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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The diurnal cycle of rainfall over New Guinea in convection-permitting WRF simulations" *by* M. E. E. Hassim et al.

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This is an interesting and well-written paper that draws attention to a pressing need to better observe, simulate, and explain the diurnal cycle of convection over the Maritime Continent region. The authors do an excellent job in reviewing our current state of knowledge on the subject, as well as in describing the complexity of the problem and the various potential mechanisms that may be involved. The convection-permitting WRF simulations are at the cutting edge, both in terms of domain size and model resolution. In terms of discussing the strengths and weaknesses of the simulations, the authors do a fairly good job, although I believe that some further discussion/mention of an apparent weakness of the model is warranted. The analysis and interpretation of the model output is also lacking in some important respects. Further discussion of these two major concerns, followed by a list of minor concerns, is given below.

Major Concern 1:

My first major concern has to do with the comparison of the observed versus simulated diurnal cycle of rainfall in Fig. 4. In particular, while the authors note that there are some differences in the timing and intensity of observed vs simulated rain features, a key difference not mentioned is that the phase speed of the simulated off-shore propagating squall line is much faster than observed (~5-7 vs ~1 m/s). This difference in phase speed is highlighted by the sloping yellow lines in my Fig. 1, which is an annotated version of the paper's Figs. 4c–f. Also noteworthy is that the apparent propagation speed of the offshore-moving system is closer to observations in the 1.33-km free-running simulation, while the signal and speed of this system is not as discernible in the 4-km free-running simulation. The morphology of the simulated off-shore propagating squall line is therefore not robust, although I do understand that the free-running simulations cover a shorter time period than the set of re-initialized runs. Nevertheless, what is robust across these model runs is the roughly 6-m/s propagation speed (sloping red lines) of a broader "envelope" of convection that moves from the mountains to the coast and beyond. Interestingly, this same sort of propagating envelope is also apparent in the TRMM observations, although in that case the envelope appears to move much faster at around 12-15 m/s. Obviously, the latter speed is close to that of the n=3 gravity mode, which the authors demonstrate is present in the model but does not effectively modulate the simulated convection. A question then emerges as to whether the observations are erroneously missing the signal of the simulated 6-m/s propagating envelope (due to potential problems with the TRMM data, as discussed by the authors) or whether the model is erroneously emphasizing coupling of convection to a both slower and shallower gravity wave mode, at the expense of coupling to the $n = 3$ mode? One possible way of addressing this question would be to appeal to another well-established (though less widely utilized) satellite-derived rainfall product:

CMORPH, which is available from NOAA at a resolution of 30 min in time and roughly 8 km in space. Barring this sort of effort, I think at a minimum that some shift in tone of the paper is needed to reflect the lack of robustness concerning the simulated offshore squall-line and the uncertainty about whether the simulated broader envelope of propagating convection is moving too slow or the observations are indicating a propagation speed that is too fast.

2) My second major concern has to do with the authors use of CAPE as a diagnostic tool for explaining variations in the simulated convection. As is well known, CAPE depends on just two factors. The first is the temperature and mixing ratio of the surface parcel, while the second is the profile of the virtual temperature T_v of the environment between the level of free convection and the level of neutral buoyancy. Thus, CAPE does not depend on the environmental humidity profile in the free troposphere, except through its effect on T_v. Also, because CAPE is a vertically integrated quantity, it does not depend strongly on wave perturbations that produce vertical oscillations in temperature, such as the the $n \geq 2$ gravity wave modes. Given these points, it seems erroneous for the authors to claim on page 18341 (lines 5-10) that the differences in environmental humidity of the free troposphere between the Offshore and NO-Offshore days "correspond to substantially larger CAPE during Offshore days (∼2100 J/kg) compared to NO-Offshore days (∼1400 J/kg)". My guess, instead, is that the change in CAPE is due mainly to increased moisture at the surface. Also, later on, the authors seem to infer that the cause of the simulated increase in CAPE offshore that precedes the squall line's passage is due to the effects of temperature perturbations associated with the n=3 mode, even though this mode alone should have only a marginal effect on CAPE, due to commensurate warming aloft. Instead, it seems more likely that this mode is acting primarily to reduce the convective inhibition, which has been shown by Tulich and Mapes (2010) to depend on the temperature and moisture profile in the lower free troposphere below roughly 4 km. I'm not sure how to test for the relative importance of changes in CAPE vs convective inhibition, but perhaps the authors could at least examine in more detail the causes of the simulated changes in CAPE.

