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Weather and Climate Dynamics Discussions

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Interactive comment

# *Interactive comment on* "How Well do Models Represent the Development of Extra-Tropical Cyclones? Evaluation of Two General Circulation Models Against NAWDEX IOP 6 Observations" *by* David L. A. Flack et al.

**Anonymous Referee #2**

Received and published: 10 November 2020

My summary: This manuscript examines the ability of GCMs to capture extratropical cyclone development and intensification using a case-study. The manuscript considers two separate models and runs each at two resolutions. A diagnostic based on the quasi-geostrophic omega equation is utilized to separate dynamic and diabatic contributions to vertical motion. The model results are compared with observations.

Recommendation: I recommend accept conditional on minor revisions. I find the paper to be well written and relatively easy to read. I appreciate the new diagnostics and the comparison with observations. There are a few details about the technical set up of



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Discussion paper

analysis that need to be explained better (as line-by-line comments for details). There is an issue with the GQ omega that also needs more explanation.

Line 102: Possible verb tense disagreement for the word “interacts”. I think you are talking about multiple PV anomalies, so it should be interact.

Line 104: For the footnote, there is a typo: the word is should follow the phi symbol.

Line 106: Placing a comma between trough and cyclonic would make the sentence easier to read.

Line 109-110: Wouldn’t Gwendal say that the cyclone participated in the NAO regime transition rather than saying it “occurred during”? I say this in jest, I’m sure you will address this later on in the manuscript.

Line 153: You write: “For both models hindcasts are initiated at 0000 UTC on 27–29 September, and 1–2 October 2016 ..”

I don’t understand the logic of the choices of two separate initialization times? Or to put it differently, why is there no initiation at 00 UTC on Sept 30? Also, those initiated on Oct 2 will have already had the genesis in the IC, I think. So what are they used for?

Also, you state on line 162 that hindcasts initiatied on Sep 27 and 28 at low resolution did not produce the cyclone. So I’m confused as to why these dates are included in the sentence on line 153. I acknowledge that I may be misunderstanding something you’ve said.

Line 212: Typo on the word ClouSat - missing the d.

Line 231: How interesting that the time-average of the static stability works in this calculation.

Line 247: What are the horizontal boundary in a global model? I suppose you choose some appropriate distance?

Line 255: You write: “Inversion of the two previous equations allows to separate “ Sounds a bit awkward. Maybe there is a word missing between allows and to?

Line 257: What do you mean by “Vertical velocity intervenes . . ..” I don’t follow what you mean by intervenes in this context.

Line 286: This 24-hour delay in deepening seems to run contrary to your statement on Line 217, where you wrote:

“In reality, for the hindcast that is compared against the observations in this study (initiated at 00 UTC 1 October 2016) neither timing nor positioning adjustments are required. “

I assume that this delay in the intensification that you discuss on Line 286 is in a hindcast initialized earlier than the Oct 1 date, but it would be good to include more clarity about this, either here, or in/near the sentence on Line 217.

Line 319-330. One thing that the table makes clear, that I don’t think is mentioned: the HR models generate a stronger DRV than ECMWF. This should be stated in the text I think.

Line 325: Figure 4 is a something of a proof of concept for your dynamic/diabatic separation. Though it is might be worthwhile to mention that the dynamics and diabatic component are tightly coupled.

Line 332: Section 4.2.2. How interesting that the LR model is more different from the HR model for the upper-level disturbance. We tend to think of diabatic forcing related to moisture parameterizations being the issue, but here that is not the case. Does this suggest that region in which the dynamics and diabatic components must interact are more sensitive to the role of resolution? Or is this an initial value problem? Or is this unique to this case-study. Alas, that is not something you can easily answer I imagine. Interesting to think – no need to provide a detailed response to this comment however.

Line 359: Section 4.2.3 Regarding the statement about these biases being the cause for the changes in track path, are you sure you can attribute it to this mechanism? If so, how?

Line 375: You write: “The averaged quasi-geostrophic baroclinic conversion is roughly reduced by two thirds in magnitude compared to that directly calculated from the model $\omega$ but is consistent . . .”

That is quite a reduction. This is the only thing that I've read (so far) in this manuscript that makes me pause and wonder, should we be so confident in their diagnostics? There is not much discussion. Are you hoping to ignore it so that others do as well, or is there good reason to be unbothered by this difference? What is the difference between the Q-G baroclinic conversion and that using modeled omega for the cyclogenesis phase?

Line 415: Figure 9 What do these plots look like for modeled omega?

Line 487: Figure 12 is a bit disheartening isn't it? As you say, ARPEGE-LR looks the most like the observations. But the HR models more closely match the dynamics of reanalysis. So what does this mean? Does the CFAD have no relationship with the dynamics of the cyclone? Or should we raise more questions about the dynamics in reanalysis? It is a tough figure to interpret, but I am glad that you include it in the manuscript.

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