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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Detailed flow, hydrometeor and lightning characteristics of an isolated thunderstorm during COPS" *by* K. Schmidt et al.

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General comments

This article provides a detailed dynamical and microphysical characterization of an isolated thunderstorm over the Black Forest from COPS IOP8b (15th July 2007). High-resolution observations are synthesized from satellite, multiple radars, photographs, and a lightning detection network to document the evolution of the storm, its internal air motions, and its hydrometeor distribution. Altogether it provides an amazingly vivid picture of the storm from multiple perspectives, reinforcing the value of intensive field experiments for illuminating otherwise elusive atmospheric processes. It complements

the extensive observational study by Behrendt et al (QJ, 2011) of the same event. A high-resolution numerical simulation of the storm is also presented, which captures its timing, structure, and location remarkably well (albeit not perfectly). Some speculation is provided for the lack of subsequent deep convection following the decay of the main storm. The figures are excellent and the text is very well written.

Specific comments

I was surprised by the lack of interpretion of the numerical simulation. It is presented as a show-and-tell of modelling technology but not a tool for science. The authors mention the potential utility of the simulation(s) for future sensitivity tests, but the simulation presented here is valuable in its own right. This numerical dataset could complement the observations in the quest to answer an important question: why did only a single storm develop? This is speculated upon in section 6 (bottom of P. 9742), but the discussion is not particularly convincing. The lack of vertical wind shear does imply that individual storm cells are probably short lived, but it does not rule out the formation of new cells along the same convergence line, particulary behind the main storm in airflow that is unaffected by outflow from the main cell. A full explanation might also consider the evolution of conditional instability and low-level forcing along the mountain convergence line.

Also, the basic motivation for this study given at the end of section 2 could be strengthened. I recommend emphasizing that despite the intense previous investigation of this particular event, no study has adequately synthesized the multiple remote-sensing platforms to reveal the detailed internal cloud structure (as far as I am aware, at least).

Technical corrections (to keep this compact, only the more notable ones are listed)

1. Throughout the paper the term "synoptic" is used to describe this study, but the actual processes of interest are definitely not synoptic. I suggest replacing this term with a more appropriate one (e.g., "mesoscale" or "cloud-scale") throughout.

2. Similarly, the term “cloud dynamics” is often used instead of the more appropriate term “turbulence”. This is particularly true for the analysis of radar-derived spectral width.

3. For consistency with the community, I would also replace “episode-type numerical prediction runs” with something like “numerical case studies”.

4. P. 9179, L. 7-8: “dynamical” -> “dynamically”

5. P. 9722: L. 10: “to its rapid” -> “on its rapid”

6. P. 9723, L. 24: “retrieved air the airborne” -> “retrieved from the”

7. P. 9724, L. 1-3: The sentence beginning with “Moreover” is a bit of a potentially misleading oversimplification of the Hanley et al (2011) conclusions. This sentence can probably be removed without damaging the paper.

8. P. 9727, L. 1: “proofed” -> proved

9. P. 9727: L. 21: “type hydrometeors” -> “hydrometeor types”

10. P. 9728: L. 5: “to multiple” -> “for multiple”

11. P. 9728, L. 26: “to consistent” -> “to obtain consistent”

12. P. 9730, L. 4: “with the same orientiation” -> “with the same orientation as the emitted pulses”

13. P. 9731, L. 25 and Fig. 2: The text should specify that this is the minimum brightness temperature”

14. P. 9732, L. 23: What do you mean by “increased?” - did it become larger or stronger?

15. P. 9733, L. 3-4: This is misleading. The decay of the storm isn’t just beginning at 1520 - one could argue that the storm was well into its decay phase by 1450.

16. P. 9733, L. 18: Why is the cloud tail a region of entrainment?

17. P. 9734 and Fig. 5: It is worthwhile to mention that the conditional sampling of winds within the cloud regions should not be expected to perfectly represent the background flow. Vertical momentum fluxes and buoyancy-driven circulations should lead to significant variations in the mean cloud properties relative to the environment.

18. P. 9735, L. 17: “also for” -> “during periods of”

19. P. 9737, L. 4: “summarizing” -> “combining”

20. P. 9737, L. 13: “high vertical motion” -> “deep vertical motion”

21. P. 9737, L. 21: “The evolution of distribution of hydrometeor content” -> “The evolution of hydrometeor distribution”

22. P. 9738, L. 15-17: Any idea why the algorithm detects hail mainly near the melting layer and below at 1435? I would have expected it to exist higher into the cloud.

23. P. 9738, L. 22-24: This sentence is confusing - the approach already made sense and this serves to confuse it.

24. P. 9739, L. 20: Do you mean “reflectance” rather than “reflectivity” here?

25, P. 9739, L. 25-27: I don’t understand this description. What is the 1 to 2 km altitude level at this area? Also the ranges provided in the text don’t match the figure.

26. P. 9740: You mention that ECMWF analyses are used for the model initiatialization. How do you get the lateral boundary conditions? Are these driven by analysis or by a true forecast? And it would help to see a plot showing the grids used by MesoNH.

27. P. 9740, L. 25: This is certainly not northerly flow.

28. P. 9743, L. 21: I would replace “over-compensating” with “dominating”

29. P. 9744, last paragraph: I suggest replacing this extremely long sentence with an itemized list.

30. Table 2: What is meant by “ca. 3” for the repetition rate for the DOWs? Also you might want to mention the wavelength range of each radar in the table.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9717, 2012.