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Interactive Comment

Interactive comment on "Cirrus clouds in convective outflow during the HIBISCUS campaign" *by* F. Fierli et al.

Anonymous Referee #4

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General comments

The authors have a worthy goal: combine a unique set of balloon-borne microlidar measurements with mesoscale model simulations in order to evaluate the hypothesis that the observed persistence of thin cirrus in deep convective outflow depends upon formation mechanisms not included in most mesoscale models. A strength of their approach is that it is guided by field measurements. However, they inexplicably exclude from their analysis the simultaneous balloon-borne water vapor measurements that were made during HIBISCUS and are crucial to understanding cloud formation and decay processes. There is also a lack of discussion regarding the basic cloud microphysics treatments in their model. There are outstanding scientific questions about such treatments that bear directly upon the study's conclusions, and it is not currently clear whether the authors are aware of those questions. Finally, model results are insufficiently explained and presented (especially modeled cloud properties). These omissions are worrying; many minor inconsistencies and grammatical and typographical errors also indicate an overall lack of care taken. I do not recommend acceptance. If revisions are requested, I would recommend that the authors address the following comments within their revised manuscript.

Specific comments

1. Use of MODIS data: The difference between MODIS aerosol optical thickness and cloud optical thickness retrievals is important. What is shown in Figure 2 with a scale that does not extend past 1.0 and a legend reading simply "optical thickness" is discussed in the text as "aerosol optical thickness" (which should not be retrieved in the presence of clouds). Cloud optical thickness, on the other hand, often exceeds 1.0 (especially near deep convection), and it is misleading to screen out those higher values from Figure 2 (as done now) because that obscures where the deep convection towers are located. Also, Aqua MODIS data appear available at circa 17:00 UTC on 24 February, which is closer to the beginning of the SF4 flight than the 24 January 13:00 UTC Terra data shown here (and I assume the authors meant February rather than January). Is there a reason those Aqua data were not used?

2. Statistical analysis of lidar data: The cloud layer distinctions described by the authors at the bottom of page 6744 appear incorrect and/or exaggerated. In Figure 3, the box outlines that overlay the lidar images obscure the fact that the three layers appear quite continuous with one another horizontally (the figure should be enlarged or the box outlines dashed or some other adjustment made). The text also states incorrect generalities about the statistics that are shown in Figure 3. For instance, it is stated that BSR values are "up to 30" and depolarization values

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"$<$20%" for the first layer, but Figure 3 clearly shows BSR values up to 40 and depolarization values that are usually *above* 20% (almost no D values are below 20% in any of the three boxes).

3. Exclusion of water vapor data: The only presentation of flight SF4 water vapor data is the text statement that "supersaturation reaches 130% throughout the layer where cirrus were observed" (page 6745, lines 1-4) per Durry et al. (2006). I fetched that paper in order to view this data: is their Figure 4 what the authors intended readers to consult? It indicates no such RHI values, as far as I can tell, and is difficult to read. At a minimum, the authors should instead refer to Marécal et al. (2007), which includes clearly legible RHI profiles in Figures 9 and 10. I am bothered by this exclusion of such a fundamental aspect of the data set from a paper analyzing cirrus cloud persistence. The presence or absence of correlations such as those found by Shibata et al. (2007, JGR 112:D03210, Figure 4b) would seem to be a crucial aspect of analyzing cloud formation mechanisms, for instance.

4. Model description: It is unclear to me how RHI exceeding 100% is obtained with the Kain-Fritsch parameterization, and I think that this needs to be explained clearly. A reviewer of the Marécal et al. (2007) manuscript also noted that obtaining RHI greater than 100% in BRAMS model must certainly be "ad hoc"; those authors didn't sufficiently address that reviewer's point in their revised manuscript, in my opinion (they did not sufficiently point out the ad hoc nature). In short, the authors should explain very clearly in the manuscript how RHI exceeding 100% is obtained in the aged cloud shown in Figure 7. Peter et al. (2006, Science 314:1399) recently provided a concise summary of the scientific issues, and they go to the heart of the cloud physics analyzed here–especially cirrus formation and especially studies using models to study such processes.

5. Modeled cloud properties: Figure 7 (or a new figure) really must indicate some-

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thing about where the cloud is located in model results. There is currently no indication of where the cloud is located at this point, which is a major omission. What phase is this cloud? What mass of condensate? What number concentration assumed? Are the predictions consistent with the BSR values from the lidar? Not nearly enough is shown here (or discussed in this manuscript) to understand what is going on microphysically in the model simulations. Since the point of the manuscript is to seek evidence for two differentiated formation mechanisms, much more analysis is required here, in my opinion.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6737, 2007.

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