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Interactive Comment

Interactive comment on "Diagnosing the average spatio-temporal impact of convective systems – Part 2: A model inter-comparison using satellite data" *by* M. S. Johnston et al.

Anonymous Referee #2

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General Comments:

These observations-vs-model studies documenting the hourly-timescale evolution of spatial-averaged convection/rainfall parameters are really worthwhile and great to see from a process-study perspective. Any obs/model analyses along these lines still remain novel, and therefore, a paper such as this one is very needed.

My one issue with this analysis relates to the sensitivity of the results to the maximum rainfall threshold chosen in determining deep convection at hour-0. For a given local maximum in scene-averaged rainfall, the one (of a 2-part) question to ask is: *when a

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Interactive Discussion

Discussion Paper

given scene-averaged rainfall occurs, to what extent does a model simulation resemble the observed temporal evolution? If it only occurs once in the model and 10 times in the observations, we don't care yet – one basically wonders if the evolutions are similar *when they occur. This is "issue A." The other issue is: does the model get the relative frequency of occurrence of the local maximum in rainfall correct and the relative frequency of occurrence of the evolutions tied to local rainfall maxima correct? In other words, models can 1) simulate the evolution of convection correctly (or not), and 2) get the frequency of occurrence of convection events correct (or not). These should be investigated separately; if not, results are hard to comment on and interpret.

I think this study mixes the two above issues by utilizing different rainfall thresholds. In turn, all the composites of parameters strongly coupled (in a water-balance, energy balance sense as well) to hour-0 rainfall (e.g. rainfall itself, CF, IWC, UTH, OLR) become strongly dependent on what part of the rainfall PDF you decided to use for compositing. An example of what I am talking about can be seen in Fig. 5 of Elsaesser and Kummerow (2013, Journal of Atmos. Sci: "A Multisensor Observational Depiction of the Transition from Light to Heavy Rainfall on Subdaily Timescales"). Here, for varying grid-averaged rainfall at hour-0 (similar to your analyses), drastically different evolutions of rainfall and saturation fractions (integrated relative humidity) occur as hour-0 rainfall changes. So, if you calculate your composites using different thresholds (say, a temporal evolution of rainfall and sat frac for hour-0 maxima ranging from 2 mm/hr to max –versus- 1 mm/hr to max), you would get different evolutions of rainfall and saturation fraction. Similarly, I would imagine that if you had chosen 2 mm/hr as a minimum for CAM instead (and kept the other model thresholds near 0.5 mm/hr), then suddenly the comparison of one model parameter composite to another would be different.

One has to get around this. In effect, there has to be some sort of normalizing – a sort of "parameter evolution per rainfall amount" perhaps. . .the authors could come up with something along these lines (alternative recommendation is below though). Overall though, I think it is a necessary thing for this study. In short, based on hydrological/energy cycle arguments, if the maximum rainfall rate averages do not agree at hour-0 between two products, then of course UTH, cloud fraction, IWC and OLR evolutions will not either. The alternative to some sort of “normalizing” could be this: Realizing that the authors want to compare the deeper convection/rainfall and starting at some consistent higher threshold in rainfall, pick some starting consistent max rainfall bin for the observations and models, and compute the evolutions of UTH, etc and store that. (Call this “Evolution Type 1”). Do it again for another rainfall bin, and compute the composites again separately for the models/obs. (Call it “Evolution Type 2”). Keep doing this for all the heavier rainfall rate bins until you reach the point where either the model or observations no longer has an agreed-upon maximum hour 0 average. At the very end, do a straight average all these realizations together (not taking into account how often each evolution occurs) and you'd ensure you have the same hour-rainfall maximum for obs/model, and then you can compare the associated UTH, CF, IWC and OLR fields most fairly (apples to apples) and address the question: when they occur, regardless of how often, how agreeable are the realizations?

Now, at this point, the authors could comment on differing frequencies of occurrences of the differing realizations (i.e we would learn if “Evolution Type 1” occurs much more frequently in the obs than in the models, *and we would know if, when it occurs, how well the model reproduces observations). With the paper as is, at this point, these things are mixed, and I cannot tell if the model does do well for a given convection evolution type (it may not though!) but has it occurring at the wrong frequency rate.

Specific Comments:

1) The rainfall PDFs in Fig. 2 – are these after rainfall was averaged to 1 degree? If so, the TMPA PDF seems to have a suspiciously large occurrence of 1-deg rainfall rates for bins > 50 mm/hr. I suspect each PDF is for model/obs at their native resolutions then? This was not clear.

2) For some of the discussion on the too regular/strong diurnal cycles of CF and IWC

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in the models. . .perhaps an artifact of the dependence of convection on CAPE in the GCMs (and utilized convection parameterizations)? No need to do further investigation, but perhaps a comment on it or mentioning it might be good.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9155, 2014.

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