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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Statistical Analysis of Ice Microphysical Properties in Tropical Mesoscale Convective Systems Derived from Cloud Radar and In-Situ Microphysical Observations" *by* Emmanuel Fontaine et al.

Wiebke Frey (Referee)

frey@tropos.de

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General comments

The manuscript by E. Fontaine et al. describes the combination of cloud radar and in situ cloud particle measurements of mesoscale convective systems as performed during four campaigns at different tropical locations. The authors perform a statistical analysis of ice particle properties in dependence of temperature (as altitude) and radar reflectivity class. They identify cloud particle parameters that are similar for the MCSs

in all four locations, namely IWC, visible extinction, ice particle number concentration, and second and third moment of the size distributions. From their results, they derive two parameterisations, one for visible extinction as a function of temperature and IWC, and one for rescaled size distributions as function of temperature and IWC which could be used in modelling efforts.

I really like the idea to combine measurements of several field campaigns. However I would love to see more discussion of the differences in meteorological/dynamical, ... conditions of those campaigns to give context why/why not ice particle properties are similar or not. Basically such a discussion will outline difficulties of merging such data sets and generalising results from each individual campaign. See my major comment on meteorological conditions. When shortcomings of merging such data sets are identified, combining such data sets yields the potential for generalising findings in order to improve model representation of clouds (in a global view) and are therefore an important contribution.

I agree with the review by Darrel Baumgardner and give further ideas for improvement below:

Major comments

Meteorological conditions:
You mention and compare data from four different field campaigns. However, you only very briefly mention the differences in conditions of these campaigns. I think it would be useful to know more about what distinguishes the data sets (see comment above). Like major differences in meteorological or dynamical conditions, land/sea convection or orographical effects, monsoon or other special season, development stages of the MCS during the aircraft observations (developing, mature, decaying), microphysical characteristics as particle shapes which you would get from the optical array probes, or any other conditions that could lead to specific characteristics of the respective data sets.

Naming convention:
The merged data set from the four campaigns is sometimes named differently in the manuscript and figures (tropical, global, global tropical...). Please keep it to one name! I would suggest not to use 'global' as it only contains tropical campaigns. As it is not clear whether different locations in the tropics have a significant influence on the data, I would suggest simply using 'merged' (or 'combined') data set, that makes it clear that not data from all over the tropics are used (as might be possible in satellite data studies for example).

Radar reflectivity zones:
How were the thresholds for the 8 zones chosen? How do you motivate the thresholds? Is it possible to interpret each zone in respect to a certain MCS development stage, or do they distinguish in some other MCS characteristic?
In some cases it seems that particularly the lower classes do not really differ from each other (e.g. page 6, line 2/3 or page 25, line 32/33). What is the reason to keep separate zones and not combining them into one?

Paper length:
At the beginning of section 5 you introduce the general outline of Figures 5, 6, and 8 - 16, which I found a very good idea to keep the description of each figure short. However, Section 5 still is too long! I would suggest to closely check from which figures/subsections you draw major conclusions and move those that only bear minor conclusions into a supplement and mention the investigation of the respective parameter in 1-2 sentences in the main manuscript. This will keep the manuscript more focused and the reader's attention. I will give some suggestions below, but you might want to identify other sections that could be moved yourself.

Discussion of results:
The discussion would benefit from relating your results to results of former studies, do your findings agree/disagree with what others have found? If your results are completely new, than it should be pointed out more clearly! Also, please point out more

clearly what your parameterisations are useful for and how the scientific community benefits from your work.

Specific comments

page 2, line 4-5: "An accurate estimation of the spatiotemporal distribution of the Ice Water Content (IWC) is a key 5 parameter for evaluating and improving numerical weather prediction (Stephens et al., 2002)."
Does this statement make sense in the light of your manuscript where you use IWC as input for your parameterisation and not obtain it as output?

page 3, line 2: better say: This study uses a data set where MCSs were observed in four different locations in the tropics and related to two different projects:

page 3, line 19: IKP-2 - please introduce abbreviation.

page 3, line 22: Are you really giving D_max in cm?

page 3, line 31: Please introduce the radar reflectivity factor Z as it is an important parameter in your study and reader less familiar with radar measurements might not properly know it.

page 3, line 35: "The processing holds particularly for both data sets of the HAIC-HIWC project. "
I don't quite understand what you want to say with this sentence? Please rephrase.

page 4, line 2/3: mean profiles of Z:
Did you perform some kind of weighting when combining the data sets from the four campaigns for the number of data points that go into the mean? If not the results/means will be shifted towards the HAIC-HIWC campaigns, which have significantly more data points. This should be discussed.

page 5, line 26: The usage of ECMWF reanalysis temperatures: Due to the much coarser resolution of the ECMWF data (compared to aircraft point measurements),

what implications does it have on the uncertainties of your results?

page 7, line 20f: U(X)/X - Is this parameter denoted by the grey shading in the subfigures b-e? Then mention it here as well.

page 11, line 1-5: You are concluding that "... σ in tropical MCS tend to be similar for all MCS locations in the same range of T and for corresponding MCS reflectivity zones, ..." but your parametrisation is only a function of T and IWC, but not dependent on reflectivity zone? How does that fit? In that respect I was missing a figure showing measured values alongside the parametrisation (not only the relative errors as in Fig. 7). It could also help to explain more how you arrive at this parameterisation.

