

Interactive comment on “Characteristics of Vertical Air Motion in Convective Clouds” by Jing Yang et al.

S. Collis (Referee)

scollis@anl.gov

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I always like to see an in-depth study of vertical motions in the atmosphere because, as the authors point out, understanding these is vital to improving our understanding of (and hence modeling capabilities) many processes influenced by vertical motions.

First, before I get to the science, this document was not ready for submission in any form. It is riddled with typographical errors making it very difficult to get to the science. I started to list them but, frankly, this is the job of an editorial service, something I recommend the author take advantage of. For example: "The COPE project was conducted from 03 July to 21 August, 2013". This is not English.. "The COPE project was conducted from the 3rd of July to the 21st August, 2013".. Write in English not in code.

I have two broad areas of concern with this manuscript:

C1

1) The authors do not address the idea of sample size or sample bias OR more importantly geometric issues of sampling, in a line, a 2/3D object (being an updraft core). See Giangrande et al 2013 for a discussion of issues with profiler systems and angle of attack.. Basically if you dissect an updraft core how do you know if you hit the strongest part of the updraft? Furthermore, up until the end, the idea of selection bias is not addressed. Even the C-130 will avoid the strongest cores. You can not build a PDF out to the tail from aircraft measurements..

You can, as the paper did somewhat, look at intrinsic updraft properties. But you can not look at the distribution. I am somewhat disappointed, given the brief reference to microphysical measurements, that the authors did not relate vertical motions to microphysical properties of the updraft cores. This is something in-situ platforms are uniquely capable of doing. Also, in the literature review of methodologies for measuring vertical motions the authors neglect scanning radar measurements such as those shown in Collis et al 2013 and Nicol et al 2015 (not to mention a raft of airborne radar measurements from the NOAA p3 (look for papers from Jorgensen) and other aircraft that use the vertical plus 45 degree tilt methods..

2) This comment relates to a specific question asked by the Journal in its review criteria "Are substantial conclusions reached?". I am deeply concerned by the authors attempt to relate the three field programs and say something about maritime versus continental convection. For one, the author did not put the cases into context.. What was the CAPE for various cases? etc.. A selection of clouds at each campaign a climatology does not make. While the author caveats his comparison even the attempt to contrast the different regime is dangerous. For one, as mentioned, the strongest cores in the region of HiCu would all but destroy even the C-130 (See the various photos associated with the Byers et al study of hail damage). To attempt to make a comparison, then state it goes contrary to common conception (Continental » Maritime) and then turn around and say "we did not sample the strongest updrafts in the continental case" is disingenuous.

C2

So negatives out of the way, one of the things that redeem the paper is the the focus on updraft shape and how that varies with height. Personally I find this very interesting as not only does the mass flux of a plume influence transport but the vertical velocity within determines many microphysical aspects. ie a plume that starts thin and then expand for the same mass flux would have lower vertical velocities aloft influencing processes like Hallett-Mossop splintering etc.. (and associated latent feedbacks).. The paper should focus more on this and the *intrinsic* differences. Things that are co-varying and less susceptible to sampling and decision bias.

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