ACPD
15, C5682–C5688, 2015

Reference: Tulich, S N., and B. E. Mapes, 2010: Transient environmental sensitivities of explicitly simulated tropical convection. *J. Atmos. Sci.*, 67, 923–940.

List of minor concerns:

1) Page 18331, Lines 24–26: The approach of one way nesting, along with the positioning of the outermost domain (d01), seems a little strange to me. In particular, why is d01 not centered on d02? Also, why not just use a single convection-permitting domain for the re-initialized runs, with ERA-interim data used to prescribe the lateral boundary conditions, i.e., what is the benefit of having the outermost (12-km) domain in these runs?

2) Page 18334, Line 13: "Specifically, the mean diurnal cycle is constructed by averaging all values at a particular time of day and the mean is constructed by a series of such averages". Do the authors mean *local* time of day?

3) Page 18335, Lines 4–5: "The observed total rainfall and the mean daily rainfall rate over New Guinea...are presented in Fig. 2a and c". Are these two fields (total rainfall and daily rainfall rate) identical except for their units? If so, then showing only one of them would seem to be sufficient. If not, then the differences between them would seem to be quite subtle and are never actually mentioned in the text, so what is the point of showing them both?

4) Page 18335, Line 26: "The excessive rainfall over the slopes is partly due to the horizontal grid spacing". This seems like an overly confident statement, given that the authors show later on how this overproduction of rainfall is not mitigated even when going to 1.33-km grid spacing.

5) Page 18337: In the discussion of Fig. 4, I did not find any mention of what appears to be a rainfall disturbance propagating from the sea to the mountains in the late morning and afternoon. As indicated in my Fig. 2, which is another annotated version of the paper's Figs. 4c–f, this disturbance has a propagation speed of roughly 3 m/s and is

ACPD

15, C5682–C5688, 2015

apparent in the observations, as well as in all of the model runs. Can the authors provide some discussion on their thoughts about this robust feature?

6) Page 18339, Lines 1–4: "Comparison of the two-week rainfall accumulations over the area of d03, on each model's native grid, demonstrates notable similarity between the two resolutions (Fig. 5). Both model resolutions show similar rainfall accumulations over the slopes of New Guinea, both in terms of intensity and area." Isn't this similarity to be expected perhaps, given that area averaged rainfall must be constrained by the large-scale moisture budget, which, in turn, is strongly constrained by the prescribed lateral flux of moisture at the boundaries of d03? Would the authors expect similar results even with a two-way nesting approach?

7) Page 18348: It might be worth mentioning in closing that this paper points to a pressing need for more detailed observations of the diurnal cycle of convection over the Maritime Continent region, given the uncertainty surrounding the observed vs simulated diurnal evolution of convection shown in Fig. 4. Perhaps, these observations will be forthcoming in the near future with the planned field campaigns over the Maritime Continent.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 18327, 2015.

