

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

Anonymous Referee #3

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This manuscript presents new measurements of high-latitude water vapour over 5 year period, and through the use of models relates the observed variability to changes in NLC characteristics. The results are very interesting, well presented, and provide valuable timely information for those studying NLCs. However, I have several general comments that I feel should be addressed before recommending acceptance of this manuscript.

General comments:

1) Considerably more detail needs to be given on the Arctic upper mesospheric water vapour observations and their analysis:

First, the reference to a poster is really not sufficient. This dataset is the only new observational data presented in the paper, and much more detail is necessary on the

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resolution and accuracy of the instrument. For example, what is the vertical resolution around 80km? Does it change with season, since water vapour is considerably higher during the summer months. I think this ground based dataset provides a unique view of mesospheric water vapour, it is deserving of a more detailed description.

Second, how are the "episodic changes" determined, and in particular the error estimates. One of the interesting results from the paper is that changes appear to be different depending on whether the full dataset is chosen (-0.045 +/- 0.006) or just summers (+0.05 +/- 0.01). However, different heights and periods were chosen for the analysis, so it is not clear that seasonality is the sole cause of the difference. Was the dataset "de-seasonalized" prior to analysis? From Figure 2 it does not appear so, but Randel and others typically do this prior to analysis, and we are asked to compare the results to those based on analysis of HALOE data. I'm very concerned that omitting the endpoints affects the calculation so much – it calls into question the small error quoted (+/- 0.01) and the significance of the quoted change. Have the authors tested the significance of the result? If so, how were the degrees of freedom determined? I would guess its number is closer to 5 (the number of seasons) than N-1 (where N is the number of days of observations). If one takes seasonal means and performs the same linear regression is the change and error the same? Statistical significance is crucially important in interpreting the new data – much effort could be expended in the future in trying to explain the claimed "strong seasonal variations", whose significance has not been proven.

2) The reader is left to wonder what is the cause of "significantly decreasing water mixing ratios." The discussion should, at a minimum, mention that the period under consideration occurs during the ascending phase of the solar cycle, and that Lyman-alpha photolysis (which varies by a factor of 2 over the solar cycle) is the dominant destruction mechanism for water vapour. In addition, it seems relevant that Lyman-alpha photolysis will vary considerably with zenith angle, and at the highest latitudes, will be much reduced due to the increased path length. Therefore, it seems reason-

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able that the solar cycle response should vary with latitude, and lead to a differing "episodic change". I seems relevant also to point out the expected influence of increasing methane emissions, which would likely be independent of latitude.

3) The term "episodic" is not suitable for describing changes that are likely related to solar cycle or slowly increasing greenhouse gas emissions. Also, since a cornerstone of the paper is a 5-year dataset (and satellite analysis over a similar period), I do not think the title is appropriate.

4) The paper uses a model to estimate changes in albedo from changes in water vapour. The simulated changes are then compared with satellite records, and shown to be consistent with a "near zero trend." This seems to depend critically on the determination of epsilon (p3050 l17). There are likely to be large uncertainties in the value of epsilon due to unknowns in the model simulations. This calls into question the trend extrapolations shown in Figure 4. Perhaps the authors to speak to this uncertainty.

Specific comments:

p3050: It is not clear to me how epsilon is determined in section 2. Assuming 'const' is in fact a constant, I obtain numbers for epsilon that are quite a bit larger. Could the authors expand on this? Is const actually a function of temperature and other factors?

Table 2: What are the altitudes for the solar occultation data?

Figure 2: I think this Figure would be much improved if the full dataset were shown, and the summer periods indicated. The Figure certainly does not show the full picture, which is a very large seasonal variation in water vapour.

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