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Interactive Comment

Interactive comment on "Constraining mass–diameter relations from hydrometeor images and cloud radar reflectivities in tropical continental and oceanic convective anvils" *by* E. Fontaine et al.

Anonymous Referee #1

Received and published: 7 March 2014

This manuscript presents a rather technical study about the mass-diameter relationship of cloud particles. The two unknown coefficients of this power law relationship are derived from aircraft measurements by in situ cloud particle probes and a radar. It is found that the coefficients have a large variability and generally decrease with increasing altitude in the altitude range considered here.

The data for this study were taken from the Megha-Tropiques campaigns which allow the authors to separate between continental (West Africa) and maritime (Indian

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Ocean) clouds. Neither in the introduction nor in the discussion the authors mention previous studies that have pointed out differences in continental and maritime clouds (e.g. Cetrone and Houze, 2009).

The introductions states that the overall purpose of this study is to improve the surface rain rate retrieval calculated with the BRAIN algorithm from satellite measurements. How this shall be accomplished is not mentioned anywhere in the manuscript. Having said that, it does not become clear if the study aims at more than presenting a retrieval algorithm for the coefficients of the mass-diameter relationship. Since, as this work shows, the retrieved coefficients are highly variable and also differ between the different clouds (here maritime and continental) one cannot readily use the coefficients found in this study for deriving cloud water content (CWC) from other measurements.

Critical points:
ice/water:
For your calculations you assume that all cloud particles are ice. Did you confirm this? Or can you prove that it is a valid assumption, since measurements were clearly taken at temperatures where clouds could be mixed-phase. Assuming ice when it is water would result in an error in the calculation of the CWC due to the different density.

Measurement uncertainties:
The instrumentation is described briefly, which is generally ok. However, what is missing is the important description of measurement errors and uncertainties. Every single one of them will propagate into the retrieval of the coefficients of the m(D) power law relationship and thus into the retrieved CWC. Therefore, a detailed discussion about measurement uncertainties and how they affect the retrieval is inevitable.
In detail:
What are the error margins of RASTA? It has huge error bars in Figure 11. What errors occur in the measurements of the cloud particle instruments, e.g. regarding number concentrations and sizes? Under the conditions where the measurements were taken, a high amount of shattering can be expected. A more detailed discussion in this regard

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would be highly desirable. How much data had to be removed due to shattering (is there a correction for the loss of data?), and thus, how does that propagate into the CWC retrieval? Can you demonstrate that the methods you use to identify and remove shattering do suffice? And please also give a short description of these methods, do not only cite the corresponding articles (so the reader has to look into those articles to find out what the correction does).

All these errors will add to the overall accuracy of the retrieval of the two coefficients and thus to the derived mass.

Full use of dataset:
In the first instance you use 2DS and PIP measurements to derive α and β. However, from Section 4 on, you only use the 2DS measurements for calculating the surface diameter relationship and use this to derive β. You correctly say that it is better to use 2DS images for submillimetric particles, but for the larger crystals you will still gain shape information from the PIP images as well. So, why not using 2DS images for the smaller particles and PIP images for the larger particles? In the following step for retrieving α, you use again the combined measurements. Wouldn't this be a source of error? What would the difference be between β_σ when only using 2DS images and β_σ when using images from both instruments? Furthermore, how do you derive γ (equation 10)? Don't you also need γ to derive σ?

Besides these weaknesses, this study has a rather technical character, mainly describing a new technique. Therefore, I would find it more appropriate for publication in a more technical journal as e.g. Atmospheric Measurement Techniques, since there also is comparatively small scientific outcome. Citing from the ACP homepage "The journal scope is focused on studies with general implications for atmospheric science rather than investigations that are primarily of local or technical interest." I have doubts that this paper fulfils the requirements of ACP.
While I do see the importance of improved retrievals for cloud water content, I can't

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see how the results here are transferable to future studies, if not the same instrumentation is used. Thus, the significance of this study to the broader scientific community remains somewhat unclear. In conclusion, I recommend rejection of this manuscript for ACP, but resubmission of a revised manuscript to a technical journal.

Specific comments

Page 2984 line 20: "concentrations of the hydrometeors increase with altitude" Please mention to what altitude layer you are referring, other studies have shown the opposite behaviour when looking at higher altitudes. Thus, it is an important additional information.

Page 2988 line 15-22: Please indicate the size range covered by the instruments.

Page 2990 line 7: Does it make sense to specify a bin width of 10 microns also in the size range where measurements are purely taken by the PIP? Have you considered an increasing bin width with increasing particle size?

Page 2992 line 13/14: 2gm^{-3} of spread in CWC sounds very much. How much is it percentage-wise?

Page 2993 line 8-10: What is a typical measurement error for RASTA (besides the mentioned calibration error)? How high are uncertainties in the CWC retrieval if RASTA uncertainties are taken into account?

