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Atmospheric Chemistry and Physics
Discussions

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' reply to comments are in bold

Referee's Summary:

This study examines the rainfall over New Guinea during the suppressed conditions based on a series of convection-permitting numerical simulations. The authors show that the WRF model simulates well the diurnal variations of precipitation in comparison with satellite observed rainfall, and reproduce the occurrence and variability of off-shore propagating overnight convection north-east of New Guinea. It is also argued that its off-shore propagation is largely controlled by background conditions, and gravity wave plays a critical role in setting its propagation speed. I think the arguments are

compelling. Overall, the topic of the paper is very important and suitable for ACP. The manuscript is also very well presented. I have a number of specific comments. I recommend publish the manuscript after minor revision.

We wish to point out that the cool anomalies associated with the gravity wave generated from precipitating land convection do not appear to initiate convection or control its propagation (as stated in the abstract). It does not set the offshore propagation speed near the coast (as the Referee summarises). Rather, we argue that it contributes to the system's longevity and maintenance by destabilising the coastal/offshore environment prior to the arrival of the squall line.

Specific comments:

First paragraph, page 11: There is a subtle difference in the phase speed in convective system between TRMM and simulation. The time-distance diagram shows that that TRMM gives faster propagation speed. This may be due to the timing bias in TRMM over land as discussed in Page 10, but could also be simulation bias. Some further discussions may be used here.

The subtle difference could also be simulation bias, perhaps related to biases in the characteristics of the gravity waves and/or convection, as the Referee suggests. The difference seen is therefore likely a combination of timing bias, simulation bias and possibly the effect of coarse-graining model output to match the 3B42 product, both spatially and temporally. The relatively coarser time resolution of TRMM observations could also erroneously imply a faster phase speed in the gridded product, which is an unavoidable consequence of using these data unfortunately. We note these possibilities in the revised text.

Last paragraph, page 6: what is the lower boundary condition over sea? Is it time varying SST or SST at the initial time? This is important since the model domain cover a large area of sea. If the SST is kept constant over time, long simulations would very likely fail because of misrepresentation of the surface conditions. A recent study (Wang, S., A. H. Sobel, F. Zhang, Y. Q. Sun, Y. Yue, and L. Zhou, 2015: Regional Simulation of the October and November MJO Events Observed during the CINDY/DYNAMO Field Campaign at Gray Zone Resolution. J. Climate, 28, 2097–2119. doi: http://dx.doi.org/10.1175/JCLI-D-14-00294.1) has demonstrated this for regional simulations of several weeks.

SSTs for all domains are prescribed every 6h from ERA-Interim data as mentioned in text (Section 2.1)

First paragraph of Page 15: The red box in Figure 7 is different from the region being analyzed. Is it chosen for convenience?

The region analyzed in this paragraph refers to the northern coastal area of New Guinea, which is the area enclosed by the red box in Fig. 7a. This is where clear differences in offshore rainfall are seen between the composite cases, shown in Fig. 6. The paragraph has been edited to make the reference to the northern coastal area clearer.

line 12-13, Page 14: Fig. 12b is a remake of Fig. 6a. Why not also mark the rainfall onset the on Fig. 6a?

Rainfall onset is now marked on Fig. 6A in the revised manuscript.

Line 21, Page 14: delete the last word "does"

The extra "does" has been deleted.

Line 9, page 15: is the CAPE calculation based on pseudo-adiabatic thermodynamics or reversible thermodynamics?

CAPE calculation is pseudo-adiabatic, with ice processes. The code used is courtesy of George Bryan (NCAR) from http://www2.mmm.ucar.edu/people/bryan/Code/getcape.F

2nd paragraph, page 15: moisture convergence is computed within the first 1 km,

which is approximately the boundary layer convergence. Figure 7 shows that moisture content within this shallow layer is similar in No-Offshore and Offshore days. So the difference in VIMFC defined in equation (1) can only be attributed to difference in boundary layer convergence (instead of difference in moisture content).

