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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Aerosol and thermodynamic effects on tropical cloud systems during TWPICE and ACTIVE" *by* P. T. May et al.

P. T. May et al.

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Response to reviewer’s comments

Response to Reviewer 1. Firstly, we would like to thank the reviewers for their constructive reviews. Please see the responses to the issues raised below. Comment 1. The technique used is to separate the population of storms into high vs. low CAPE, CIN, or aerosol, or high vs. moderate vs. low shear, and to compare the means and standard deviations of various storm properties for each subset. This is a clumsy way to do things because (a) correlations between individual (presumed) independent variables may hide true causality (we see one example of this, in fact - the unexpected shear dependence is explained as a wind speed and time to build CAPE dependence); (b) if storms are clustered near the cutoff value between high and low subsets (as will be true if any of these variables has a near-Gaussian pdf) the actual dependence on any variable will be muted. I realize that the authors are dealing with small-number statistics, but it would at least seem valuable to (a) separate storms into lowest and highest thirds or quartiles instead, and (b) scatter plot the independent variables for each storm against each other to reveal correlations (e.g., CAPE vs. shear, aerosol vs. CAPE, shear, CIN) that complicate any interpretation. This is especially true for the tentative aerosol effect the authors report - they tell us that there are similar CAPE values for high vs. low aerosol, but how about the CIN and shear values for these subsets? If high vs. low aerosol are color coded blue vs. red in a scatter plot of CAPE vs. shear, much of the needed information can be presented in a single plot.

Response. As the reviewer points out, and is mentioned in the text, the thermodynamic indices used here have some significant potential correlations and these are discussed, as the reviewer mentions. Regarding the use of comparing top and bottom quartiles etc, the issue is as the reviewer states the relatively small data set. With the reduced samples the significance levels of the differences changes only very slightly and in fact the means of the cloud system indices also change only slightly. Therefore I have not included revised figures and discussion, but have made a comment regarding this at the end of Sect 4. I have included the suggested scatter plot, as this does indeed show the relative independence of the CAPE, shear and aerosol regimes in Darwin. The possible correlation of the achieved CAPE on the islands as distinct from the mainland with shear/mean wind is discussed. Note the CAPE is estimated from a 9am sounding (P3).

Comment 2. Shear effects on convection are more complex than the manuscript suggests. A certain amount of shear is conducive to further development, but too great a shear separates downdraft cold pools from the parent storm by too great a distance and inhibits mesoscale cluster development. Some numerical studies suggest that the combination of CAPE and shear is the crucial parameter (e.g., Weisman and Klemp 1982, Mon. Wea. Rev.).

Response: These tropical observations are in a relatively low mean shear environment

ACPD

8, S8174–S8177, 2008

Interactive Comment

(see the new Fig 3._ compared with , for example the high shear cases in Weisman and Klemp (1982).

3. CAPE is a nice summary parameter, but the vertical structure of the buoyancy profile ("shape of the CAPE") may be more relevant to storm strength, with buoyancy below the freezing level being especially important.

Response: I agree with these comments, but the aim here was to stratify data using some simple indices. The CAPEs being used were the operationally available values, but the CAPE up to the freezing level may be a useful index for future work. May et al MWR 2002 showed examples where essentially all the CAPE up to the FZL was being released in tropical showers in a similar environment to these Hector storms.

Comment 4. On p. 14259 the authors suggest that different mid-level humidities may complicate the interpretation of an aerosol effect. But the parameter they show us is equivalent potential temperature, which depends on both temperature and humidity. The way to illustrate humidity differences is to plot relative humidity profiles for each subset directly.

Response. I think the Theta E profiles show quite graphically the differences in the mid level moisture and also provides some of the kind of potential instability information discussed in the previous comment. The potential temperature profiles are essentially identical (now in text). I also note that by definition the relative humidity is also temperature dependent

Regarding the minor technical comments. 1. CAPE is calculated with Theta V (text is corrected). 2. Unfortunately, we do not have reliable lightning data for the whole period. 3. The uncertainty in CAPE’s is largely due to meteorological noise and is difficult to estimate. We could probably devote a whole paper to this issue. 4. I have edited the text to reflect the confidence levels which were explicit in the figures and discussion. 5. I have changed the figures to all use LT.

ACPD

8, S8174–S8177, 2008

Interactive Comment

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Discussion Paper

Response to reviewer 2

I thank both reviewers for their careful and constructive comments. With regard to the general comments, I agree with the excellent suggestion of examining drop size distribution information in a statistical way for different environmental regimes. Some of this work is underway. We are looking at retrieved drop size distribution parameters for a variety of regimes and locations. This is mentioned in the conclusions as future work as it will be a substantial new piece of work. Within this, our plan is to extend this to wider areas as well as the island thunderstorms.

Specific comments: 1. Regarding the distributions of the storm parameters. The distributions pf the parameters within a regime are reasonably Gaussian with the exception of the 40 dBZ areas but the samples are small. This is discussed now on page 4. Both technical comments have been done.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 14247, 2008.

ACPD

8, S8174–S8177, 2008

Interactive Comment