

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

**Anonymous Referee #2**

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The following is my report on manuscript acpd-2004-0057, entitled "Noctilucent Clouds and the mesospheric water vapour: The past decade" by U. von Zahn, U. Beger, and J. Fiedler.

General comments: This manuscript, entitled "Noctilucent Clouds and the mesospheric water vapour: The past decade", (acpd-2004-0057) presents new measurements on variations of mesospheric water (H<sub>2</sub>O) and noctilucent clouds (NLC) in the past decade. It also uses a state-of-the-art model to determine a theoretical sensitivity of NLC to H<sub>2</sub>O variability, and attempts to answer the question of how NLC are controlled by H<sub>2</sub>O fluctuations on a decadal time scale. The subject is one of recent debate in the literature and at meetings, and is thus timely and scientifically interesting. The authors offer new insight into explaining the very different behavior of decadal-scale changes in satellite-derived NLC albedo and NLC frequency. The high threshold of

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SBUV certainly must play a role in explaining at least part of the difference. The paper is well written, and the figures are clear and appropriate.

Specific Comments: On the other hand, there is one major problem, and several minor ones, with the methodology, discussion and conclusions, I request the authors consider the following items. One item relating to the methodology is of crucial importance, as the conclusions will differ considerably if the correct methods are applied. The other items deal with terminology, motivation of the paper, omission of an important reference, and the lack of discussion of solar effects and possibilities in explaining the latitude-dependent trend of water.

(Crucial): The use of the quantity  $b$  (beta) to quantify the brightness of both lidar and satellite backscattering measurements is incorrect. As defined in the literature,  $b$  applies only to the backscattering geometry (scattering angle  $=180^\circ$ ). The dependence of the scattering on particle radius (through water vapor changes) at other angles is very different from that at  $180^\circ$ . In fact, Mie scattering calculations show that at backscattering angles appropriate to the SBUV experiment ( $110$ - $130^\circ$ ), the dependence on  $H_2O$  is much weaker than for  $180^\circ$ . The value of the quantity  $\epsilon$  is in fact closer to unity for these angles, and for the relevant water content. The authors value of  $2.1$  is appropriate but only for backscattering geometry. In other words, the SBUV data series is much less sensitive to changes in water vapor than claimed by the present paper. Their agreement with the earlier results of Thomas et al (2003) is due to the fact that these authors made the same mistake, i.e. scaling the backscattering results to other angles. It is a simple matter to incorporate the angle dependence into the calculations. The authors seem to be aware of this, but attempt to sweep it under the carpet in their statement "it is the sign(!) of the observed changes with time which matter most for our discussion and not so much the absolute value." However they then proceed to use their quantitative results to support their hypothesis, and to criticize other claims in the literature having to do with the value of the sensitivity of NLC brightness to water vapor changes. Their discussion goes way beyond

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discussing the sign of the changes. For example, Figure 4 specifically makes the point that the microwave time series (for summer) showing positive increases of water best fits the SBUV time series from 1996 onward, and that other water vapor data sets for lower latitudes (showing negative changes) do NOT fit the time series. I would argue that because of the insensitivity of the SBUV measurements to water vapor changes, that this conclusion is no longer valid. The small length of the time series, the scatter in the data coupled with the uncertainty of solar effects which the authors do not address at all, probably allows a much wider range of variability than allowed by the authors, probably including negative changes. We won't know the answer until they redo the calculation with the correct sensitivity factor.

Another example of their use of their quantitative results for the water vapor increase is to contrast their value (+2.3%/decade) with the higher value of Thomas et al (1989) of +6%/decade. This statement clearly violates their earlier disclaimer that they are concerned primarily with the sign of the change. I would guess that in fact when the proper sensitivity factor is used, the 6%/decade number is more appropriate for high-latitude summer conditions. Also, it should be made clear that the Thomas et al value was theoretical, and based on the assumption that the future increase would be purely due to methane increases with a constant value of 1.3% (which was the best value for the methane trend at that time). We now know that the global increase of methane has decreased considerably in the past decade.

