Atmos. Chem. Phys. Discuss., 8, S6299–S6301, 2008
www.atmos-chem-phys-discuss.net/8/S6299/2008/

Atmospheric Chemistry and Physics Discussions

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8, S6299–S6301, 2008

Interactive Comment

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

Anonymous Referee #1

Received and published: 24 August 2008

This manuscript contains potentially useful information about different characteristics of the deep convection over the Tiwi Islands in response to different atmospheric conditions. The primary limitation is the fairly crude separation of the various effects, which limits the reader's ability to understand cause and effect. The manuscript is suitable for publication after the following minor comments are addressed.

Primary comments:

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.

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.).

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.

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.

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Minor technical comments:

1. CAPE should be calculated using virtual potential temperature.

2. Was lightning measured during this time period? Does it provide any relevant information?

3. p. 14257, l. 3: "CAPE values have large uncertainties" - how about an estimate of the uncertainty?

4. p. 14257, l. 5: I would not call the results a "clear signal" in many cases, considering the $>$ 50% probability of equal means for so many of the plots.

5. The authors label Figs. 4-7 with UTC, which is pretty useless to the non-Australian reader. Use LT, which immediately conveys information to any reader.

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