Yes, mass convergence in the first 1 km of the boundary layer mostly explains the difference in VIMFC. This point is now reiterated in the revised text.

Line 11-12, page 15: this description of the wind speed is not very accurate. Low level wind normal to coast actually shows quite some variations - it varies from -3 m/s at 1 km to 4 m/s at 3 km. This is comparable to the shear strength at upper levels except that the shear layer is much shallower.

Low-level wind speed below 500m is similar between the 2 cases. Vertical shear between 1-3 km is also very similar with shear values of $0.0025\,s^{-1}$ and $0.00235\,s^{-1}$ for NO-Offshore and Offshore days, respectively. The description has been made clearer in the revised text.

Line 21-23, Page 16: It's not clear what difference between Offshore and No-Offshore specifically is discussed.

The sentence has been changed to read "differences in background environmental conditions near the northern coast..."

Line 27, Page 16: The steering has not been discussed before. It's unclear winds at what levels steer the convective systems. Suggest clarify or remove this.

The phrase "steered by the mean wind" has been removed.

Line 10-12 of Page 18: The dry simulation (top right panel in Fig. 11) shows the signature of $n = 3$ wave, but it's not easy to see the three antinodes in the moist runs at 15 LT.

One can see 3 antinodes in the Offshore and NO-Offshore panels, keeping in

mind that the depth of the troposphere extends to about 15 km. The is because the vertical wavelength, λ_Z, is related to the depth of the troposphere, Z_T, by $\lambda_Z = \frac{2Z_T}{n}$, where n is the mode number. By definition, the wavelength is also twice the distance between adjacent antinodes. The distance between antinodes at the 650-km mark in the Offshore panel is approximately 5 km. Thus, while it is a gravity wave with a 10 km vertical wavelength, we note that it is not a pure sinusodial wave since the lower half-wavelength has the strongest amplitude. Arguably, what the simulated convection is really setting is a strong 5-km deep cold anomaly near the surface with weaker alternating sign anomalies aloft.

Line 14 of Page 18: "sub-cloud" is confusing. Typical depth of "sub-cloud" layer is only a few hundred meters above the surface. I guess the manuscript actually means below the thick stratiform clouds.

What we mean is depth of precipitation below the melting level. In Fig. 11, this can be inferred by the height of the 0.05 g/kg rain mixing ratio drawn by the line contour (4-5 km). This has been clarified in the revised text.

Figure 11: Cloud layer in the No-Offshore days is substantially shallower than the Offshore days at distance greater than 240 km at all the three times shown in Fig 11. Thus, it appears that the free-troposphere dry conditions in the No-Offshore days (as shown in Fig. 7a) greatly suppress deep convection.

Yes. This is especially true for the region near the northern coast. We agree with the Referee's comment here and it has been incorporated into text.

Line 8-11, Page 20: Fig. 12d actually shows there is hardly any propagation of temperature anomaly over land (200 - 450 km) as it is in phase with diurnal cycles, while its propagation over sea (from the bluish area 450- 590 km, 21pm-6am) is slightly faster than 8 m/s. Also from Fig. 12 c and d, it seems that the propagation speed of the temperature anomaly is similar despite the difference in amplitude.

The apparent non-propagation of temperature anomaly over land in Fig. 12d, as compared to Fig. 12c, suggests differences in the source of the heating/cooling. In the moist simulations, the source of the anomaly is primarily diabatic forcing from the convection, which is located further inland. In the dry simulation, the source is essentially the boundary layer heating/cooling, which extends to the coast.

With regards to the offshore anomaly, the aspect ratio of the plots makes the propagation speeds appear similar. However, the propagation speed in the dry simulation is indeed (slightly) slower, consistent with the shallower cold anomaly (shorter gravity wave vertical wavelength) seen in Fig. 11.

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