Review on "*Aerosols, Clouds, and Precipitation in the North-Atlantic Trades Observed During the Barbados Aerosol Cloud Experiment. Part I: Distributions and Variability*" by Jung et at.

General Comment:

This paper presents a nice overview of „aerosol, cloud, and precipitation features" as measured over Barbados. Airborne data has been sampled in-situ combined with a cloud radar during 12 research flight. My overall opinion is that this paper has the clear motivation to characterize the observations without going into much detail of individual findings. I have no concerns about this strategy, in particular because the observation period nicely covers the three typical aerosol types typically observed at Barbados. However, at some points (see specific comments) a somewhat deeper analysis and discussion of individual findings would improve the paper instead of refering to another promised upcoming paper.

However, I have a few major critical points which have to be seriously discussed before I can suggest this paper for publication.

1.) **Page 5, beginning of sec 2.5:** I have serious doubts about this method of simply combining two size distributions with one of it is sampled under ambeient and the other under dry conditions. The two size distributions may line up well but this could be really by chance! A careful analysis of this issue is absolute necessary. A lot of bigger aerosol might be sea salt particles which are highly hygroscopy and there should be a big difference if you measure it under dry or ambient conditions. Please include at least error bars which describe this effect! This is a serious point which needs more careful discussion.

2.) **Page 9, Sec 3.3.2:** Do I correctly understand? The PSD is averaged over all flight height for one individual flight - including sub-cloud layer and cloud layer heights? Does this make sense? Why not at least distinguish between sub-cloud layer and cloud layer aerosol? I have serious doubts about the representativiness of these size distributions. In particular for the situation where dust is advected the size distribution should be a strong function of height.

   Furthermore, all size distributions show more or less exactly the same general structure/shape with characteristic peaks and shoulder at the same size. It looks like that the distributions differ only due to dilution although you mentioed that you had three different types of aerosol loading? Although the y-axis is logarithmic this seems quite strange. I discussed this issue with an expert for OPC measurements and we both wonder if this could be an instrumet artefact. Please discuss this issue in detail.

3.) **Page 16 (Summary):** This section is quite often a word-by-word summarize of the previous sections. The discussion is missed out completely. I totally understand that for a data overview paper a discussion is not a trivial task but repeating all the obersations word by word is not a convincing solution.

Specific Comments:

Page 2, line 3ff: How are your findings biased by the sampling strategy - is it natural to assume that one tries to hit the bigger clouds which bias the frequency of observed cloud types?

P2, l 15ff: One might mention at least a few open questions. "Cloud have to better understood" is a quite generic statement.

P2, l 17: Don't forget Malkus' landmark papers here; although these experiments were smaller three nice papers came out o fit.

P2, l 31: Why is Barbados a perfect place for such studies?- Be more specific here.

P 4, l 6: "PVM-100 water content" is not a parameter - please use liquid water content instead.

P4, l 8: Provide location of the company, use „Inc“ instead „inc“.

P4, l 10ff: You should mention at this point which instrument measures under environmental conditions and which one is a closed system which samples under dried conditions - it's important here!

P4, title of sec 2.3: Better "stratification"? Large-scale would also imply a horizontal component - right?

P4, l 26: Better characterized/analysed instead of „defined“ ?

P5, l 10ff: I have serious doubts about this point: The two size distributions may line up well but this could be really by chance! A careful analysis of this issue is absolute necessary. A lot of bigger aerosol might be sea salt particles which are highly hygroscopy and there should be a big difference if you measure it under dry or ambient conditions. Please include at least error bars which describe this effect! This is a serious point which needs more careful discussion (see general comment).

P 6, l 6: You could include the equation (in text) for the LCL determination but not realy necessary

P 6, l 33: Is there a difference/bias between the radiosondes and TO-profiles for determing the inversion? You mentioned earlier the method how to determine the main inversion and here you say that the inversion was poorly defined? Please specify!

P8 , l22: In the plot you abbreviate super-saturation with "ss", here with „s“ - be consistent.

