

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

### **Anonymous Referee #1**

Received and published: 10 March 2003

This paper presents observations of noctilucent clouds (NLC) near 78° N and represents an important contribution to the NLC record, which is primarily comprised of observations at much lower latitudes. The essential details of these NLC are provided including their brightness, their altitude and their thickness, which will all be useful for comparisons with the much larger database available from lower latitudes. These results from high latitudes will also be useful for modeling the distribution of NLC over the polar cap. Simultaneous temperature measurements from meteorological rockets are also included and the authors use these data to draw conclusions about the appearance of NLC and the degree of supersaturation in the polar summer mesosphere.

The two biggest concerns that the referee has with this work are as follows: The reliability of the temperature measurements is unclear and the assumptions about the vertical distribution of water vapour in the polar summer mesosphere are outdated.

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The referee recognizes that the falling sphere technique of measuring temperature has heritage, but is concerned that the authors are not considering its limitations, particularly in the upper portion of the retrievals. Both temperature and water vapor impact the calculated degree of supersaturation, making the interpretation of the NLC environment much more uncertain than the authors indicate. The referee requests that the authors revise their analysis in light of these concerns.

General comments on the scientific content follow and more technical comments are listed at the end.

1) Section 1. P. 523, Line 13. The revision to the Gadsden [1998] data showed that there was no observed trend since the mid-1960s. This qualification should be added since there has been an upward trend in reported sightings since the 19th century and the ground-based data are necessarily limited by subjectivity and viewing conditions.

2) Section 3.1, pp. 528-529. On the top of p. 529, the authors state that the top and bottom of the NLC is 85.1 and 82.5 km, respectively while the average altitude is 83.5 $\pm$ 1.2 km. They then state that 'This indicates a steeper gradient at the lower edge of the layer' which is not justified by the quoted uncertainty. This statement is misleading and the referee asks that it be removed.

3) Section 3.1, p. 529. The authors claim that they are not missing any NLC because 'The upper edge of the NLC layer is always located below the lower edge of the potassium layer.' This presumes that the distribution of ice particles is falling off monotonically with altitude above the NLC, for which they show no evidence. Unless they can show that this is the case, the referee requests that this paragraph be removed.

4) Section 3.2. For reasons that are entirely unclear to the referee, the authors have ignored the latest work on the vertical distribution of water vapour in the Arctic summer mesosphere. Observational evidence from two satellite-borne experiments [Summers et al., GRL, 28, 3601, 2001] as well as microphysical modeling results [Rapp et al., JGR, 107, 10.1029/2001JD001241, 2002] have all shown that a water vapour layer

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exists near 82 km in the polar summer mesosphere. Model results show that this layer arises from the condensation, sedimentation and sublimation of ice particles in the Arctic summer and reported peak mixing ratios are 10-15 ppmv.

More important to the submitted work, however, is that this layer builds at the expense of the water above it so that the upper mesosphere becomes severely dehydrated. The authors use two cases to calculate the degree of saturation: a constant 5 ppmv of water vapour and a model profile from Körner and Sonnemann [2001]. The second case is from a large-scale model that does not contain the required microphysics and as far as the referee can tell, the first case is not supported by anything. Why not use the water vapour profile at the end of the 24-hour simulation of ice particle growth and decay from Figure 7c of Rapp et al.? This would be much more consistent with the current understanding of water vapour in this region and would also simplify Figures 6 and 7 (only one frost point temperature line).

Dehydrating the upper mesosphere significantly impacts the calculation of frost point temperatures in Figures 6 and 7. In particular, the region of supersaturation would be narrower than what is now shown and this is critical to understanding where NLC ice particles condense. Results from their calculations of S in Table 3 would also change.

5) Section 3.2. The referee is unconvinced that the lidar temperature profile and the falling sphere (FS) profile in Figure 6 show 'nice agreement'. First of all without error bars the reader has no indication whatsoever of what might be 'nice'. The referee strongly requests that the authors put error bars throughout the altitude ranges of reported temperatures (FS and K lidar) in Figures 6 and 7. This temperature uncertainty must be propagated into the calculation of frost point temperatures.

In addition, the authors themselves state that the 'start temperature' of the FS profile is defined by the K lidar temperature so it is misleading to suggest agreement anywhere near this altitude. Although it is certainly useful to have the FS boundary condition provided by the K lidar, the authors need to show how dependent the FS temperatures

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at 89-90 km are on the boundary condition at 94 km. For example, if the 94 km temperature were 10 K higher or lower, how much does the FS temperature change at 89 km? The contribution to the uncertainty in temperature at 89 km due to the start temperature should be called out separately from their total uncertainty.

