

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

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Responses to the Comments on the ACPD manuscript 2004-0057.

Responses to Comments by Anonymous Referee #2

1. Quote from the comment of the referee: "The SBUV data series is much less sensitive to changes in water vapor than claimed by the present paper. In fact, Mie scattering calculations show that at backscattering angles appropriate to the SBUV experiment (110-130°) the dependence on H₂O is much weaker than for 180°. Thomas et al. (2003) made the same mistake".

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Response (1a): The argument is oddly worded. Mie scatter calculations can not show any dependence on ambient H₂O. Mie scatter calculations yield the dependence of the scatter cross section of a single particle on its radius or more generally, the dependence of a volume scatter cross section on the size distribution of particles. Yet, in which way a NLC particle size distribution changes with the ambient H₂O mixing ratio cannot be deduced from Mie scatter calculations. Only global 3-D models of the life cycle of NLC particles can give us a self-consistent answer to this question.

Response (1b): We agree with the referee that it would be desirable to simulate more quantitatively the scatter observations by which the SBUV instruments allow the determination of NLC albedos at 252 nm wavelength. To this end we have subdivided our former section 2 into three chapters, the third one dealing specifically with a new numerical 3-D model calculation of the sensitivity of 252 nm NLC albedo versus assumed ambient water vapor mixing ratio at 80 km and 69°N. Calculated NLC particle size distributions are shown in a new Fig. 4. Our results are as follows:

If compared to our 532 nm backscatter calculations for 67.5°N of section 2.1, the calculations of 252 nm hemispheric average residual albedo A show a smaller dependence on the background mixing ratio $f(\text{H}_2\text{O})$. This smaller dependence on the H₂O abundance is caused (a) mostly by the shorter wavelength of 252 nm and (b) by the wider particle size distribution encountered in the hemispheric sampling of the SBUV instruments as compared to our local 532 nm lidar measurements. As regards (a), at a fixed wavelength the scatter cross section of a single particle as function of the particle radius passes through a marked minimum when going from the Rayleigh scatter into the Mie scatter regime. At 252 nm this minimum in scatter cross section occurs at a single particle radius r close to 80 nm. For a particle ensemble with a lognormal radius distribution and distribution width $\sigma = 1.4$, the minimum slope of $d(\beta)/dr$ occurs, however, close to a particle radius of 50 nm which is also close to the optical radius $r(\text{opt})$ of the mean hemispheric particle distribution. For a hemispheric-mean particle size distribution, its width σ is so large, however, that the “single particle minimum”

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is very much smeared out and a genuine minimum of scatter coefficient beta versus median radius $r(\text{med})$ does not exist anymore. What remains of the minimum is only a moderate change in the positive gradient of the scatter coefficient beta with increasing $r(\text{med})$.

We summarize the basic results of the COMMA/IAP model concerning the sensitivity of NLC brightness on the background water vapour at 80 km altitude as follows: For lidar backscatter observations at 532 nm our model predicts a 22% increase of beta for a 10% increase of $f(\text{H}_2\text{O})$, whereas for hemispheric SBUV observations at 252 nm it predicts only a 12% increase of the albedo A for the same increase in $f(\text{H}_2\text{O})$.

2. Is it really the sign of the observed changes which matters most?

Response: We withdraw this statement which anyway was not part of the original text submitted by us.

3. The referee suggests that the term “episodic” might be misleading when referring to decadal time scales because it implies a timescale of the order of one year.

Response: We disagree with the opinion that "episodic events" have time scales in the order of one year. In geophysics, episodic events can have quite different time scales. Our point is that we want to allow for variations with an episodic character, lasting for a finite length of time only and not necessarily being a component of a periodic variation or long-term "trend". See also our reponse to comment # 29.

4. Quote from the comment of the referee: “... the solar cycle must be a dominant effect in causing decadal variations in upper mesospheric water and NLC. This has been pointed out in many papers dating back to Garcia (1989)”.

