Review of “A global analysis of the dry-dynamic forcing during cyclone growth and propagation” by Besson et al.

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This manuscript investigates how dry dynamical factors influence the intensification of extra-tropical cyclones and their propagation direction. The analysis is based on 38 years of ERA-Interim reanalysis and focuses on the extended cold season. The analysis is novel and even if some results are as expected, this manuscript is a valuable contribution to the field. However, there are two major issues with the manuscript which need to be carefully considered before this manuscript can be accepted. First, a theoretical background and justification of the two variables is lacking (major points 1) as is a clear explanation of all diagnostics and how they were computed (major point 2, minor point 3). Secondly, I am concerned about how some of these results may depend on subjective choices made in this analysis, namely the size of the bins in the phase space (major point 3) and on the decision to analyse all times between the time of genesis and minimum mean sea level pressure together (major point 4). In addition, minor comments – which also often refer to subjective choices made in the analysis - are also detailed below.

Major comments

1. The choice of the two variables (Eddy Growth Rate and the QG upper-level forcing) is not clearly explained / motivated. Why these two specific variables in the specific layers and not other variables / different layers? The manuscript also lacks an in-depth theoretical discussion about what these two variables really represent. In particular, it is stated that the Eddy Growth Rate represents the low-level baroclinicity, which is true, but the lower tropospheric stability (N) also has a large effect on the Eady Growth Rate. This aspect is not considered in the analysis and interpretation of the results. Lastly, how the two variables relate to each other is not considered either theoretically or in the analysis. It would be very interesting to see a map of how these two variables correlate with each other in a climatological sense (without the additional requirement of a cyclone being present). This could be shown in a third panel in Figure S2.
2. Related to major point 1, additional details should be presented in this manuscript concerning how these diagnostics were calculated rather than just referring to Graf et al (2017). It is not clear over which layers the Eady Growth Rate is calculated – in line 86 it is stated that the Eddy Growth Rate is “representative for low-to-mid tropospheric levels”. Additional aspects that need to be considered are: are the vertical derivatives calculated taking just two pressure levels? How is the static stability in the omega equation calculated (often this is taken to be a global constant)? How is the Brunt-Väisälä frequency calculated? - the equation is given in terms of height not pressure. Is model level data from ERA-Interim used (as suggested in line 80) or is it pressure level data?
3. Line 144 / Figure S1 / Lines 160-161. Selection of the number and width of bins in the 2D histogram. What is the justification for using linearly spaced bins? Would the results differ if the bins were designed so that each bin at approximately the same number of samples present? This is potentially a critical problem in this analysis and needs to be investigated. In

the current format, the 4 forcing categories defined in ~Line 160 and used for much of the analysis have hugely different number of data points (Figure S1). For example, the 4 boxes in the top left (Q↑E↓) have 1675 data points whereas the 4 boxes in the bottom left (Q↓E↓) have 21,042 data points – more than 10 times as many.

4. Lines 135-136 and many of the results. It is stated that "All of the analysis in section 3 and 4 will be restricted to the phase with normalized times between -1 and 0". I agree it makes sense to focus on the time during which the cyclones are intensifying, however, I do not agree that considering all times between the time of genesis and time of minimum mean sea level pressure all together is a good decision. This is because cyclone structure, location (geographically and relative to the jet) and clearly intensify varies hugely during this time. Because of this, in most previous cyclone composite studies (e.g. Catto et al, 2010, Dacre et al, 2012, Flaounas et al, 2015) different offset times relative to the time of maximum intensity are considered separately. For example, how do Figures 4 and 5 change if only normalised times from e.g. -1 to -0.5 or from -0.5 to 0 are considered? Also, would the numbers of timesteps in each bin (e.g. Figure S1) change if the time period was split into two?

Minor comments

1. Line 100, The radius of 1000 km is somewhat arbitrary but I appreciate that some value needs to be selected. Some brief justification is necessary though e.g. has a similar radius been used by others? More importantly, it should be clearly stated what this radius is meant to represent - the size of the cyclone or the size of the area which can affect the subsequent evolution of the cyclone?
2. Line 138. The three geographic boxes. Are the results sensitive to the choice of these areas? The reason for this comment is that these areas are quite large and cover the central and end parts of the main storm tracks regions. Why not consider the start and end of the storm track regions separately as it is well known that cyclones with their genesis in the western North Atlantic differ from those with their genesis in the eastern North Atlantic.
3. Section 2.3 / Figure 1. The 12-hour change in SLP. This diagnostic is not clear to me. At first I thought this was the maximum 12 hour deepening rate (the text on line 147 caused this thought) but this cannot be correct given that it has positive and negative values in the histograms in Figure 1. Please can it be clarified what this is – this aspect caused me quite a lot of confusion throughout the manuscript.
4. Related to minor point 3, how does the 12-hour change in mean sea level pressure relate to the normalised time presented in lines ~125 – 135? e.g. How does this "real" time relate to normalised time?
5. Lines 154-155. Does this statement that none of the distributions is strongly skewed apply to all areas of the phase space e.g. what would the distributions look like for a point in the middle which has a much larger sample size? Potentially, the distributions become more Gaussian in the middle are are most extreme in the corners of the phase space?
6. Line 194 – Very minor comment. It is not clear (to me) what is meant by the Greenland Shelf – is this the land / ice mass of Greenland?

Figure comments

- Figure 2 caption. Can the months analysed be added here? It would help remind a reader that only the extended cold season is analysed.
- Figure 4 and 5. These are presented on a cyclone-centre relative grid in terms of longitude and latitude. How does the 1000 km radius used earlier relate to this lon / lat space? The physical distance in kilometres between 20 degrees of longitude decreases with increasing latitude. Is it valid to assume that 10 degrees in longitude or latitude is approximately 1000 km? If valid, could this assumption be added to the captions?
- Figure 4 and 5 – related to major points 3 and 4 above. The mean values are presented in these cyclone composites but how much variability is there within each of these composites? This variability may be large due to the different offset times considered all together here. Furthermore, the variability may differ considerably between the 4 classes given the difference in the number of samples in each class.
- Figure 7. What are the grey / blocked out areas in the bottom left and bottom right of this figure? Also the caption needs a capital letter for "additionally".
- Figure S2 has a odd map projection and is lacking longitude and latitude labels. Can this projection / figure style be changed to match the maps shown in the main manuscript?

References

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