Also, why were the K lidar temperatures averaged and smoothed in the analysis and if this was done before the FS data reduction, why are the 94 km points not identical? Please elaborate on this point in the text. The referee also requests that the authors indicate the vertical resolution of both the K lidar temperatures and the FS temperatures at 89 km.

6) Section 4.1. P. 531, Lines 4-5. The sentence 'there is an extended height range with super-saturation ( $T_{atm} < T_{frost}$ ) at approximately 82-92 km' must be re-evaluated in light of comments #3 and #4.

7) Section 4.1. P. 531, Lines 10-11. The decay of an ice particle depends on many factors, including temperature, fall speed and vertical wind. Please reference the statement 'an ice particle with a radius of 20 nm will exist for several hours before it is completely evaporated'.

8) Section 4.1, Page 531, Line 12. In light of comment #3, please include an additional statement at the end of the first paragraph indicating how S varies with a factor of 10 change in the local water vapour.

9) Section 4.1. P. 531, Lines 13-15. It is not clear to the referee which studies assume  $S=1$  at the NLC peak. In addition, uncertainties in temperature and water vapour together with the narrowness of the NLC make the referee skeptical about the authors' ability to say anything about where  $S=1$  in relation to their NLC. The referee requests that this sentence be removed.

10) Section 4.1. P. 531, Line 21. Please indicate the range of S values found at the NLC peak determined by Rapp et al., compare these values to the range of S calculated by

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authors and comment on the differences.

11) Section 4.1. P. 531, Lines 27-28. It is not clear what the authors mean by 'arguments derived from steady-state assumptions may not be applicable' Please be more explicit: What is it about the formation environment that they think is uncoupling the peak BSC from the super-saturation?

12) Section 4.1. P. 532, Lines 8-9. Given comments #3 and #4, the S values of 26, 2.9 and 0.2 should be recalculated with a range of values for each case. Given uncertainties in both temperature and water vapour between 87-88 km, the referee believes that the two significant figures in these numbers are not justified.

13) Section 4.1. P. 532, Lines 19-22. What are the authors saying about transport of an ice particle by the background winds? Are they saying that the particle may have seen many different temperatures and/or water vapour concentrations during its transport, which determine the final BSC observed? If the temperatures between 69° N and 78° N are really similar, can the observed variation in BSC be driven by gravity wave variability? The authors need to be more explicit.

14) Section 4.1. P. 533, lines 5-16. The high latitude (78° N) observations of NLC compared with those at 69° N provide a good opportunity for intercomparison with satellite PMC observations. The authors indicate that increasing frequency with increasing latitude is observed in the PMC record. However, they can be much more quantitative than that. Figure 7 of Thomas et al. [1991] shows a two-satellite comparison of PMC frequency with latitude. The authors can normalize their results in wavelength and scattering angle to the satellite results. The reader would like to know if the factor of 2-3 change in NLC frequency from 69° N to 78° N is consistent with published PMC results.

Technical comments:

1) Section 1. P. 523, Line 2. The reference of Jesse [1889] is so ancient that the referee

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must presume that the authors would like to cite the first reported NLC observation in the scientific literature. Jesse, however, was not the first to report an NLC. This distinction belongs to Robert Leslie [Nature, 'Sky Glows', 32, 245, 1885] and is the appropriate reference here.

2) Section 1. P. 523, Line 11. Gadsden [1998] did not suggest that the NLC increase is related to anthropogenic change so this reference should follow 'in the last decades'.

3) Section 1. P. 523, Line 14. Since it was Lübken [2000] who studied the temperature trends in the mesosphere the Kirkwood and Stebel [2003] reference should follow the statement 'there is in fact no observed trend since the mid-1960s' to avoid ambiguity.

4) Section 2.1. The color scale in Figure 1 is saturated for BSC and should include the entire dynamic range of the observations, including the reported maximum BSC of  $20.1 \times 10^{-10} \text{ m/sr}$ .

5) Section 3.1. P. 528, Line 19. Do the authors really mean centroid=center of mass? This would mean that the distribution of ice particle sizes is known well enough to determine this point. Perhaps they mean the midpoint altitude of the NLC?

6) Section 4.1. P. 532, Line 17-18 should read 'in the lower part of the height range of super-saturation but details...'

7) Section 4.2. P. 532, Line 1 should read 'R/M/R-lidar at ALOMAR (69° N) have recently been...'

8) Sections 4.2. P. 533, Line 1 should read '...our statistics are poorer compared to...'

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 521, 2003.

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