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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.

Anonymous Referee #1

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Review of ACP manuscript

"On the microphysical processes of noctilucent clouds (NLC): observations and modelling of mean and width of the particle size-distribution"

by Baumgarten, Fiedler and Rapp

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

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Interactive Discussion

Discussion Paper

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 dependence of width and mean of the PSD are qualitatively reproduced by the model simulations. Sensitivity studies with different eddy diffusion profiles indicate that turbulence 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 H_2O, since these are changed. Therefore, the third process mentioned is directly coupled to T and H_2O and not independent. Again, a really minor point.

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.

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.

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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.

Page 3609, line 12: "particles soundings" -> "particle soundings"

Page 3612, line 25: "agree to" -> "agree with"

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?

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.

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.

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

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).

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.

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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).

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