hervig et al. [2003] using solar-cycle averaged satellite observations of the Arctic mesosphere (7 ppmv) by the Halogen Occultation Experiment on NASA‘s UARS satellite. This dramatic difference with previous work is not mentioned by the authors, who dismiss the work as irrelevant due to the 11-year average employed (p. 3053). However, if the 80 km Arctic summer water vapor is as episodically stable as the authors say, it seems that these averaged satellite observations are indeed relevant. The Hervig et al. results are at higher vertical resolution than the microwave observations, and show a large peak at 82 km. Moreover, data and models show that the water vapor falls off above this peak. Therefore, the altitude registration of the microwave data and the width of the weighting functions are important. The reviewer asks that the authors show their weighting functions and convolve them with the published profile of Hervig et al. for direct comparison. The reviewer also requests the altitude uncertainty and a retrieved profile from the microwave results. How much water vapor can be tolerated at 80 km from the microwave observations?

2) Given that the annual mean at 77.5 km shows a strong decrease from 1995-2001

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[Hartogh et al., 2001], the reviewer is curious about selection of months for inclusion in a trend analysis. The increase is derived from daily averaged values at 80 km of June, July and August but the lifetime of the water vapor is about a month at 80 km [Koerner and Sonnemann, 2001]. It is not clear to the reviewer that the water vapor observed prior to June is not contributing to MC formation. How does the 'summer' trend change upon consideration of weeks prior to June? What if some of the weeks at the end of August are removed? The reviewer asks that the authors show 80 km observations throughout the year in Figure 2 (and delete Section 6.2 which reserves this for future work) and justify their selection of June 1 to August 31 as the days over which to infer a trend.

3) Why is 2002 and 2003 water vapor data not shown? The reviewer requests a brief explanation in the text.

The Satellite Observations of MC

1) In Section 5 and Figure 3 the authors fit a trend to the data from 1996-2000. Line (b) is the best fit to the data in Figure 4 and is derived from a summer water vapor trend from 1997-2000. The reviewer therefore requests that the data be fit instead between the relevant years 1997-2000. This fit should be shown both on Figures 3 and 4 with the recalculated uncertainty clearly indicated, perhaps as a shaded area underneath the data points and the model fits. The uncertainty in the fitted trend is so large that it needs to be shown clearly in the context of the calculated trends. To improve clarity, the reviewer requests that Figure 4 be revised to show only the time period 1994 to the present.

2) The reviewer points out that the SBUV albedo is calculated in units of sr^{-1} but historically has been mislabled as a unitless quantity in PMC observations. The reference for a discussion of this is Stevens et al. [2004]. The units in Figures 3 and 4 and all reported albedo values in the text should therefore be changed accordingly. Since the quantity β is reported in units m^{-1} and is used to express the cloud brightness at 532

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nm, the reviewer suggests that β not be used in describing the reported MC albedo to avoid further confusion and suggests instead ‘SBUV albedo‘.

3) In Section 5, model trends are fit to the data and the best fit is argued to be line (b), which is derived from the July-August means of 80 km water vapor near 67° N from 1997-2000. If water vapor trends show radically different behavior between 20° and 67° N (Table 2), is it unreasonable to expect different trends between 50-82° N? The reviewer requests a statement specifically stating that the water vapor trend inferred at 67° N is assumed to apply to all of the MC data from 50-82° N.

4) In Section 6.3, the authors suggest an ‘instrumental bias‘ in the SBUV data due to a threshold that they regard as high relative to the average range of reported albedos. The reviewer takes issue with the authors‘ arguments in the following ways:

A) The reviewer regards an instrumental bias (or an ‘instrumentally caused truncation‘) as something different than the assignment of a high threshold for PMC detections, which is more of a data analysis concern. If one exists at all, the reviewer suggests that the authors rephrase their concerns as simply ‘a bias‘.

B) Regarding the albedo observations, it seems that there are two separate points the authors are trying to make at once. The average brightness of the MC relative to a threshold value and the spread of observed MC albedo. For both of these quantities, the optical properties at 532 nm will respond differently to variations in ambient conditions than the observations at 252 nm because the size parameter $x=2\pi r/\lambda$ is smaller at 532 nm, as indicated by the reviewer earlier. Unless the authors can show explicitly otherwise, the reviewer believes it is at least premature to suggest that the observed $\beta_{avg}/\beta_{threshold}$ and the dynamic range in β at 532 nm should be the same as the integrated directional albedo at 252 nm.

