Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-642-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new Description of Probability Density Distributions of Polar Mesospheric Clouds (PMC)" *by* Uwe Berger et al.

Anonymous Referee #1

Received and published: 13 August 2018

GENERAL SUMMARY AND COMMENTS

This paper presents a new analysis of the statistical behavior of various polar mesospheric cloud (PMC) properties, using observational data from the ALOMAR lidar in Norway. The authors show that the commonly used g-distribution, which prescribes an exponential dependence for the cumulative probability of a given parameter value, is not adequate for the full range of all PMC quantities determined from lidar data. A revised probability density function called the Z-distribution is developed that has two free parameters (scale and shape), and simplifies to the g-distribution when shape = 1. The new function provides a better representation of quantities such as ice mass density and ice radius, and is also shown to enable the creation of "artificial" data from one parameter into a second parameter given Z-distributions of both quantities.

This work is a valuable extension of the original g-distribution concept. However, it is not clear that the proposed applicability to long-term trend studies is justified. Some suggestions and comments related to specific items are provided below.

SPECIFIC COMMENTS

1. p. 2, line 1: Since the monograph cited here may not be easily accessible for many readers, I suggest reproducing the relevant figure (with permission) to help introduce the basic concept of the g-distribution.

2. p. 3, line 8: This choice obscures the inter-annual variability in PMC behavior that has been demonstrated in many previous studies (e.g. Rong et al. [2014], DeLand and Thomas [2015], Fiedler et al. [2017]), which can represent a factor of two variation for the slope of the g-distribution at the latitude of ALOMAR. How does this averaging affect the applicability of the results derived later to any specific PMC season?

3. p. 3, line 10: Is the three-color mode of operation used less often? Other papers discussing ALOMAR lidar measurements talk about 15-minute binning (e.g. Fiedler et al. [2017]), so I would actually expect many more individual profiles to be available from 15 years of data.

4. p. 3, line 31: I'm not sure I understand how the standard deviation can be equal to the mean with a threshold of zero. Statistically, one sigma should not encompass all data smaller than the mean, but with the definition given in line 29, you are not allowing for negative values of x. So if (mean – st.dev.) = 0, where does (mean – 2*sigma) fall?

5. p. 5, line 6: I'm not sure that the term "obviously" is appropriate. The fit line in Figure 2a does go through the data, but the fluctuations between y = 40-90 look comparable in magnitude to those between x = 20-40 in Figure 1a. The rolloff at y < 40 in Figure 2a is more significant to me, and it suggests that using a higher threshold (e.g. 40) would yield a satisfactory fit.

6. p. 5, lines 17-18: Regarding "larger discrepancies", see comment #5.

7. p. 7, lines 3-4: MBS is a first-level measured quantity, whereas IMD, R, and n are derived based on various assumptions. Does this "failure" say something about the functional forms used to create the latter group of products?

8. p. 8, lines 2-3: This figure uses data well below the previously defined fit threshold for both MBS and IMD. Does the result change if MBS > 3, IMD > 20 are required as specified for Figures 1 and 2?. What about IMD > 40, as suggested in comment #5?

9. p. 13, lines 15-16: The first two derived threshold values are close to those given in Section 3.1.2. Is the third value a maximum?

10. p. 13, line 33: This statement seems to connect back to lines 23-24 on this page. Isn't it circular reasoning to say that they agree?

11. p. 14, lines 5-6: This statement is physically plausible for radius. It seems reasonable for IMD which is proportional to rˆ3. Not sure about MBS, because it seems like large density could overcome the dependence on r (but is this true if MBS is proportional to rˆ6?).

12. p. 18, lines 17-19: I'm still not convinced that the parameters derived from a multi-season collection of data are valid to use for this type of "synthetic" data calculation with a smaller subset of original IWC data, based on the previous comments about interannual variations.

13. p. 20, lines 10-13: Is this statement saying that the uncertainty in the retrieval assumptions is large enough to justify the difference in b? What level of agreement would be needed for confidence?

14. p. 20, lines 15-17: This goal would require quantitative answers to comment #13 in order to be able to identify such changes. It also goes back to comment #2 regarding the question of how these fits behave with different individual years of data, and discussing how much noise increases with the reduction in the number of samples.

TYPOGRAPHICAL ERRORS

p. 5, lines 14-15: "unequal" could be "not equal to".

p. 5, line 15: "unequal" could be "not equal to".

p. 8, line 26: "allows to" should be "allows us to".

p. 9, line 9: "particulary" should be "particularly".

p. 14, line 8: "tale" should be "tail".

p. 15, line 7: "Have in mind" could be "Please keep in mind".

p. 16, line 11: "outcome" should be "outcomes".

p. 24, line 12: "stimulus" could be "stimulating".

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-642, 2018.