My opinion is that the term 'episodic' is misleading when referring to decadal time scales. The authors quote Randal et al (2000) for this term, but my study of this paper revealed only two uses of the term 'episodic', the first (page 277) refers to warming by the El Chichon and Pinatubo volcanic eruptions, and the other usage referred to an after-effect of Pinatubo. Why not use 'decadal-scale' variability? This term doesn't imply anything about the character (e.g. whether it is non-linear or periodic). 'Episodic' refers to a distinctive event, such as a volcanic eruption, or randomly-spaced series of impulsive events, such as volcanic eruptions. My opinion is that the implied time

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scale of an episodic event (from start to finish of the forcing function) is of the order of one year, and not ten years. This is an important issue because these terms tend to propagate in the future literature.

In the introduction, and throughout the paper, the reader must be curious as to why water vapor should vary with time at all, why it is interesting, etc. No discussion is given of the methane-oxidation hypothesis, nor of solar-cycle variability. The author's desire to stick strictly to empirical data and models is commendable, but leaves out the motivation for the research. On a more practical matter, the solar cycle must be a dominant effect in causing decadal-scale variations in upper mesospheric water, and NLC. This has been pointed out in many papers dating back to Garcia (1989), but is ignored in the current paper. Thus for example, on page 8, they state that "from 1987 to 2002..the upper mesosphere at 20N lost..12% of its total water content". No explanation of this result is given anywhere. Isn't it obvious that this is because the solar UV irradiance increased by a factor of two between 1996 and 2002? (Furthermore, it is not really "lost", just cycled into molecular hydrogen.) This solar cycle dependence of water above 70 km has been emphasized in a number of recent papers (e.g. Randal, 2000), but the authors avoid discussing this effect. They only mention it in a modeling context, but a 10-11 year cycle is very obvious in the SBUV data set of DeLand et al (2003), and in the European NLC data set. I have seen no long-term data sets that are good enough to definitively rule out the 11-year period, so it is very much a candidate for at least indirectly forcing the atmosphere to respond with the solar cycle.

The authors should be aware of the rebuttal to their EOS paper by Thomas et al (EOS, vol. 84, no. 36, September, 2003)? The paper is not referenced, nor mentioned, except in a very off-handed way on page 17, where they state that "we consider a total rejection of the data record of Fogle and Haurwitz (1974) as unfounded and hence an unacceptable approach (von Zahn, 2003)". They are obviously referring to statements in the Thomas et al EOS paper. It is also important to refer to this EOS paper in quoting SBUV albedo variability, because the Shettle et al (2003) reference in a workshop

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proceedings is not yet refereed or published. Furthermore they claim that the Shettle et al paper is the only published paper which contains NOAA-17 data. This is incorrect, as is shown in the Figure of the Thomas et al EOS reference.

I found the paragraph on page 3064 discussing the ground-based record to be self-contradictory. On the one hand they find that the European data set and the Fogle-Haurwitz [1974] data set to contradict one another, in regards to the 1967 maximum. They acknowledge the difficulty. The authors proposed the explanation that the European data was just starting in the 1960's and perhaps the activity was artificially small. However they do not criticize the "equally-believable" Fogle-Haurwitz data, which showed a large peak in 1967, despite the problems admitted by Fogle and Haurwitz themselves in their earlier 1966 paper that "the apparent variation in the number of NLC reported per year is likely due more to the fluctuating interest in NLC over the years than to a real variation in NLC activity." (See also the Thomas et al EOS paper for a discussion of this point.) However at the end of this paragraph, their solution is (wisely) to ignore the pre-1970 data, despite their considering the rejection of the F-H data as "an unacceptable approach". Didn't they just reject it themselves? If the F-H data were indeed better than the European data, why not use it instead?

A possible explanation of the latitudinal variation of the water vapor trend lies in the balance of solar cycle and upward transport, and how these influences (but particularly upward winds) vary with latitude and season. Except to refer to the unpublished results of Sonnemann and Grygalashvyly, the non-expert reader must again draw his/her own conclusions as to the possible cause, but with no guidance as to possible mechanisms.

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

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