**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "On microphysical processes of noctilucent clouds (NLC): observations and modeling of mean and width of the particle size-distribution" *by* G. Baumgarten et al.

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General comments: This study deals with LIDAR observations and CARMA modelling results of the mean and the width of Noctilucent cloud (NLC) particle size distributions (PSD). Multi-colour LIDAR observations at the Alomar observatory spanning more than a decade are used to infer the two free parameters of an assumed Gaussian NLC PSD. For mean sizes of less than about 40 nm the observations show a near-linear dependence of the width and the mean of the PSD, which is an interesting phenomenon. The observed de- pendence of width and mean of the PSD are qualitatively reproduced by

the model simulations. Sensitivity studies with different eddy diffusion profiles indicate that turbu- lence is one of the main drivers for most of the observed features of the dependence of width on mean size of the PSD. The paper is interesting, well written and of importance for the NLC community, which appears to be in the process of finding ways for comparing different NLC particle size retrievals. I have no major objections to the publication of this manuscript, but believe that the paper can be further improved if the authors consider the specific comments listed below. Especially, the eddy diffusion profile(s) used should be discussed in more detail as described in the specific comments below.

Specific comments:

Page 3607, line 3: "or dynamical parameters like wave activity". This is really a minor point, but the effect of wave activity on NLCs is directly related to temperature and H2O, since these are changed. Therefore, the third process mentioned is directly coupled to T and H2O and not independent. Again, a really minor point.

We agree but prefer to mention the wave activity explicitly as it impacts the time available for NLC particles to grow.

Page 3608, lines 2-4: You mention that the method is appropriate for mono-modal PSDs with non-spherical particles, but the assumed particle shape is not mentioned here or anywhere else in the manuscript, as far as I can tell. This should be done. The differences in mean sizes between the different particle shapes is not large, as shown in several other publications by the first author; still, the shape should be mentioned.

We updated the manuscript and mention the shape.

Page 3608, lines 11/12: Here the thresholds for the colour ratio errors are introduced ad hoc. It would be good to briefly explain where these values come from.

We updated the manuscript and include a motivation.

Page 3608, lines15/16: I suggest explaining briefly how the upper, peak and lower

layers are defined (Method 2). You refer to Baumgarten and Fiedler two sentences later, but an explicit definition will make the paper easier to understand.

We updated the manuscript and explain the definition of the layers in the paper.

Page 3609, line 12: "particles soundings" → "particle soundings"

done

Page 3612, line 25: "agree to" → "agree with"

done

Page 3612, last line: you mention that wave mixing is not included in the model simulations. Can you give a rough estimate on the relative strength of turbulent and wave mixing. How strongly do you expect the model results to be affected by wave mixing, if the latter were included?

Unfortunately we can not answer this question by now due to the nonlinearity of the NLC microphysics. However there are several publcations highlighting the importance of wave mixing on the sodium layer (e.g. Liu and Gardner, 2004).

Page 3613, lines 1-3: It would be good to include a bit more information on the eddy diffusion coefficient profile used for the simulations. The source of the profile is only mentioned in the Fig. caption. How representative is this profile? How variable is it in the real atmosphere? Are the factors 0.1 and 10 representing the expected variability? This information would be useful for the readers to judge the implications of the model simulations.

we discuss this point in the manuscript: "This set of parameters was based on observations of energy dissipation rates that varied about one order of magnitude from the mean (Lübken et al, 2002). It is noteworthy that the turbulence occurrence rate reaches about 90 % at 88 km and is tightly connected to the mesopause altitude (Rapp and Lübken, 2003). Even in the altitude range of 82 km to 86 km the turbulence oc-

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currence rate is about 20 % to 50 %. We expect that eddy diffusion of less than 0.1 times the mean (i.e. absence of turbulence) is not applicable for NLC growth. Otherwise turbulence must be virually absent throughout the whole time needed to grow NLC particles from a small nucleus till they can be detected by lidar. "

Page 3613, lines 11-13: related to the last point: how constant is the peak altitude in the eddy diffusion profile? Or what is known about it? Your argument is based on a peak altitude of close to 90 km and will not be valid anymore if the peak altitude changes by a few km.

we discuss this point in the manuscript: "It is noteworthy that the turbulence occurrence rate reaches about 90 % at 88 km and is tightly connected to the mesopause altitude (Rapp and Lübken, 2003, their Fig. 7)."

Page 3616: Li reference: can this by updated yet?

We updated reference accordingly.

Pages 3618/3619: Tables 1 and 2: The readability of these tables could be enhanced by adding another column listing which method the lines correspond to (1, 2 or 3).

We updated tables as suggested.

Page 3624, Fig. 5: Most of the lines shown in this Fig. also have symbols (open and solid circles), but as far as I can tell, these are not discussed in the text (Excuse, if I missed something). Do they mean anything? If not, I suggest removing them.

We removed the symbols.

Page 3625, Fig. 6: I suggest showing measures for the variability (or error) of both the LIDAR and the RADAR results (i.e., mean plus-minus 1 sigma).

Fig. 6 shows only a sketch of the altitude dependence of particle sizes. Adding variability might lead to miss-interpretation. We have updated the figure and the caption to clarify this point.

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