Response: It is a major point of our paper to show that today the work of Garcia (1989) must be considered outdated. To this end we have now included a chapter 2.2 into our manuscript. In this chapter we present a study based on the COMMA/IAP 3-D model of the sensitivity of NLC brightness at 532 nm wavelength on solar Lyman alpha

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fluxes, again for the latitude of ALOMAR (69°N) and summer solstices. We support our conclusions by the addition of two new figures #2 and #3. Our model predicts only a minor increase of NLC brightness (about +12%) when going from solar maximum to solar minimum conditions.

Why are our results much different from those of Garcia (1989)? A number of causes contribute to this result. In comparison to Garcia (1989), we input our model with (a) a stronger gravity wave momentum deposition leading to about twice higher vertical background winds and to reduced time scales for vertical transport of water vapour, (b) stronger vertical eddy diffusion leading to faster vertical mixing, (c) an improved OH-photochemistry leading to enhanced chemical heating and a different partitioning of the OyHx compounds in the mesosphere, and (d) modern data for solar Lyman alpha fluxes, comprising e.g. a smaller ratio of fluxes at solar maximum over minimum conditions. The combined effect of the changes (a) and (b) makes the time constants for vertical transport of water much smaller than those for photo dissociation of H₂O by Lyman alpha anywhere below 80 km. Thus, in our COMMA/IAP model the water vapour in the NLC region (= upper mesosphere in Arctic summer) is much less effected by variations of solar Lyman alpha fluxes than hitherto presumed.

5. Reviewer: "... they claim that the Shettle et al. paper is the only published paper wich contains NOAA-17 data. This is incorrect ..."

Response: For use in our own studies, we have digitized and analyzed the latest published data of the Thomas et al. team, assuming that it is the best data available. These data come from Fig. 6a of the paper Thomas, G.E., M. Rapp, J.J. Olivero, E.P. Shettle, and M. DeLand, Long-term variability in the brightness and occurrence frequency of mesospheric clouds explained by water vapor changes, *Memoirs British Astron. Assoc., Aurora Section*, Proc. of the "Mesospheric Clouds 2003" meeting, 18-22 Aug 2003, Galway, Ireland, 2003b.

These Proceedings contain, however, also the paper Shettle, E.P., M.T. DeLand, G.E.

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Thomas, and J.J. Olivero, Variations in the average brightness of polar mesospheric clouds from twenty-four years of SBUV measurements, *Memoirs British Astron. Assoc., Proc.* "Mesospheric Clouds 2002" workshop, Galway, Ireland, August 18-20, 2003. The fact is that the above Thomas et al. paper does NOT contain the low NOAA-17 data point, whereas Shettle et al. do show it. This led us to the statement in our manuscript on the NOAA-17 data, not considering the earlier paper of Thomas, G.E., J.J. Olivero, M. DeLand, and E.P. Shettle, Comment on "Are noctilucent clouds truly a 'miner's canary for global change?", *EOS*, Vol. 84, No. 36, 352-353, 2003a. Hence, we herewith withdraw our statement of "the NOAA-17 data being shown only in the Shettle et al. (2003) paper".

But why is the low NOAA-17 data point shown in the earlier Thomas et al. (2003a) paper and not shown in the later Thomas et al. (2003b) paper?

6. On the relative values of the Fogle and Haurwitz (1967) versus Gadsden (2002) data sets.

Response: We have recently studied this topic in depth and have added in our manuscript a paragraph in chapter 6.3 dealing with it. Our studies have led us to believe that a major cause for the very different numbers of NLC nights per year in the late-1960s given by these two sources is the rather different geographical area over which their observations were collected. Gadsden refers to the work of Paton (1968; 1969) who collected 88% of his data from a rather small longitude sector (only 10° wide). Fogle and Haurwitz present, however, a quasi-hemispherical sum of nights (see their list of references); Thus, Gadsden's statistics is effected much more by large-scale variability of NLC and unfavourable weather conditions than that of Fogle and Haurwitz. Additional factors have been identified which might have contributed to the relative smallness of the 1967/68 maximum in the Gadsden statistics. Considering this background, neither data set is useful for a robust trend analysis of the occurrence rate of NLCs (von Zahn, U., and J. Rendtel, Long-term records of visual observations of the occurrence rate of NLC: Can they be used to derive a significant trend of this

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rate?, paper presented at the “International Workshop on Layered Phenomena in the Mesopause Region”, 14-17 Sept. 2004, Cambridge, UK, 2004). We plan to publish a paper on this topic in the very near future.

