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Comment

Interactive comment on “A numerical study of the early stages of a tropical cyclogenesis in relation to the MJO” by J. Guerbette et al.

Anonymous Referee #2

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This paper is clearly written and the methodology and execution seem sound enough. It is superficial, though. In my view it does not make a significant contribution to the field, because the analysis lacks enough depth to teach a knowledgeable reader anything new.

The paper is about a simulation of the development of a tropical cyclone. It would be normal to compare the results against observations. The only comparison performed is for a single snapshot, against scatterometer data, in fig. 2. Why are track and intensity predictions from the model not compared against other available observations? For example, best-track estimates from the RSMC? The authors say (p. 4924) that no comparison is possible for Nov. 20-29 because of lack of OSCAT data but the RSMC best track estimates are surely reasonable for this purpose. Other model fields, such

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as cloud-top temperatures, could also be compared to satellite observations.

The central result, in fig. 5, appears to be the relationship between barotropic conversion and intensification. This result does not seem so impressive to me. There is some correspondence before the 28th, but none after that. I am not saying that there is no relationship between barotropic conversion and intensification, but why focus on this process when it is at most just part of the story? Why not do a more substantive analysis of the factors controlling intensity? We do not see, for example, any plots showing the evolution of the vortex structure, or the development or structure of convection in the model, the baroclinic processes, surface fluxes, moisture fields etc.

How robust are the simulation results to the normal uncertainties? Would the results differ if the initial conditions were slightly perturbed? What if the simulation is just started a day or two earlier or later?

In short this is not an outright bad paper, but it's a superficial one. A single simulation is performed and analyzed in a competent, but cursory way. The reader does not learn much about tropical cyclogenesis, and certainly not about the MJO. Thus I have recommended rejection, applying the standard I would use for other top journals in the field. Because the work is competent, it could be accepted with minor revision if the journal wishes to apply a lower standard.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 4919, 2015.

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