

Interactive comment on “Using depolarization to quantify ice nucleating particle concentrations: a new method” by Jake Zenker et al.

Anonymous Referee #4

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This manuscript (Using depolarization to quantify ice nucleating particle concentrations: a new method by Zenker et al.) capitalizes on the ability of the CASPOL detection method to capture the depolarization information from particles, droplets and ice particles in the TAMU CFDC and identify them under different operating conditions. The method may be applicable to other systems but each CFDC is unique. The manuscript includes the development of a new empirical analysis method, to quantify ice nucleating particle concentrations and presents a way to deal with especially the data obtained during water droplet breakthrough, which is difficult to interpret. I believe that the manuscript topic does fall into the scope of AMT. Generally, the paper is readable, the analysis is carefully done and discussion points seem to be well supported by data. There is a limitation for this method in that higher concentrations only obtainable

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in a laboratory are applicable; the authors are upfront about this limitation.

There are some major considerations that, if addressed, could strengthen the paper:

The authors may want to consider strengthening the end of their introduction to describe in more details the trajectory of work presented in the paper. Such a road map is limited here and more details could be helpful. In the body, there is little text regarding the comparison but there is a lot of text with many details regarding the development of the empirical analysis, yet these seem equally weighted in the introduction.

In terms of the training data, the text notes that no droplets below $2 \mu\text{m}$ were studied and this is reflected in figure 3. However, figure 6 shows training droplets at $0.7 \mu\text{m}$. This is confusing. Further, since this size is a cut-off point for the analysis, it might be helpful to include smaller particles generation or to explain how the data in figure 6 was observed.

There may be minor scientific issues associated with the depolarization theory (that section of the paper was difficult to follow and there seemed to me to be some confusion or missing information associated with representations of matrices, matrix elements and values and/or units). In particular, the section on page 12 surrounding equations 6-7 is especially confusing. The authors note that these equations deal with the amplitude matrix, but then their inclusion in the equation appears to be an element with only one index. Further, it would be helpful to explain this part of the model further. What do these relationships (eqn 6-7) represent? I see how they combine to create eqn 8 but why?

It would also potentially be helpful for the authors to further discuss the use of the T matrix model for dust (and ice)? A recent technical note (Koepke et al., ACP, 2015, 5947) may be helpful. Generally, the paper would be enhanced with some additional details, clarity or references (and/or possibly even information in the experimental section) associated with the model calculations.

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Overall, there is a lack of consistency within the text and figures where attention to detail would help. This is true, especially with the ordering of the types of particles within the different sections and also within the figures and captions. Further axis labels should include units where possible. A specific example is that in Fig. 6, there are both model and experimental results displayed but the y axis includes the model label and the x axis is missing units. Some additional specifics are included below.

Specific comments:

Pg 9, line 24: e is missing from the

Pg 10, line 11: "both" is unnecessary and confusing

Pg 10, line 19-20: In final copy, watch for placement of the minus sign

Pg 11, line 6-7: placement of training

Pg 11, line 8: based on the figure, the authors mean total backscatter vs depolarization ratio. I'd also suggest reversing the order in the follow up sentence on lines 8-10.

Pg 12, line 7: k is in eqn 3, but omega and t are not present. Is the equation missing time dependence? Also, r is not defined until line 11.

Pg 12, equation 4: related to above, are both matrices and matrix elements included?

Pg 12, missing comma after P_{ij}

Pg 14, line 5: I think you mean Fig. 3a here.

Pg 14, lines 14-17: This is confusing, please clarify. In figure 3b, it seems the % of total population of all particles having a depolarization ratio of ~ 0.2 is close to 100%. How do other ratios exist for the population? This also makes interpretation of values in the text confusing.

Fig 6: Does it make sense to include the error bar information in the caption to make the figure less busy? Or at least remove and caption some of it?

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Pg 17, line 1: typo of added "u". Also here you switch from $><$ notation to larger than and smaller than.

Pg 18, line 12: typo likely

Pg 20, line 4: double check wording for how this figure is introduced and also in the caption to be consistent and correct

Pg 20, line 5: suggest figure or Fig. 7

Fig 8: Caption could be improved, especially repetition in description of panel c.

Pg 20, line 26, suggest: In 2 out of 3 cases shown. Alternatively, you may want to clearly state (as you do later) that 27 cases/periods were evaluated (see Fig 9).

Pg 21, line 7: center panel of Fig. 8c?

Pg 21, line 12: is data missing from fig 9 or is it just hard to see?

Fig 10: Which axis contains the data for the new method? I suspect the x, but am unsure due to confusion noted above. Please clarify and update axes. Would it make sense to fit this data to observe if there is a small bias in the new data?

Pg 22, paragraph beginning on line 5: I am confused about how the errors in two regions can be 500 and 50% but overall it's 32%. I believe this is averaged values for each region considered. Is this the best way to present the uncertainty? Also as a minor detail, spacing when reporting numbers is inconsistent here and somewhat throughout the document, which would probably be fixed upon typesetting.

Figure 11: Consistency with previous figures and also double check captioning.

Pg 23, line 9: Does Fig 11b warrant more of a discussion? Can a literature comparison be included?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-166, 2017.

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