Responses to Comments by Referee M. Stevens

7. The referee asks for a specification of the “average” ice particle radius.

Response: As our new Fig. 4 shows, our integration (over altitude) of the entire ice particles region leads to particle size distributions which are dominated by small particles. Such wide distributions can not easily be characterized by a single “radius” for its particles. As an example, we have included in Fig. 4 radii as typically used to describe particle distributions on purely statistically and optically weighted statistically means for the case of (H₂O) = 5.4 ppmv. The median and mean particle radii are only 5 and 8 nm, respectively. For the optical signal observed by the SBUV instrument at 252 nm, the largest contribution comes, however, from particles with a radius of 56 nm which we call the optical radius $r(\text{opt})$. 75% of the total optical signal comes from particles with radii larger than 45 nm. Furthermore, the optical radius depends only slightly (± 4 nm) on the scattering angle α . Hence, the median radius is an order of magnitude smaller than the optical radius $r(\text{opt})$.

8. Referee: He asks for model calculations of $\beta = f(252 \text{ nm}, 120^\circ)$.

Response: see our extensive response (1b) to comment #1..

9. Referee: "Why are NLC modeled after 5 days of particle growth?"

Response: Because at that time, the output of our COMMA/IAP model has reached a steady state. This time has nothing to do with an “age” of the cloud. (1) In real life and in the model, the ice cloud is permanent during the summer season, hence has no specific “age”. (2) The length of time that a NLC is visually observed represents only the time span for minor changes in the radii of the large ice particles.

10. The referee asks for comparison with HALOE water data as analyzed by Hervig et

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al. (2003).

Response: We have included two paragraphs on this topic our section 3. We note, in addition, that at least for 69°N the Hervig et al. H₂O data conflict in essential points with a large body of available PMSE radar observations and falling sphere data on the thermal structure of the upper mesosphere. The HALOE water vapor mixing ratio of 5 to 6 ppmv at 86 to 88 km imply very large supersaturation ratios throughout the Arctic summer and thus appear way too high, at the same time their climatological mean ice cloud extends to altitudes too low by about 2 km. For NLC aeronomy, these are rather large deviations, yet unexplained by Hervig et al. 11. The referee raises the very interesting question: How much water is there in the Arctic summer at 80 km altitude?

Response: We do not know accurately. The most direct observations of this parameter are those of Seele and Hartogh (1999) and Hartogh et al. (2001) who arrive at 4.0 ppmv. The derivation of gaseous mesospheric water in the presence of ice clouds (!) from HALOE data, as done by Hervig et al. (2003), is certainly a very indirect way to arrive at the ambient water vapor mixing ratio. Their result for 80 km and 69°N is about 6 ppmv. Can this value be considered more accurate than the Hartogh value? We do not believe so (see also our response to comment #11). Thus, currently we have a conflicting situation which only future and more robust observations will resolve.

12. Justify the selection of months “June-August”

Response: The optically active, hence "large" NLC particles are produced from water vapor brought up from regions below the NLC. According to our COMMA/IAP model, under Arctic summer conditions, the water vapor mixing ratio at 80 km is very little effected by the phase changing processes higher up and solar Lyman alpha (see our new Fig. 2). The time constant for vertical coupling of water vapor from 80 km upwards into the NLC region are only days as calculated from the mean vertical background wind. We presume therefore, that in comparison to the length of the NLC season this coupling time constant is very short and that there is not significant “memory effect” of

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the NLC for any longer term changes of water vapor in the upper mesosphere.