P9, l 15: Do I correctly understand? The PSD is averaged over all flight heights for one individual flight - including sub-cloud layer and cloud layer heights? Does this averaging makes sense? Why not at least distinguish between sub-cloud layer and cloud layer aerosol? I have serious doubts about the representativiness of these size distributions. In particular for the situation where dust is advected the size distribution should be a function of height.

Furthermore, all size distributions show more or less exactly the same general structure with characteristic peaks and shoulder at the same size. It looks like that the distributions differ only due to dilution although you mentioed that you had three different types of aerosol loading? Although the y-axis is logarithmic this seems quite strange. I discussed this issue with an expert for OPC and we both wonder if this could be an instrumet artefact. This pont has to be discussed in detail (see general point).

P9, l 18: I do not agree with the statement that PSDs avageraged over sub-cloud and cloud

layer can provide any insight in processes. You average over regions where different processes are dominant. I suggest re-wording.

P 9, l 32: See my comment about combining an aerosol size dristribution measured with a closed system (dried aerosol) and an open syestem (ambient conditions).

P 10, l 27: I don't understand exactly what you mean with "precipitating clouds exhibit more organized mesoscale features"? Can you please specify!

P 11, l 6: About LWC measureemnts with the PVM-100A; it is known that the transfer function oft he PVM-100A decreases with increasing droplet size (see Wendisch, Garrett, and Strapp: 2002). The droplets in trade wind cumuli have comparable big droplets due to the low number concentration and this underestimation of the PVM might be an issue. Although you might not have the droplet sizes available you should at least mention this issue because it could partly result in the bigger discrepancy bebetween measured and adiabatic value around cloud top region. Of course you could take the effective radius derived from the PVM-100A and estimate the transfer function based on this parameter – but maybe mentioning this problem is also fine.

P 11, l 9: I don't completely understand this criterium, does it really completely rule out the possibility of counting shattered droplets? Please explain in more detail.

Furthermore, a few line below you mention CARRIBA_dry: In the "polluted" case of CARRIBA_dry (April 24th) the aerosol number concentration in the SCL was about 265 particles per cm^3 resulting in mean droplet number concentration of 80 droplets per cm^3 (measured with a PDI). In your plots you have aerosol number concentrations of 100 to 300 per cm3 in the SCL (see Fig 2c) (assuming air density about 1 kg/m^3 in the SCL) but resulting in 100 to 200 droplets per cm^3 between z = 500 and 1000 m) in Fig 10b. This means that about 75% oft he aerosol particle will be activated? Furthermore, your profile of N_d is at least slightly increasing with height! It is not very typical that droplet number concentration is increasing with height although processes such as secondary activation might play a role for a few cases. Please discuss this point and offer an explanation for this behaviour.

On a first glance this looks like an inconsistency to me and should be discussed in much more detail. For example, you could show a profile of N_droplets for one flight which can be directly compared to the aerosol profile of the SCL? Then you can estimate how many aerosol particles are really activated. Your data suggest that more than 70% (or so) of the aeroosl as measured in the SCL will be activated

P 11, l 9: Again, the parameter is "LWC" and not "PVM-100 LWC", by the way, in the plots the liquid water mixing ratio plotted and not the LWC.

P 11, l 10: Wasn't there a Phase Doppler Interferometer (PICT) installed? If yes and if it worked properly, you should have a reliable estimate of the droplet number concentration (and size distribution).

P 11, l 15: I don't understand the "comparison" with the CARRIBA_dry campaign in this context. The three differernt types of aerosol origin were also observed during CARRIBA.

End of p 11: I am not an expert in radar meteorology but a liitle bit more detailed discussion would be appreciated. What can I learn from the radar measurements?

P 12, l 6: From reading this part of the text it is difficult to understand the differences of the four patterns in Fig 12. – in particular between panel b) and c). How is "strongest precipitationd" defined and why do you exclude a few precipitating clouds in panel d) ? – On a first glance this seems to be a little bit arbitrary and the motivation for doing this remains unclear. A few more word would help the reader.

