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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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Authors' responses are in bold.

Referee's Summary:

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

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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 ($\sim 5-7$ 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.

A similar comment on the offshore signal was also raised by Anonymous Referee 2, but with respect to Fig. 4a and 4b, where he notes that TRMM (Fig. 4a) appears to have a faster offshore signal than WRF (Fig. 4b). Note that Figs. 4c-f show the composite diurnal cycle, i.e., averaged over the entire month and across all the transect lines seen in Fig. 1a of manuscript. Compositing the diurnal cycle over the whole month could reduce the offshore propagating speed in the mean signal seen from TRMM in 4c, especially since WRF tends to over-simulate the offshore events.

In any case, estimated propagation speeds do depend on the choice of phase lines drawn. In Figure 1 here (Figs. 4c-f in revised text), we present what we

think are more appropriate choices for phase lines drawn through the rainfall peak signals over land (black) and over water (yellow) and for the broader envelope of convection (red). We don't feel that drawing a line between the peak over land and peak over the ocean properly represents the propagation speed of the systems – the offshore and onshore convection are parts of different morphologies. In our assessment, estimated phase speeds of the rain signals do not differ much between TRMM and WRF. They also show that the morphology of simulated squalls is indeed robust across the two model resolutions (similar phase speeds, though slightly faster offshore in WRF 4km), as supported by Figure 2 here also. Due to likely timing/intensity bias over terrain, the peak rainfall signal from TRMM moves slightly slower than WRF over land. The faster phase speed for the peak rain signal offshore from WRF is possibly due to there being more simulated squalls in the later half of the month. Arguably, the coarser resolution (in both time and space) of the TRMM product could also erroneously imply a slightly faster propagation speed for the broader convective envelope.

The purpose of Figs. 4c-f was mainly to show that WRF, on average, was able to capture the gross features of the diurnal cycle seen from TRMM observations, including the propagation signals. We acknowledge that propagation speed differences between TRMM and WRF could be the result of simulation bias, perhaps related to errors in the gravity wave characteristics and/or convection. However, it is more likely a combination of timing/intensity bias from the observations, simulation bias and the effect of coarse-graining the model output. We note these possibilities in the revised text.

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?

Based on the phase lines drawn in Fig. 1 here, the estimated speeds of the broader "envelope" of convection in TRMM and WRF are roughly similar (only slightly faster in TRMM for reasons discussed above). They are also slower than an $n = 3$ gravity wave mode. Rather, the speeds are reminiscent of the "gust front" mode identified by the author in his study (Tulich and Mapes, 2010, Multiscale convective wave disturbances in the Tropics: Insights from a two-dimensional cloud-resolving model, J. Atmos. Sci., 65, 140-155, 10.1175/2007JAS2353.1)

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.

Vincent and Lane (2015, submitted to Monthly Weather Review) have compared CMOPRH, TRMM and rain gauge data over New Guinea. In those comparisons, TRMM actually performs better than CMORPH relative to gauges. Examining other useful satellite products as the Referee suggests will continue to form part of our ongoing research.

Reference: Vincent, C. L. And T. P. Lane, 2015: Evolution of the diurnal precipitation cycle with the passage of a Madden-Julian Oscillation event through the Maritime Continent (submitted to Monthly Weather Review)

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 Tv 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 Tv. 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.

The Referee is correct to state that CAPE depends only on the surface/mixed-layer temperature and moisture, and the environmental virtual temperature. Yet,

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the ability to support deep convection is predicated by the amount of free tropospheric moisture. It is clear that there is considerably less mid-level moisture on NO-Offshore days compared to Offshore days. Thus, on NO-Offshore days, the coastal/offshore environment is less likely to support deep convection at night despite having moderate CAPE, which is what we see in the simulations.

While we note that mean surface moisture between Offshore and NO-Offshore days are similar (as pointed out by Referee 2), the diurnal development (and thus changes) in CAPE near the coast on Offshore days (Fig. 12 of manuscript) does correspond to the diurnal evolution of mass (moisture) convergence in the boundary layer averaged for the northern coast region (Fig. 8 of manuscript). CAPE builds up near the coast from 1500 LT to midnight and gets destroyed later as the squall moves offshore. We have thus revised the sentence to read "Substantially larger CAPE occurs during Offshore days (~ 2100 J/kg) than during NO-Offshore days (~ 1400 J/kg) due to the increased boundary layer moisture convergence on Offshore days between 1500 LT to midnight (Fig. 8)".