13. Referee: Why is ALOMAR water data from 2002 and 2003 not shown?

Response: We included in section 3 of our paper the following answer: "Due to an instrument refurbishment, H₂O data were obtained for only parts of the summers 2001 and 2002; The data of 2003 and 2004 are still being processed and are not yet available for this publication".

14. Referee: Improve fit periods for data.

The ALOMAR H₂O microwave data are available since summer 1996 and all relevant regression lines have been calculated accordingly (new Fig. 6 and 8).

The ALOMAR lidar data are available since summer 1997 and all relevant regression lines have been calculated accordingly (new Fig. 7).

17. Referee: SBUV albedo has unit sr⁻¹.

Response: We have corrected the incorrectly labeled figures. We also changed the letter for a SBUV albedo from beta to A throughout the paper. 18. This referee, as referee DeLand too, takes issue with our suggested "instrumental bias" of the SBUV observations.

Response: (1) We take this remark as an overdue lesson in English and have eliminated this word entirely from our manuscript.(2) We note, though, that in his introduction, the Anonymous Referee #2 specifically agrees with us on the topic! In addition, at the Cambridge Workshop on "Layered Phenomena in the Mesopause Region" in September 2004, Shettle et al. presented a paper in which they arbitrarily lowered the detection threshold of the SBUV instruments with dramatic changes of the ensuing NLC parameters. Thus, even the authors, who are responsible in the first place for the published SBUV NLC albedo values, agree with our suggestion that there is a significant effect of the chosen SBUV detection threshold on the deduced climatological means of NLC parameters.

19. The referee starts with a comment on long-term variations in NLC properties and then introduces the argument that solar Lyman alpha is a (strong) forcing mechanism for NLC.

Response: Our paper concentrates on discussion of the past decade (see the title of the paper) and the unexpected changes in water vapor mixing ratio in the mesosphere. Concerning the presumed forcing of NLC by seasonally changing solar Lyman alpha, see our response to comment #4.

20. The referee voices the opinion that for the SBUV instruments, "the scattering geometry is fairly simple".

Response: With due respect to the opinion of the referee we note that SBUV instruments represent only one of various types of satellite instruments which have been used to study NLC in one way or the other. Even for the nadir-viewing SBUV instruments, the scattering angles for solar photons vary from about 100 to 130°, while the groundbased lidar operates on a single scatter angle (= 180°).

21. The referee's remark on "the drastic change of the H2O trend in the mid-1990s":

Response: Again we see the presumption that the observed H2O variations are explained by solar Lyman alpha variations. We do not know of any numerical model which has proven this cause-and-effect argument. We emphasize, though, that we do NOT dispute a variation of NLC occurrence rate and brightness which has a period close to that of solar cycle activity. But there exist numerous solar indices varying with the same period. The fact that Lyman alpha shows this period is no proof whatsoever that photodissociation of H2O by Lyman alpha is the "culprit". In fact the unexplained phase shift between the solar Lyman alpha flux and NLC reaction is another argument against this simple answer. See also our response to comment #4.

22. The referee's states that "An increase in occurrence rate implies the creation of

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more new NLC particles, whereas an increase in NLC brightness suggests growth of the existing particles".

Response: We wonder how the referee might prove his point of view. We prefer to keep the more general view that both the occurrence rate and brightness depend on both the number and the size (distribution) of the particles.

23. The referee takes issue with our suggested "instrumental bias" of the SBUV observations.

Response: Accepted. See our response to comment #18.

24. The referee's remarks on long-term trends in NLC occurrence rate.

Response: We deleted entirely any discussion of the long-term visual records of NLC occurrence rate from chapter 6.5. We plan to publish soon a paper on our inability to use these observations for any derivation of long-term changes of NLC parameters. Hence, we decided not to argue even slightly the other way around in the present manuscript.

Responses to Comment by F. Lübken

25. The reviewer asks for a comment on the observed episodic increase of 80 km water mixing ratio versus an observed decrease of lidar-observed NLC brightness.