page 12, line 22: "... identical image data processing to remove shattering artefacts..." which method exactly do you use? This might be quite important for the resulting data. For example, when you use the interarrival time method from Field et al., 2006, using the same time threshold for all data sets might lead to errors. The best threshold might even vary from one flight to the next in the same campaign (at least in my experience, see e.g. Frey et al., 2011, where interarrival times have been adapted for each single flight). Thus, using only one threshold for all four campaigns might lead to removal of 'good' images in one case and incomplete removal of 'bad' images in another case.

page 12/13, Section 5.3 until page 13, line 20: Since the conclusions from Figure 8 and 9 are similar, and due to the uncertainties related to the small particles, I think Fig. 8 and the corresponding text would be a good candidate to move to a supplement and only briefly mention here. See main comment about the length of the manuscript above.

page 14, line 30: "in the decaying parts"
I think here is the first time where you mention these MCS to be decaying. Is it true for the whole of the Maldives measurements, or only for parts of the measurements with the small particles (or only for specific Z)? This relates to my main comment about the meteorological differences between the MCSs in the different campaigns.

page 15, line 29/30: What are e_p and e_s?

page 16, line 10: "merged with subsequently sampled rather small local convective systems"
Since you focus your study on MCS, can these rather small convective systems still be classified as MCS? Otherwise, should they not be removed from the data set?

page 15-17/Section 5.4:
Maybe this section could also go into the supplement (no major conclusion drawn)? and only briefly mention in main manuscript.

page 17/18, section 5.5:
Also this section could be moved to a supplement?

page 18, line 22: "Commonly, number PSD of ice hydrometeors are modeled with Gamma distributions."
Maybe give one or two example references.

page 22, line 22: How would M_3 derived from parameterised M_2 according to Eq. (18+13) look lie? Why don't you show this as well? Above you make an improvement to the M_2 parameterisation, so why do you use the presumably worse parameterisation for deriving M_3 here now? Which would also presumably lead to a worse M_3 retrieval?

page 23, line 14/15: "blue solid lines represent median relative error when estimated M3 is calculated from parametrized M2 from Eq. (18+13) and Eq. (19)."
This is not, what the legend in Figure 17 says?

page 23, lines 19-27: Maybe show the figures in a supplement?

page 24, line 20: "extrapolates PSD" - I think you mean measured PSD (I strongly suggest to never extrapolate any PSD!). Please rephrase!

page 25, line 32/33: Is there a difference between zones 1-5 or would it actually suffice to combine them into one reflectivity zone? (related to my main comment on radar

reflectivity zones)

page 26, lines 14-20: Would it be possible to make a more general point here: Possibly that aggregation process efficiency is higher for convection over land than over islands and higher over islands close to large land masses than over islands in the middle of an ocean? Are there other studies you could relate to? Or is it the convective system size (larger in Niamey...)?

Technical corrections

page 2, line 34: “predictions fails” - predictions fail

page 3, line 18: an ’and’ missing: “..., and the cloud radar RASTA...”

page 4, line 14: Should be 4, not 5 airborne campaigns.

page 7, line 11, and page 8 line1: “(Figures 5, 6, 8, 9, 10, 11, 12, 13, 14, 15 and 16)” better (Figures 5, 6, and 8 - 16)

page 8, line 17: “Globally”- do you mean ’generally’?

page 10, line 14: start with lower case ’with’ after equation.

page 18, line 15: “different from one MCS type of MCS to another” remove ’of MCS’.

page 26, line 5: “for individual the MCS locations),”
remove ’the’ and ’)’.

Figures and Tables

Figure 1: This plot is very busy. Maybe it would be easier to read if you split the figure into four subfigures (one for each campaign) with the black lines for the merged data set in each of the subfigures?

Figure 2: Colours: Generally, it is advised to choose colourblind-friendly colour schemes, and the rainbow scheme is unfortunately not one of those (among other shortcomings of this colour table, see e.g. the open letter to the scientific community

here: https://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/). In subfigure b, I find it hard to distinguish between MCS zone 2 and 3, thus a different colouring would help here, too.
The axis labels (numbers) are too small, please enlarge.

Figures 5, 6, and 8 - 16:
I don’t think it is important to name the campaigns each time, it suffices to mention the locations (also in figure captions and text).
The subfigures are rather small, maybe you could move the legends to the top or bottom of the figure to allow more space for the subfigures?
Caption line 1/2: Maybe better: ... for the different MCS reflectivity zones using the results from the four locations. (removing the second sentence)

Figure 7: You say IWC in the caption but the figure label say CWC. Same in Figure 17.

Figure 14: The caption says “for M_2 per for unity dimension.”, the axis annotation says: “M_2 $[m^{-1}]$, so not unity dimension. Please clarify.

Figure 18: See comment about colours above.

Table 2: The table is extremely hard to read, unfortunately! Maybe it could help to swap the ’with respect to median of’ column with a ’radar reflectivity zone’ column, and give the parameters in front of the temperature brackets.

Tables in Appendix: I assume that the decimal point should actually be a point and not a comma - as in all your appendix tables?

References

Field, P. R., Heymsfield, A. J., and A. Bansemer: Shattering and Particle Interarrival Times Measured by Optical Array Probes in Ice Clouds, Journal of Atmospheric and Oceanic Technology, 23, 1357-1371, 2006

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