Atmos. Chem. Phys. Discuss., 3, S1342–S1347, 2003
www.atmos-chem-phys.org/acpd/3/S1342/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "First observations of noctilucent clouds by lidar at Svalbard" *by* J. Höffner et al.

J. Höffner et al.

Received and published: 19 August 2003

General Remarks:

We appreciate the comments from the reviewer and have taken his suggestions of improvements into account. We would like to comment his main concerns (water vapor distribution and temperature uncertainties) first and then respond to his remarks point by point.

We agree with the referee that the (unknown) vertical water vapor profile is a potential source of uncertainty for the calculation of the degree of saturation (S). In fact, we have discussed this uncertainty at various places in the manuscript and we have expanded this discussion in the revised version (see below for details). For reasons outlined below, however, we do not agree with the referee's recommendation to use a profile with a water vapor peak of 10-15 ppm at ~82 km similar to those published in Fig. 7c of Rapp et al. (2002) or in Summers et al. (2001). The profile shown in Fig. 7c of Rapp et al. comes from a 1-dimensional microphysical model which makes idealized assumptions (as every model), for example that the background conditions are stationary for many hours. Furthermore, it does not take into account horizontal transport etc. The experimental results published in Summers et al. show only some very few profiles and some of them even don't show a water vapor peak. Furthermore, these measurements were made at a different latitude (appr. 10 degrees further south) compared to our measurements. We would like to note that even the profiles in Rapp et al. and in Summers et al. differ substantially above ~85 km. In summary we do not see convincing evidence that the water vapor profile in the polar mesosphere always shows a 'peak' due to freeze drying. The best we can do is to take a H_2O profile from a 3-dim model which takes into account the effect of freeze drying. Results from such a model have recently been published (von Zahn & Berger, J. Geophys. Res., 108(D7), 8451, doi: 10.1029/2002JD002409, 2003.). This model indeed shows that the magnitude of the water vapor peak decreases towards the pole. In the revised version of our manuscript we have used two model profiles for H_2O: one from Körner & Sonnemann (as before), and in addition one from von Zahn & Berger. We have discussed the implications of the difference in water vapor in the revised version of our MS. We have also included a discussion on the water vapor results from HALOE.

As requested by the referee we have expanded our discussion on the uncertainty of FS temperatures and the implications on the calculation of S. This includes the potential uncertainty of the 'start temperature' and its connection to the K lidar temperatures. Apart from this, we have deleted the comparison between FS and K lidar temperatures since this is not the main point of our paper and will be published soon in a separate paper. We would like to note that a $\pm$10 K uncertainty in the start temperature at 94 km results in an uncertainty of below 1 K at NLC altitudes (83 km) which corresponds to an uncertainty of about 30% in S (see below for more details).

We now come to a detailed discussion of the referee's comments point by point.

We agree with the referee and have added a qualification on the NLC trends.

We agree with the referee and have removed the statement comparing the upper and lower gradient.

The implication stated on p.529 (l.5) is indeed not given in this way, as the referee has noted. Our data analysis neither show any case of an NLC reaching into the K layer nor an portion of an NLC well inside the layer with the given sensitivity of $\beta > 10^{-10}$/m/sr, which holds for the whole range of the layer when the temporal resolution is reduced. Therefore, we still assume that there are errorneous influences of the slightly reduced sensitivity in the layer to our statistics.

As outlined above we have now used two model profiles of H_2O to determine S, where one model (von Zahn & Berger, 2003) includes the freeze drying effect. In Table 3 we now list S values using both models. The main conclusions in our manuscript, namely that S varies greatly at NLC altitudes (including values smaller and greater than one), remains unaffected. We note, that we have already discussed the effect of a temperature uncertainty on S (p. 531, line 12). Following the request from the referee we have expanded this discussion. For example, at NLC altitudes a factor of 10 in H_2O corresponds to an uncertainty of $\pm$8 K in temperature, significantly larger than the FS temperature uncertainty at NLC altitudes.

In section 3.2 we have removed the intercomparison between FS and K lidar temperatures since this is not the topic of this manuscript but is published in a separate paper (Lautenbach et al., in preparation for publication in Geophys. Res. Lett.). We have expanded the discussion on the FS temperatures uncertainties, including the potential effect of the 'start temperature'.

ACPD

3, S1342–S1347, 2003

Interactive Comment

The statement has been re-evaluted as outlined above.

The rates of evaporation were calculated with the equations given by Gadsen (Planet. Space Sci., 1981).

As requested we added a statement on S variation with [H_2O]

Steady state models assume time-independent vertical wind, temperature and water vapour concentration. As discussed in detail by Berger and von Zahn (JGR, 107(A11), 1366, 2002) icy particles grow as long as S>1 (Eq. 11). The particles fall while they grow and reach the larges size at the S=1 level. Below the particles melt and their size decreases. Because the BSC depends on r^6, this implies that the largest BSC is found at S=1. This is still valid for models which include freeze drying.
Only if time-dependent conditions are taken into account the situation may change. The vertical wind when modulated by tides and gravity waves can cause the maximum BSC to appear away from the S=1 level, which can be seen from their Figures 31 and 33. The uncertainties are discussed above, and the deviation from S=1 remains evident.

In the work by Rapp et al. (2002) values of S were found in the range of about 0.2 to 10, in the presence of gravity waves. The values are now included and disussed in the MS.

Is answered in the context of ad 9.

We have recalculated the S values and now show 2 values using H_2O values from the two models mentioned above. We also show 2 frost point lines in Figure 6 and 7.

Yes, we wanted to state that the ice particles have probably encountered various background conditions (temperatures, water vapor etc.) if they are transported for several hours. A detailed discussion of this topic requires comprehensive model studies and is

ACPD

3, S1342–S1347, 2003

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

beyond the scope of our paper. We have modified the text to be more specific.

A more quantitative comparison with results given by Thomas et al. [1991] is problematic as they cover one season on the southern hemisphere only. The occurrence frequency in that case increases by a factor of 10 from about 0.4% towards 4% for SBUV between 69S and 78S. We have been in contact with G. E. Thomas and were provided with mean frequencies from the satellite observations which are not yet published. The frequencies are 20-40% at 69° and 50-70% at 78° which is in line with the lidar observations. The proposed normalization of the NIR lidar measurements to the UV wavelength and scattering angles of the satellite observations is not possible without exact knowledge of the particle size distribution. Furhermore, we think it not necessary as for both lidar and satellite occurrence rates from the two latitudes are available and can be compared. This has also the advantage of using the same observation periods for both comparisons.

Comments to 'Technical comments':

We have taken the suggestions for improvement into account and have made the appropriate changes in the manuscript. We would like to comment some of the topics separately:

done

done

done

Due to the limited dynamic range of a colour scale we prefer the saturated scaling. If the (linear) scaling is changed to exceed $20 \cdot 10^{-10}$/m/sr there are hardly any structures visible apart from the peak near 5 UT. To avoid misinterpretation the maximum value is now included in the figure caption.

ACPD

3, S1342–S1347, 2003

The term ‘center of mass’ in our MS does not refer to the physical mass of the cloud. It should have read the mathematic term ‘center of gravity’ and was meant only to clarify the word ‘centroid’ for which different interpretations can be found in the literature. To avoid confusion we now give the definition of the geometric centroid $z_c = \sum(\beta \cdot z)/\sum\beta$ of the BSC profile as an explanation.

done

done

done

Kühlungsborn, 19 August 2003. (F.-J. Lübken, J. Höffner and C. Fricke-Begemann)

ACPD

3, S1342–S1347, 2003

Interactive Comment