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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnosing the average spatio-temporal impact of convective systems – Part 1: A methodology for evaluating climate models" *by* M. S. Johnston et al.

Anonymous Referee #1

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General Comments:

The authors document the short (sub-daily/hourly) timescale evolution of upper-tropospheric relative humidity, albedo, OLR, etc. relative to local maxima in surface rainfall (which they assume serves as a proxy for deep convection). The goal is to determine the impact of deep convection on the environment, similar to (and building on) prior work performed by Zelinka and Hartmann (2009). The new/novel components of this work then pertain to importing this same analysis technique to model output (here, the EC-Earth climate model). A particularly worthwhile aspect of this work deals with evaluating a climate model on these hourly timescales where convec-

tive processes/parameterizations operate (i.e. process study approach), as opposed to simply comparing an observed tropical climate field/map to a model climate field (which is what is often done).

One big issue I had with this work deals with the interpretation of results. The authors note that the convective events "propagate" westward at $\sim$4m/s. They note the opposite occurs in the model (slow eastward propagation). The use of "propagation" is awkward as is since the analysis presented is an Eulerian one, but the slow (and opposite) speeds and details provided within the manuscript suggest to me that they're, on average, comparing convection primarily associated with one convectively coupled wave type in the observations (Equatorial Rossby [ER] wave?) and convection, on average, associated with a different wave type in the model (Kelvin wave perhaps?). This isn't necessarily a problem at first glance, except that the general atmospheric flow and spatial scales associated with each wave type is different. This may imply that certain fields (say humidity, or stratiform cloud, or ice) may be different not because the model is making convection behave differently from what observations show, but because the advection and spatial scales of anomalies are different when "similar" convection is embedded, on average, in a different wave type.

I could envision one finding similar results in an observations-only study by comparing fields associated with an ER wave to fields associated with a Kelvin wave. For example, if one wave has associated with it more intense pole-ward upper-trop divergence and the other has more zonal flow (like a Kelvin wave for instance), then the spatial scale/field of humidity may be more spread-out (in a pole-ward sense) in the upper troposphere when you are comparing convection in one wave to convection in a different wave. I question if some of the differences shown in this work are not necessarily the result of fundamentally different convection/environment relations between the model and observations (of course, this is certainly plausible too), but because on average (i.e. a composite), the convection is embedded in one wave type in the observations and another in the model. This is my one large concern with the results shown in this manuscript, and because a good deal of the discussion centers on how spatially expansive/broad anomalies are, the dominant wave types varying between the model/obs could definitely matter.

On the non-science side, while it became too much for me to keep track of each and every grammatically inaccurate statement, I noticed it enough to recommend a thorough grammar check/proofreading before re-submission. I did make note of a few specific sentences below, but it is far from a complete inventory.

Specific Comments:

1) The use of the term "DC events" throughout seems inappropriate. The authors are looking at convective envelopes (that may contain a number of MCSs and propagating systems) that, in a composite, will look like a 1000-km blob. But, it's not really an event – it could very well be the all the convection/rainfall associated with a propagating convectively coupled wave. I'd suggest thinking about using the term "DC event" and maybe re-defining a new term.

2) In the abstract (and in the manuscript text itself), it's noted that "DC events move eastward… westward…"). This is a composite, and convection associated with MJO/ER/Kelvin (and other wave types) will move in different directions. Whichever wave is more dominant (eastward moving? westward?) and more "rainy" will, after compositing, slightly tug the average propagation direction to just greater/less than 0, at least I would think so. You could be averaging 1000 convective envelopes that move eastward at 5 m/s, and 1000 that propagate westward at -10 m/s, and get an average of -5 m/s. I'd re-state throughout. The speeds noted (4 m/s) are slow anyways – even slower than the composite MJO – and convective "events" or MCSs themselves go faster than this.

3) P. 13658, lines 15-20: If ice particles scatter microwave radiation and lower the Tb, wouldn't that cause an underestimation of UTH? The scene would appear to be "cooler", thus less emission from water vapor (i.e. less water vapor). The authors said

this would result in an "overestimation of UTH". Please verify/check.

4) P. 13660, lines 15-20: What is the vertical resolution of the model? I realize it is likely not constant with greater resolution in the lower-trop, but please give some average numbers (or average lower-trop res and average upper-trop res).

5) P. 13663, lines 5:10: Time period used is 2007-2008. I am wondering – can this be expanded? I note the sampling issues with, say, CloudSat (e.g. Fig. 3). Can the latter part of 2006 and any additional months after 2008 be used that would result in a nicer composite of CloudSat results?

6) P. 13665 and 13666, discussion of Figures 1 and 2. The authors discuss Fig. 2 first, and then go to Fig. 1. Perhaps, to maintain discussion in chronological order with figure numbers, these two figures should be reversed in order?

7) P. 13667, lines 1-5: "…variables that are least affected by aliasing." Unless I'm mistaken, aren't some of these most affected (like OLR for instance) by aliasing due to the architecture of the satellites orbits per the discussion regarding Fig. 1?

8) P. 13669, line 11: What does this statement mean? "The cloud ice content and cloud fraction follow the ITCZ pattern at 200 hPa."

9) P. 13670, lines 15-20: The observed and model OLR patterns are discussed and compared. Could what is found be the result of wind flow pattern differences associated with different wave types (one composite wave type in the obs, another in the model) causing different advective tendencies of high cloud/vapor? This pertains to my general discussion/issue with all results presented within.

10) P. 13672, lines 20-25: In the model, there are few clouds from above the freezing level (500 mb) to below 250 mb. Is this due to the spectrum of cloud types allowed in the convective parameterization? (i.e. only really deep plumes allowed?). And, where is the problem – in the stratiform or convective cloud field? Stratifying Fig. 6 into convective/stratiform cloud fractions could provide some insight on what cloud types

are missing. Is it possible to do something similar for the observations or no?

Technical Corrections:

[Not a comprehensive list, and I did not verify that all the references were valid/checked]

1) Figure captions/labels are too small.

2) P. 13655, second paragraph of Intro – poorly worded/grammar.

3) P. 13656, lines 19-20: maybe re-word the sentence?

4) P. 13661, line 5: add “the” before convective

5) P. 13661, line 20: “elaborated” – maybe use “modified”? elaborated sounds like you’re explaining it further or better, which is not what I think you are trying to say.

6) P. 13664, line 18: add “in” after “difference”

7) P. 13665, line 10: left off Figure number.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13653, 2013.