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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The fine-scale structure of the trade wind cumuli over Barbados – an introduction to the CARRIBA project" *by* H. Siebert et al.

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Response to the review by Patric Chuang:

We thank Patric Chuang for his valuable comments about our manuscript. Partly his overall comment is quite similar to the main concern of the second reviewer; a detailed response is given below to each point. In the following, the reviewer comments are italic and the responses are regular.

This manuscript is an introduction and overview of the CARRIBA project, which centers on airborne measurements in a trade wind cumulus region off Barbados during

two month-long deployments. The overall goals of the project involve the interactions among aerosol, clouds, turbulence and radiation, and this paper is mainly organized along these four themes. Like any field program overview paper, it's difficult to balance describing interesting results, often of a preliminary nature, with the need to document the basic meteorological and other characteristics during the project time period. In my opinion, the primary purpose of the paper is the latter, with just enough of the former mixed into it to make the paper seem worthy of scientific publication.

My main over-arching comment is that I would like to actually see less science and more documentation. The science that is contained in here is certainly interesting, but because it covers select topics throughout all four themes, each scientific point is either rather modest in scope or just the start of something quite interesting. I don't think any of the scientific analyses and conclusions contained in here will be the reason future papers reference this one.

We follow the reviewer's point of view but together with the main concern of the anonymous reviewer we decided to completely rewrite the second part (Chaper 4) of the original manuscript. A pure technical documentation of the campaign is not the objective of this paper..

Our idea for the revised version is now to select two measurement flights on two days with almost identical synoptical situations but different aerosol conditions in the sub-cloud layer. We analyze the "cloud response" to the different aerosol load in terms of the parameter space for cloud-turbulence interaction and study the Twomey effect. With this new section we have covered all four leading topics and have demonstrated the "laboratory-like conditions" of the trades. Furthermore, we now show results which are more unique to the ACTOS / SMART-HELIOS setup as requested by the anonymous reviewer. Modelers interested in small-scale cloud turbulence studies can use these two different days for initialization because the parameter space is completely described and documented. From our understanding this is a good balance between scientific result and documentation

So I would instead prefer to see the paper be more complete. For example, Table 2 in Lu et al., JGR Atmos, 2008 "Aerosol-cloud relationships in continental shallow cumulus" has proven to be very helpful for many subsequent studies. Is there any chance that the authors would consider shifting some of their focus onto this type of synopsis? I can imagine one table for each theme (with maybe another one for basic meteorology), covering the basic properties of interest for each flight, for example. This will help other scientists not directly involved with the project to quickly understand each flight, but also help jog the memory of scientists directly involved as their memories fade a few years down the line.

For technical issues we run a "wiki" including overview-plots and flight documentation for potential users of our data. If someone else would like to use the data – which is more than welcome – we are confident that the data overview (Table and Fig 6) together with the (technical) description of the data/measurements should be enough to see if these data is useful. For more details one has to get into contact with us and we are more than happy to provide access to the wiki but we don't want to publish the wiki in the paper. We think the overview table and Fig. 6 well describe the "mean" conditions for the flights.

I will note that the ACP manuscript review guidelines, under Scientific Significance, asks "Does the manuscript represent a substantial contribution to scientific progress within the scope of Atmospheric Chemistry and Physics (substantial new concepts, ideas, methods, or data)?". In my mind, this manuscript should focus mainly on data and leave most of the science for later. That said, I think the authors should consider these remarks but edit the paper to achieve a balance of documentation and science that they are comfortable with.

We agree with this comment and are confident that we achieved this balance with the new chapter 4. More specific issues such as the details of cloud edges, or Twomey effect and so on are considered for other publications and three manuscripts with scientific focus are almost ready or even submitted/accepted. The focus of the overview

paper is now on mean conditions/observations of all flight days and the very specific conditions for the two selected days in Chap. 4

Specific Comments

I'm attaching a marked-up PDF file of the submitted manuscript with portions highlighted throughout. Those without any comments are basically typos/grammatical mistakes. There are some with some substantial comments as well, which I'll reproduce here:

p. (286)23, line 11: is this sigma with all data put into one pile? it seems to me that one might be just as (or more) interested in a typical sigma for each day. otherwise the sigma might just represent the differences in the means between days.

The sigma is with all data put into one pile and should emphasize on the quite steady mean conditions in the SCL. A sigma for one day is not really robust because we have usually only one profile at the beginning of each flight.

23, 22-23: given the location of deebles pt and the prevailing wind it doesn't seem possible that the land can explain a drier atmosphere. could it just be bias in the ACTOS sampling?

I think this is just a misunderstanding, we speculate that the profiles based on regular radiosondes are drier because they are launched over land. The lidar and ACTOS profiles are over the ocean or at the coast but under comparable conditions. Because the ECMWF products are partly depending on the radiosonde data their profiles could be drier compared with lidar/ACTOS.

24, 4-5: you might consider removing discussion of the ACTOS profiles. I'm very surprised that you have more faith in ECMWF reanalysis profiles than those directly measured by ACTOS, though.

Of course I trust the measurements of ACTOS more than the ECMWF product but anyway it should be considered that we have one complete profile for each ACTOS

flight and the variability in terms of the humidity profiles cannot be ignored - the same argument is of course valid for the radiosonde profiles but maybe this is going too far. We have deleted one misleading sentence in the manuscript.

25, 25: need supersaturation. make sure you clarify this throughout the manuscript, since the instrument description is that it measures ccn at "different supersaturations".

Absolute correct statement, we went carefully through the manuscript to change accordingly. In fact, for the manuscript we only use CCN data from profiles, which are all measured at 0.25

The following comments relate to the manuscript part that has been completely rewritten but we will answer to a few points, anyway.

28, 1: it sure doesn't look obvious to me. if you really want to make this point, please show a scatter plot so we can more clearly see the quality of the correlation.

We will not discuss the issue of giant nuclei in this paper but in general we agree that a scatter plot is necessary to discuss this relation more quantitatively.

29, 6: this seems rather low to me, i'm guessing by a factor of 2 or more. given the aerosol concentrations you find, what is the updraft velocity this would correspond to for a rising adiabatic parcel?

We have removed this part but one of our PhD-students is preparing his thesis focusing on this question; a publication about this topic will be prepared by the end of this year.

31, 4-5: I think you're probably correct, but I don't think the data really shows this. Panel A in Fig 11 shows bursts of particles when clouds are absent and present. And clouds occur without the presence of particle bursts. Just because the inset suggests some relationship, I don't think that the data can rule out that clouds and particle bursts are randomly distributed without any causal relationship at all.

Interesting point: we will not discuss these observations in this paper anymore but an

independent publication about this topic will be compiled. About 90

31, 21: really? to my eye, there is a fairly substantial fraction of points where the difference appears quite a bit larger than this. if you really want to make this comparison, it seems that a scatter plot is a more appropriate presentation of the data.

This part has also been removed and in the new section only a short paragraph about the Twomey effect is included. The largest uncertainties are introduced by 3d-effects caused by a high degree of cloud inhomogeneities. Therefore, in the revised manuscript only "more homogeneous" cloud fields are considered and the agreement between retrievals and in-situ measurements are much better. Anyway, if you are interested please see the accepted paper by Werner et al 2012, JGR.

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