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Atmospheric Measurement Techniques Discussions

Interactive comment on "A Method for Computing the Three-Dimensional Radial Distribution Function of Cloud Particles from Holographic Images" *by* Michael L. Larsen and Raymond A. Shaw

Anonymous Referee #3

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General comments

The authors present a novel method for calculating the radial distribution function (rdf), a quantitative descriptor of clustering of particles in a volume. Among other applications, the rdf is used in the cloud microphysics community to inform process rates (i.e. as one component of turbulent collision-coalescence kernels) and diagnose mixing state. Other methods for calculating the rdf either assume an infinite domain (i.e. in direct numerical simulations of cloud drops with periodic boundary conditions) or rely on questionable assumptions to derive the rdf from effectively 1-dimensional *in*

situ cloud probe observations. Given that one of the authors has been a key player in the development of a probe that can measure a truly 3d cloud volume, it makes sense that the derivation of the rdf should be revisited. That said, I am not convinced that this study will find broad application beyond the small community of digital holography observationalists.

The derivation of the effective volume rdf is a rather intuitive solution. While the algorithm for computing it may be "inelegant" (authors' description), barring the development of new instrumentation with cavernous sampling volume, complex geometry and/or extremely fine pixel resolution, the brute force method of computing volume normalization factors seems sufficient. It is unfortunate that only 8 holograms were analyzed, and I would ask that the authors comment on whether there exists a threshold concentration below which *g(r)* cannot be accurately computed. Holograms with lower concentration will likely increase the "noise floor" length scale where *g(r)* begins to deviate from unity but increasing the number of samples may offset this effect. If there does exist a low concentration limit, does that imply that this analysis is only suitable for a small subset of relatively high drop concentration environments? I can envision clustering properties having some dependence on concentration, especially in light of other work using the Π Chamber (e.g. Chandrakar *et al.*, 2016; Desai *et al.*, 2018). Finally, it is unclear to me how application of this analysis to *in situ* measurements furthers the development of microphysical parameterizations in the absence of collocated measurements of turbulence intensity or supersaturation state, and I invite the authors to expand on their vision for how this may be accomplished. These concerns are all relatively minor though, and I recommend this study for publication in AMT.

Specific comments

Comments are given as "page X, line/figure Y" and are listed in order from beginning to end of the manuscript.

- P. 6, Fig. 1 caption: Incorporate the narrative portion of this caption into the main text. The information given here is important, and reading it from the figure caption makes it more difficult to connect it with what's happening in the text (i.e. 4th paragraph of section 3.3).

- P. 9, Fig. 3: It's difficult to differentiate the blue and black markers unless the figure is magnified. Consider choosing more strongly contrasting colors or using different markers. Also, I assume "a. u." stands for arbitrary units but it took me a while to figure this out – please define this in the caption.
- P. 10, L. 18-19: Is there any study you can cite re: fragmentation near the optical windows? Is this a problem for liquid, ice, or all particles?
- P. 11, L. 9-10: I agree that a quantitative comparison with the small sample here would not be appropriate, but is it worth adding a theoretical Matérn process to Fig. 6 for qualitative comparison with the mean curve? Disregard this comment if adding a theoretical curve distracts from the point you're trying to make.
- P. 11, L. 16: Is the issue that there are large uncertainties in 1d *in situ* results, or rather that it's unclear from a theoretical perspective whether they are extensible to 3 dimensions given the pile of underlying assumptions? I fully agree with the first full sentence of P. 12 that measuring the 3d rdf is highly desirable, but I don't think uncertainty should be the focus unless you can quantify how much it is reduced by increasing dimensionality. As I understand, the point is that your method requires fewer assumptions be made and there is greater consistency between measurement and application of the rdf.
- P. 12, Fig. 5 caption: Same as Fig. 1 caption - you are using the caption to communicate information that belongs in the main text.

Technical corrections

- P. 1, L. 12: Is the bold-faced "**?**" next to Onishi et al. (2015) a missing reference?
- P. 2, L. 28 & 30: Extra set of parentheses surrounding reference list
- P. 3, L. 1: Extra right parenthesis at end of sentence.
- P. 4, L. 6: "...in the measurement volume. *N* is the..." – comma instead of a period.
- P. 4, L. 14: "...and $N_{ex}(r_\circ)$ are the number..." – should be "is" instead of "are"
- P. 11, Fig. 4 caption: "The volume simulated here corresponds from..." – should be "corresponds to"
- P. 14, L. 11: "calculate the volume of the *n*-dimensional sphere of the shell" – I'm confused by this, did you mean to say "sphere of the shell?"
- P. 14, L. 19: Is there a factor of $1/N$ missing from the equation or is it just a straight sum?

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