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Atmospheric Measurement Techniques Discussions

Interactive comment on "Joint Analysis of Convective Structure from the APR-2 Precipitation Radar and the DAWN Doppler Wind Lidar During the 2017 Convective Processes Experiment (CPEX)" *by* F. Joseph Turk et al.

Anonymous Referee #2

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General comments

This well written manuscript presents novel, unique and relevant collocated airborne Doppler lidar and radar measurements in complex, convective subtropical environments. Focus is set to a common display of both instruments' data sets from two exemplary NASA DC-8 research flights, to show regions of common data overlap and the measurement limits of each instrument. The paper is fully suitable to the scope of AMT, and both the scientific relevance and the data quality are outstanding. However, or because of this, I find it disappointing that scientific conclusions from such an inter-

esting instrument combination are missing, and that the reader is just referred to future studies.

There exists a WCRP Grand Challenge on Clouds, Circulation and Climate Sensitivity (which the authors do not address - anyway), because one of the big science questions is the feedback of convection on dynamics. This DC-8 instrumentation is perfectly suited to address this question, and I think the readers would like to see more details and (preliminary) conclusions hereto.

So here is my suggestion how to avoid disappointment without too much extra effort. The interesting questions are: what is the evolution in time of the probed cloud cluster? Did you observe secondary circulations due to cloud growth? Is convergence or divergence visible in the measurements? So, at the end of section 3, maybe also 4, you should answer these questions. An extra, perhaps 3-d, sketch of the cloud cluster with the essential wind arrows resulting from all flight segments at both 2- and 8-km heights would be very helpful.

Specific comments

The abstract is misleading. It repeats the nicely written overarching science issues from the introduction (it is OK to address them in the introduction), suggesting to the reader that these big questions are the main topic (which is not quite correct), but in fact it lacks the major results of the paper (= the answers to my above questions). Furthermore, it mentions "transport of water vapor" (line 15) and "Frequent dropsonde data" (l. 19) which both are not major topics of the paper.

Lines 37 and 64: these sentences highlight the importance of the vertical distribution of water vapor, a very interesting topic, yet which is (unfortunately) not addressed in this paper. The dropsondes could provide the humidity profiles, but I guess there were too few of them on these two flights to make solid statements, and/or this topic is beyond the scope of this study? Could you comment on this?

Line 79: “. . .radar and DWL observations from two exemplary flight days” to be more precise. In this context, I find that Table 1 is not at all needed.

Fig 1 is difficult to understand. Fig 5 suggests that at 2 km asl the +-45° lidar positions at 30° off-nadir angle have about the same separation than the APR swath width at +-25°. Could that be illustrated in Fig 1?

Line 125: please explain the synoptic situation, and in more detail why you chose this particular situation out of 16 flights. Is the isolated cloud cluster you probed a beginning MCS? What is its relation to the extended cloud band to the north? Was this situation typical for the whole campaign, or was it a very particular “golden day”?

Fig 4 is very difficult to understand: what if you would swap Fig 4 and Fig 5? Beginning with the zoom, it would be much easier to understand the heavy full segment 1 overview. Why is the dropsonde at different places? Were there two different dropsondes? Please explain.

Line 225 and caption of Fig 8: explain why you think the convection is growing. Fig 15: is green still the dropsondes? So there are many more dropsondes on this day? Please explain.

Technical corrections

Line 45: 2x "associated“, and verb is missing.

Line 107: remove “highly capable”.

Line 110: “ a constant 30° elevation angle”, I do not understand, you mean probably an off-nadir angle of 30°.

Fig 2: the GOES image in the expanded box is very coarse, it should be available at much higher resolution if this is a visible imagery, and lat/lon indications would make the big image easier to interpret. Also, highlight the 4 segments from sections 3.1 - 3.4.

Line 140: “before they had developed..”

Lines 160 and 176: the unit dB is probably wrong when characterizing a DAWN SNR level.

Line 185: “show the LOS projections to msl from the DC-8”

Fig 5: in the upper panel there are no winds, and in the lower panel there is no dropsonde, so you may want to adapt both lower left text boxes.

Fig 6: I do not see any dropsonde, so you may want to adapt both lower left text boxes.

Line 249: “winds (not shown)”, you could refer to Fig 6, showing a region quite close where the winds are shown.

Line 256: “of the DAWN. . .”

Figs 9, 11, 13 and 15: the color bar is too large.

Line 393: 2x “examine”

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