P 12, l 22: Have you really excluded three precipitating clouds or three days with precip?

P12, l 24: I am confused here: a few lines above you claimed that clouds deeper 500 to 600 m have a significant chance of precipitating and now you analyze the non-precipitating clouds in panel c) and d) with a thickness of about 1300 m? isn't this contradictionary or do I misunderstood something? Please specify!

P 13, l 10: One should also mention in the text thtat the photo in Fig 15 is not from BACEX

P 13, l 15: The following part is more discussion and not data analysis. However, I do not see how this discussion is justified by the observed data and its analysis. This part is a little bit confusing to me and should be re-worded.

P 13, l 27: Although there is a strong collaboration between the CARRIBA community and the activity ot the MPI group, the work of L Nuijens et al. should not be considered as part of CARRIBA.

P 14, l 1: I think the frequency of precip should be mentioned earlier - it is a quite fundamental point and comes surprisingly late in this section.

P 14, l 4: How is the precipitation LWC defined here?

About „Summary and Discussion" Overall comment: This section is really mostly a word-by-word summarize of the previous observations. The discussion part is missed out completely. I totally understand that for a data overview paper a discussion is not a trivial task but repeating all the obersations word by word is not a convincing solution and I suggest a complete re-wording of this section.

P 14, l 32: It is a little bit strange to talk about hurricane seasons for Barbados because Barbados wasn't influenced by a hurricane for several decades (before your flights, the first hurricane „Tomas" came in just a few month later...

P 15, l 2ff: This sentence is quite confusing because one can interpret it such as the maximum of N_a is below the inversion which of course makes no sense if it is steadily decreasing with height - I suggest re-wording to make it clear what you mean.

P 15, l 12: Just a comment but there are very contradicionary studies about a possible influence of GCCN on the development of precipitation - could be at least mentioned here.

P 15, l 16: What do you mean with "could provide" I think at this point a deeper data analysis woud be interesting.

P 15, l 24: I think a little bit more quantitative analysis would be justified at this point! That cumulus clouds are far away from being adiabatic is well known.

Figures:

Fig 1: to include the units in brackets is confusing because one would expect the paramater which was mentioned before - the potential temperature is "\Theta" and not "K" – just a formal point.

Fig 1: Although I would do it in the same way: is there a good argument for taking the readings in the height range between 100 and 200 m? I assume the radiosonde takes some time to provide reliable readings? If you would take data from the TO you could take the 30m level legs - right?

Fig 2: Although it is a nice overview details of the profiles are difficult to detect - this is particular true for \Theta and q_l.

Fig 2c: The unit „particles per mg“ - a kind of aerosol number mixing ratio - is not very common (although it makes sense...). Usually I would expect particles per cm^3 normalized to standard conditions - please specify in the text what you did here.

Fig 2a: In my pdf copy the "\Theta" does not show up.

Fig 2 caption: Why do you use sometimes symbols, sometimes not? I suggest to use symbols in the xlabels of the plot and in the caption I suggest using "water vapor mixing ratio q_l (g/kg)" and being consistent with this notation throughout the paper.

Fig 3: You could consider plotting the 1-sigma as classical error bars or at least dotted lines. First I was confused because it looks like another data set but you have described it in the figure caption so everything is technicaly correct.

Fig 5: Difficult to figure out the absolute values.

Fig 5, ylabel: Is here a "micron" missing? it does not show up in my pdf copy? Please check all figures for this problem.

Fig 9: The red dashed lines should be a little bit thicker!

Fig 10: From this profiles it looks like that cloud base is 250 m below LCL ? and around cloud base you have super-adiabatic q_l?

I don't understand the calculation of adiabatic q_l; is it estimated for each cloud core individually? If so, why do you have super adiabatic q_l between mean LCL and minimum LCL? Why not normalizing each cloud with its adiabatic value or showing the difference between actual q_l and adiabatic value for each cloud?