ACPD

15, C5682–C5688, 2015

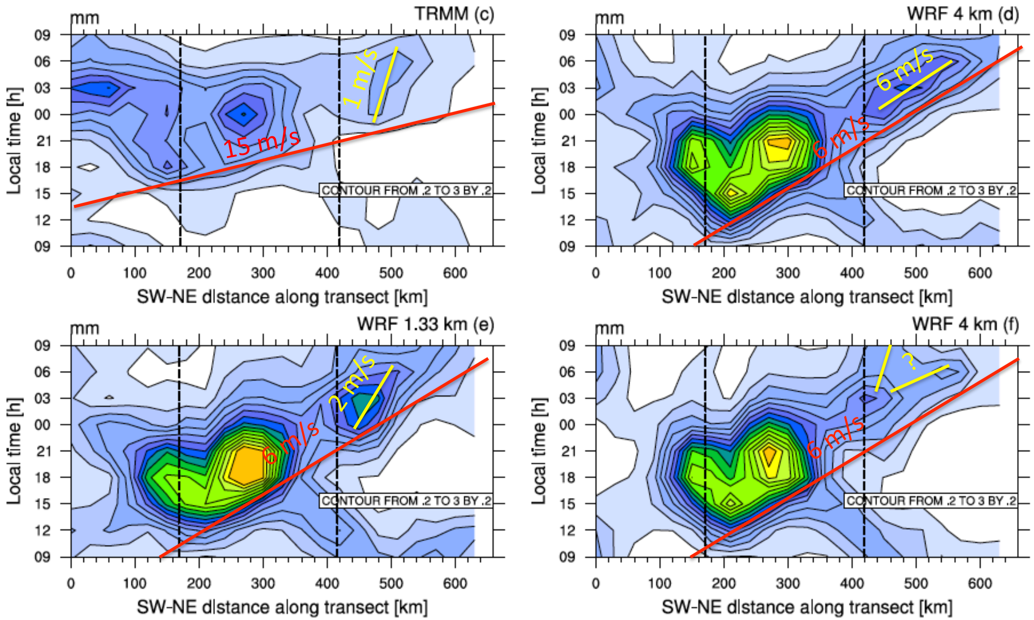

Fig 1. My annotated version of the paper's Figs. 4c-f.

Fig. 1.

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ACPD
15, C5682–C5688, 2015

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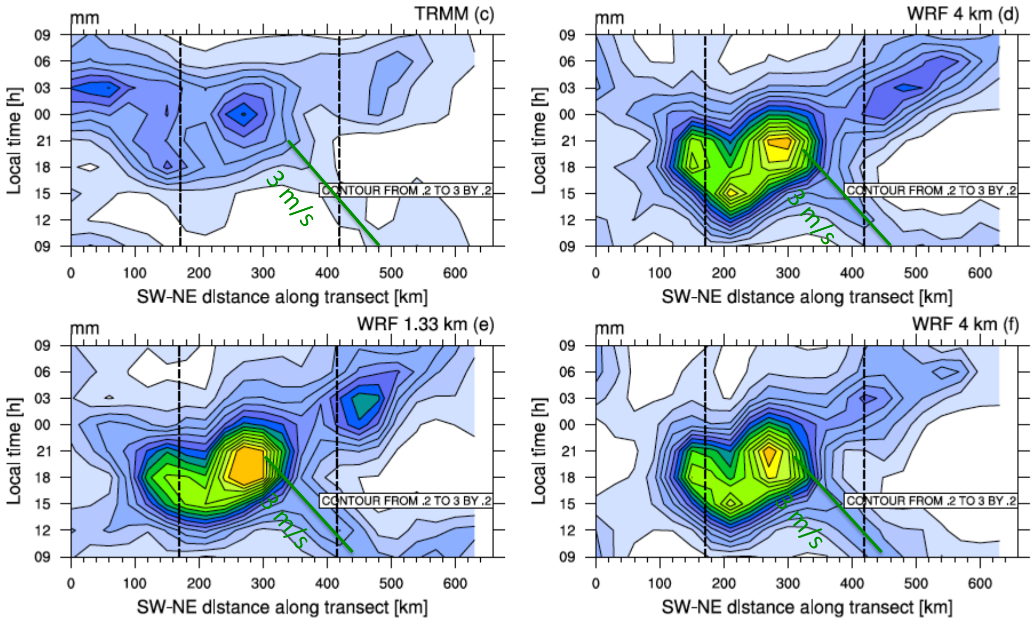

Fig 2. My annotated version of the paper's Figs. 4c-f.

Fig. 2.

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