Response: We have included a new Fig. 7 on which we comment in chapters 4.1 and 5.1. We recognize that over the 5-year period of these two series of measurements the observed decrease of NLC brightness $\beta(n)$ can not be satisfactorily explained by the increase of solar Lyman alpha flux nor is it in accordance with the observed summer increase of water vapour. There are a number of scenarios which might be responsible for this situation: (1) The observed changes of NLC brightness are due to other causes than considered so far. Candidate causes are small changes in the thermal structure of the mesopause region and/or changed dynamics; (2) The wave-driven variability of NLC brightness is simply too large to allow any robust conclusions from a short observation period as ours; (3) We are still missing an important process

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in our modelling of the polar summer mesopause region. We compare this situation with that of the water vapour in the middle mesosphere which we have shown here to have exhibited dramatic changes in the past decade which are also not understood and which are still beyond any robust model calculations.

26. The reviewer asks for a more precise definition of NLC "brightness" in order to allow a better comparison of models and observations.

Response: We have now adopted two different definitions of NLC "brightness". For the case of groundbased lidar observations at 532 nm, we use the volume backscatter coefficient beta as a quantitative measure of NLC brightness. For the case of the SBUV satellite observations, we use the NLC residual albedo A at 252 nm. Furthermore, we use two very different model runs to numerically calculate either one of these parameters. Hence, the comparison between models and observations has become much more direct than before.

Responses to Comments by Anonymous Referee #3

27. The referee asks for more details on the Arctic upper mesospheric water water observations and their analysis, as well as how the episodic changes are determined and in particular the error estimates?

Response: The microwave spectrometer, its observational techniques, and data analysis procedures have not been changed in any basic way since the paper of Seele and Hartogh (1999) was published. Furthermore, this paper already contains summer and winter data. As we are dealing with the data of many instruments in our current paper, we prefer to keep the instrument descriptions reasonably short. We have also noted in the paper the sincere intent of P. Hartogh to publish soon a paper dedicated to the recent results of his instrument.

28. Referee: What is the cause of "significantly decreasing water mixing ratios"?

Response: We do not know for sure. We just point out that since the mid-1990s (which

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is now a decade ago) the observations do NOT show the predicted global increase of mesospheric water vapor mixing ratio.

29. Referee: The term "episodic" is not suitable for describing changes that are likely related to solar cycle or slowly increasing greenhouse gas emissions.

Response: We agree. But we do not agree with the referee that the two causes which he cites must be considered the only causes for change of NLC. As we point out in our paper, many more factors need be considered, some of which we evidently do not understand at all. One outstanding example are the observations of the water vapor mixing ratio in the middle and upper mesosphere the variations of which are definitely not understood in terms of the above suggested two causes. Hence, they are genuinely episodic in character in the sense of starting and stopping in a (yet) little understood fashion.

30. Referee: How has epsilon be derived and what are its uncertainty?

Response: We have expanded on our explanation of the derivation of epsilon which can be found now in chapter 2.1. Concerning the uncertainty of epsilon, its largest part comes from the nonlinearity of beta with the assumed water vapor mixing ratio at 80 km. The respective numbers are given in the text. As reference point on this slightly non-linear curve $\beta = f(\text{H}_2\text{O})$, we have chosen the mean beta-value as measured by our ALOMAR lidar. This situation introduces an uncertainty of epsilon of about $\pm 30\%$. In no way can it change the sign of the predicted change.

31. Referee: What are the altitudes for the solar occultation data?

Response: The highest altitudes, at which episodic changes have been derived from the data, are given in table 2.

32. Referee: Suggests to show in Fig. 5 the full annual cycle of H₂O.

Response: As the referee remarks correctly, the suggested figure would be dominated by the strong seasonal variation of H₂O in the Arctic upper mesosphere. But it is exactly

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for that reason that we have focussed on presentation of the summer months. NLCs are strictly a summer phenomenon and we prefer to keep the focus on this season.

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

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