Changes in simulated CAPE due increased moisture convergence appears to be more important than changes in simulated CIN as relatively similar CIN features are seen between Offshore and NO-Offshore days for the northern coast/offshore region (Fig. 3 here). The Reviewer is correct about the relative roles of the $n > 2$ modes for affecting CAPE and CIN in idealized scenarios. However, in these realistic scenarios the lower altitude cold temperature perturbations are larger than those in the upper troposphere at 03 UTC (e.g., Fig. 12) , i.e., they are not purely sinusoidal waves. This property of the waves does explain the strong increases in CAPE seen offshore.

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

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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?

The choice of domain d02 being positioned off-center with respect to the outermost domain d01 was purely to save computational cost, given the focus is over New Guinea and the northern region of Australia.

One-way nesting was used so that independent model solutions at different grid resolutions could be generated and compared.

The ERA-Interim data used as lateral boundary conditions are at $0.75^{\circ} \times 0.75^{\circ}$ or ~ 80 km . The maximum recommended nesting ratio for WRF is 7:1, with the ideal being 3:1. Hence, the 12 km outer domain allowed ERA-Interim data to be dynamically downscaled to the recommended resolution for use as corresponding boundary flow fields for the nested 4 km inner domain.

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?

Yes. The technique can also be applied to UTC, since local time is just an integer value offset of UTC.

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?

Plots showing mean daily rainfall rate is now removed in the revised manuscript.

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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.

Arguably, 1.33-km horizontal grid spacing is still inadequate to resolve entrainment processes, which actually require large-eddy-simulation type resolutions. Furthermore, better resolved slopes (i.e., a more peaked topography at 1.33 km) would induce stronger upslope winds, enhancing updrafts and resulting in more rainfall over the slopes.

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 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?

The rainfall disturbance seen propagating from the sea to the mountains in the late morning refers to onshore convection that develops and slowly migrates inland following the penetration of sea breeze. This robust diurnal feature is characteristic of coastal environments around the islands of the Maritime Continent and has been documented in previous studies. Text describing this has been added to the relevant paragraph in the revised manuscript.

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

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lateral flux of moisture at the boundaries of d03? Would the authors expect similar results even with a two-way nesting approach?

Yes, such similarity is to be expected as the Referee noted. We surmise that a similar set of results would also occur 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.

The Referee has raised a valid issue, which we mention in the revised manuscript. We hope our results/analysis aid in the design of the observational network for the proposed Years of Maritime Continent field campaign in 2017-2018 (e.g., the locations of radiosonde launch sites or ground-based doppler radar in order to observe in more detail the phenomena captured by the simulations). The revised text now includes the following statements: "Clearly, understanding these systems would benefit from increased observations. For example, radiosonde launch sites located about 150 km offshore would be highly useful. In addition, radars at coastal sites and/or on ships could also observe the passage and structure of convective systems from land through to ocean. Such observations should form part of future field campaigns like the YMC (Years of Maritime Continent)"

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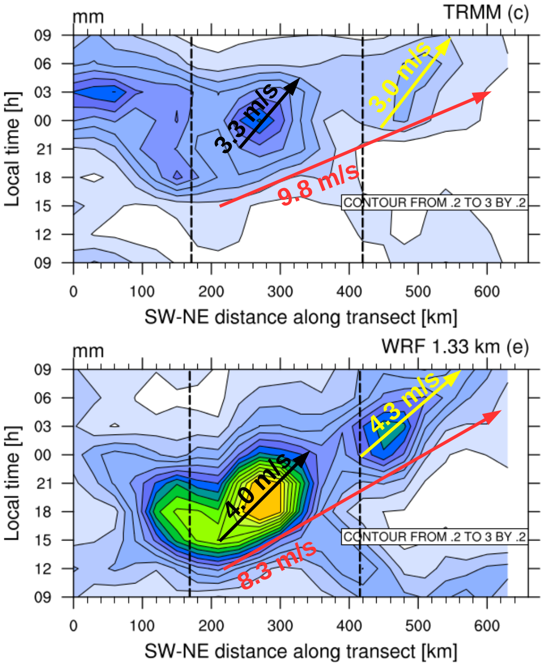

Fig. 1. As per Figs. 4c-f in manuscript, but with phase lines and estimated propagation speeds drawn for peak rain signals over land (black), over water (yellow) and for the broader convective envelope (red).