C) Optical properties aside, the reviewer again suggests care in consideration of the latitudinal coverage. The reported SBUV MC albedos are at all latitudes between 50-82° whereas the ground based MC observations are at one latitude (69°). The authors

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nonetheless directly compare observed ground based MC variations with satellite derived variations. Should one expect the same geophysical conditions driving the same MC brightnesses and variability for the two data sets? The reviewer is not so sure and requests additional details to justify.

D) Though the SBUV albedo is ‘close‘ to the threshold value used, this is not sufficient evidence to suggest an ‘instrumentally caused truncation‘. One must also consider the distribution about the mean and as indicated by the authors, this is between 9 to 12 x 10^{-6} and above the 7 x 10^{-6} threshold. Given this information alone, it is not clear that any clouds are missed at all. The reviewer requests further details from the authors on how they view the actual distribution of 252 nm MC albedo and how it relates to the observed distribution.

Unless the authors can respond clearly to concerns B, C and D, the reviewer requests that this criticism of the reported SBUV MC occurrence rate be removed from the paper.

Other General Comments

1) In Section 6.1, even if the microwave water vapor trend at 80 km is shown to be robust (see comments on this above), it seems premature to state ‘it has become evident that the episodic changes of H_2O are smaller at high latitudes‘. Perhaps ’...our data suggest that the episodic changes...‘ is a better representation of the facts.

2) The reviewer suggests a general restructuring of the paper where the data is shown first and the model afterwards rather than the other way around. In this way, the relevance of the modeling is placed in the context of the quality of the data. Thus Section 4 would become Section 2 and Section 2 would become Section 4.

Technical Comments

Thomas et al. [2004] is listed in the text as Thomas et al. [2003].

P. 3047 (bottom). It is not clear to what 2-dimensional model the authors are referring. If they are referring to the CARMA model, then to the reviewer‘s knowledge the only

CARMA results relevant to this work are those from the 1-dimensional version of this model.

P. 3055, Section 4, 1st line, ‘observations‘ should be ‘observation‘.

P. 3057, 5th line, a question mark is missing after ‘β‘. An ‘a‘ is missing after ‘at least‘.

P. 3060, ‘R=1.15‘ should read ‘1.15‘.

P. 3061, 4th line, ‘gets‘ should be ‘get‘.

P. 3062, 5th line, ‘effects‘ should be ‘affects‘.

P. 3062, 5th line from bottom, ‘80 m km‘ should read ‘80 km‘.

P. 3062, bottom, suggest referencing Siskind et al. [2004] who have submitted a model study of solar cycle effects on MC.

In Section 6.3 and Table 3, it would be clearer to give a range for F from the SBUV data. Rather than ‘F≈4.5‘, the reviewer calculates from Figure 9 of DeLand et al. [2003] that the 3.0<F<6.2.

References

DeLand, M.T., E.P. Shettle, G.E. Thomas and J.J. Olivero, Solar backscattered ultraviolet (SBUV) observations of polar mesospheric clouds (PMCs) over two solar cycles, J. Geophys. Res., 108(D8), 8445, doi:10.1029/2002JD002398 (2003).

Hartogh, P., Song, L. and Jarchow, Ch., Annual and interannual variations of polar upper stratospheric and mesospheric water vapour, poster presented at Network for the Detection os Stratospheric Change (NDSC)-Meeting, Arcachon, France, 24-27 September, 2001.

Hervig, M., M. McHugh and M.E. Summers, Water vapor enhancement in the polar summer mesosphere and its relationship to polar mesospheric clouds, Geophys. Res. Lett., 30(20), 2041, doi:10.1029/2003GL018089, 2003.

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Koerner, U. and G.R. Sonnemann, Global three-dimensional modeling of the water vapor concentration of the mesosphere-mesopause region and implications with respect to the noctilucent cloud region, J. Geophys. Res., 106(D9), 9639-9651, 2001.

Rapp, M., F.-J. Lübken, A. Müllemann, G.E. Thomas and E.J. Jensen, Small-scale temperature variations in the vicinity of NLC: Experimental and model results, J. Geophys. Res., 107(D19), 4392, doi:10.1029/2001JD001241, 2002.

Siskind, D.E., M.H. Stevens and C.R. Englert, A model study of global mesospheric cloudiness, submitted to JASTP, 2004.

Stevens, M.H., C.R. Englert, M.T. DeLand and M. Hervig, The polar mesospheric cloud mass in the Arctic summer, submitted to J. Geophys. Res., 2004.

van de Hulst, H.C., Light Scattering by Small Particles, Dover Publications, New York, 1981.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 3045, 2004.