Page 2999 line 2/3: While you only use two flights for the analysis for MT2011, I wonder why you don't also use the other two flights stated in Table 1 as a third class – oceanic isolated convective system?

Page 2999 line 14: I am sceptical if you can see a decrease of β_σ with altitude. I would say it is fairly constant, also taking the error bars into account.

Page 2999 line 19: Houze 2004 would be a good reference here.

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Page 2999 line 24: Are the clouds in SH2010 continental or maritime?

Page 3000 line 2: You refer to Figure 12d, I assume you mean Figure 11d?

Page 3000 line 7-14: I cannot see a general decrease of CWC with altitude for the MT2010 case. I can rather see an increase in the lower levels and than a more or less constant behaviour. A decrease is only visible if you look solely at the uppermost three points.

Page 3001 l 5/6: "..., while this decrease is less pronounced for MT2011" - I can hardly see a decrease!

Page 3001 line 7/8: "This observational result may be due to low number of samples available in the high altitude during MT2011." From Figure 11 I still read about 30 samples here (at minimum at 240K). I recommend deleting this sentence.

Page 3001 line 22-24: For the decrease in the uppermost part (<245K), are you comparing more than two temperature bins? I would leave this sentence out.

Page 3002 line 4-6 (and following part): As you mention, a good correlation between CWC and radar reflectivity is no surprise since you use the reflectivity to derive CWC. So, of what use is this correlation then? Why are you doing it?

Page 3004 line 27: As mentioned above, I am not convinced that β decreases in the MT2011 case.

Page 3005: In the discussion about differences between continental and oceanic convective systems, I would appreciate some references to previous studies that show differences in those clouds. E.g. Cetrone and Houze, 2009, Frey et al., 2011.

Page 3006 line 2/3: I think that there are also aggregates visible in the images from MT2011.

Page 3007/3008: "...and in the fourth L is constant and equal to 16 pixels. L has been chosen out of the size range of [10;100] pixels with 1000 simulations for columns in

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each of the four cases.” I think one of the “L”s should be a “H”.

Figure 1:
The line from PIP measurements shows particles smaller 100μm, while that is the smallest size detectable for the PIP?! In Fig 1b, the PIP distribution starts at about 250μm, why is there such a difference in the PIP size range between the distributions in Fig.1a and Fig.1b?
How can the composite distribution differ from the 2DS distribution at sizes around 90μm?
While you mention a general good agreement between the two probes, I find the discrepancy in the overlap region at around 100μm and at around 1mm not negligible. Can you comment on these?

Figure 2:
The caption says that you show the effective reflectivity Z_e, while the graph shows Q_{back}. What is corect?

Figure 6:
You may want to consider grouping these images according to their habit classes (and specify these on the plot).

Figure 7:
The blue contours around the blue symbols are not recognisable. Changing the colours of the dark blue symbols would be desirable.

Figure 8:
Please add the subscript σ on β and α (Fig 8c and d), the caption of Fig 8e says that CWC (black line) is deduced from β_σ and α_σ while the annotation in the Figure suggests it’s the average CWC deduced from β_i and α_i. What is correct? Why are there gaps in the black line?

Figure 11:

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You mention in the text that data points around the melting layer have to be treated with care. Please indicate the melting layer on the plot (e.g. with a shading).
Why are there no error bars for the total number concentration? Please add.

Figure 12g-h:
You write equivalent reflectivity in the caption, in the Figure it says total backscatter coefficient (Q_{back}), what is correct?

Figures 11 and 15:
The choice of colours in these figures is unfavourable for colour blind people. You may want to consider another colour pair.

References:
Cetrone, J. and Houze, R. A.: Anvil clouds of tropical mesoscale convective systems in monsoon regions, Q. J. Roy. Meteorol. Soc., 135, 305–317, 2009.

Frey, W., Borrmann, S., Kunkel, D., Weigel, R., de Reus, M., Schlager, H., Roiger, A., Voigt, C., Hoor, P., Curtius, J., Krämer, M., Schiller, C., Volk, C. M., Homan, C. D., Fierli, F., Di Donfrancesco, G., Ulanovsky, A., Ravegnani, F., Sitnikov, N. M., Viciani, S., D'Amato, F., Shur, G. N., Belyaev, G. V., Law, K. S., and Cairo, F.: In situ measurements of tropical cloud properties in the West African Monsoon: upper tropospheric ice clouds, Mesoscale Convective System outflow, and subvisual cirrus, Atmos. Chem. Phys., 11, 5569-5590, doi:10.5194/acp-11-5569-2011, 2011.

Houze, R. A.: Mesoscale convective systems, Rev. Geophys., 42, RG4003, doi:10.1029/2004RG000150, 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2983, 2014.

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