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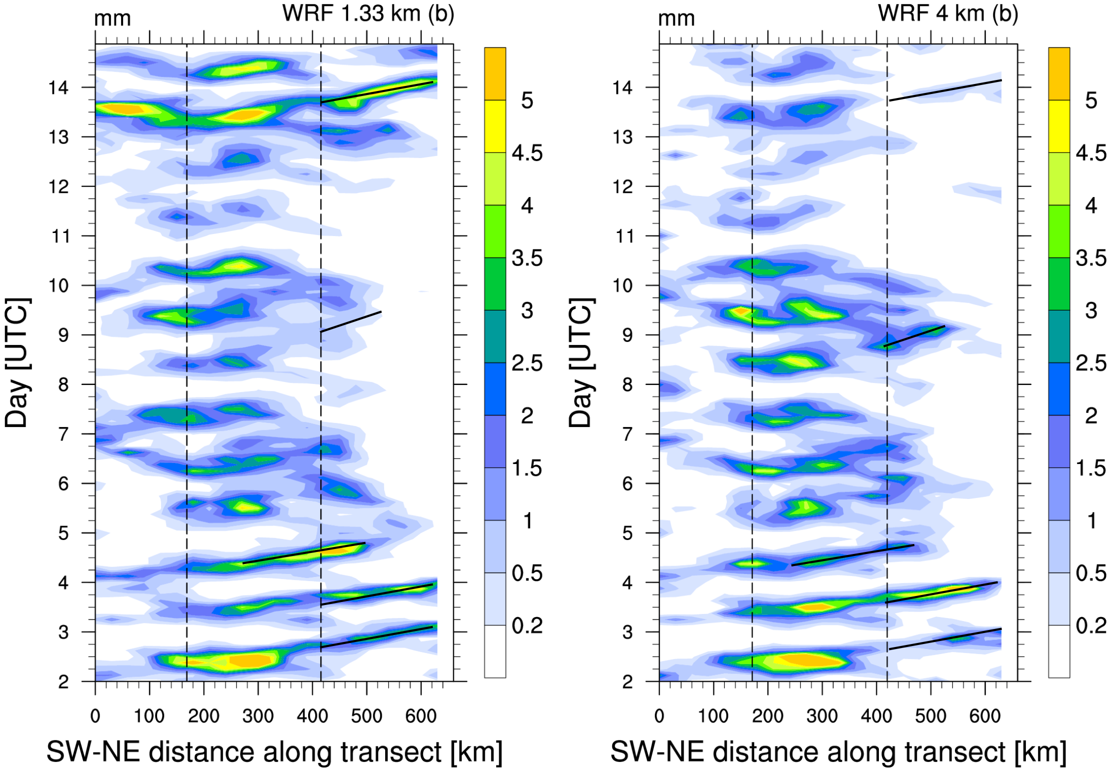

Fig. 2. As per Fig. 4a and 4b of manuscript but for WRF at 1.33km (left) and 4 km (right), respectively. These have been coarse-grained to match the TRMM resolution.

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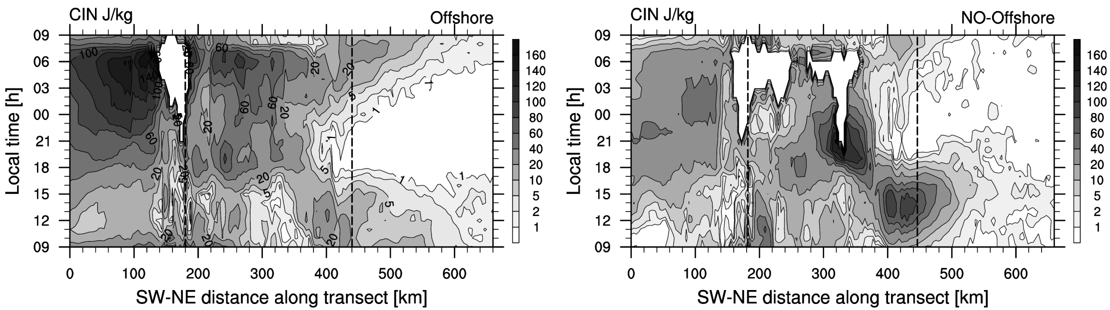

Fig. 3. Mean diurnal evolution of simulated convective inhibition (CIN) for Offshore (left) and NO-Offshore (right) days, averaged across the transects in Fig. 1 of manuscript.