Response to Reviewers for WCD-2020-43

Dear Editor,

We thank both reviewers for taking the time to review and comment upon our revised manuscript. Following the comments we have made some revisions to the manuscript.

We provide detailed responses below with our comments and changes in green italics.

Kind regards,

David Flack, Gwendal Riviere, Ionela Musat, Romain Roehrig, Sandrine Bony, Julien Delanoe, Quitterie Cazenave and Jacques Pelon

Referees' comments are in black and authors' answers in green

Response to Anonymous Referee #1

Thank you for addressing my concerns related to the presentation of the manuscript. The message of the paper is now clearer and the length has been reduced. Therefore, the reading of the paper was more enjoyable this time.

I recommend to publish this paper after one minor comment and a few small technical corrections have been performed.

Minor comment:

In Summary, line 424 you state that "Increasing the resolution does not increase the relative contribution of diabatic heating...", which I agree, based on Fig. 7. But based on Fig. 9 (i,j,k,l), I think that ω_Q increases with resolution. Still, in lines 293 and 425 you state that your results disagree with Willison et al. (2013) and Trzeciak et al. (2016) papers, but I find the fact that the ω_Q increases with resolution being quite consistent with Willison et al (2013) and Trzeciak et al. (2016) papers.

Can you please double-check the papers by Willison and Trzeciak, if they meant the increase of diabatic processes with the resolution, or the increase of the relative contribution of diabatic processes with the resolution. I think these two aspects are slightly different things, because also the dynamical parts (adiabatic processes) can increase with the resolution (and in fact does so in your results) and partly compensate for the effects of diabatic processes.

We agree that an increase in relative contribution is not the same as just an increase. Therefore, as suggested we have double-checked the Willison and Trzeciak papers as requested. After this checking we stand by our comments that the two papers emphasize an increase of the relative contribution of diabatic processes.

Willison et al (2013) computed eddy heat fluxes for low and high resolution runs and found that the contribution of the eddy heat fluxes coming from diabatically-induced winds increase with resolution: p 2246 "The area average of 950–250-hPa integrated geostrophic eddy heat flux over the maritime storm track increases 24.4% when simulated at 20-km grid spacing. The area-average heat flux by the diabatic winds (y 0 dia T 0) increases even more dramatically at 52.6%. A relatively larger increase in diabatic flux suggests that the enhancement of diabatic effects with increased-resolution results from a strong positive feedback".

Trzeciak et al (2016) also looked at the relative contribution of dynamical and diabatic terms in another equation, in the pressure tendency equation. Their conclusions emphasized the weight of the diabatic term relative to the dynamical one increases with resolution. They even found that some dynamical terms are larger in coarser resolution: p 3444: "The fact that in this case the lack of diabatic contributions is compensated by larger Dφ values is remarkable and point to a stronger upper-level control of the cyclogenesis, which can be sufficiently resolved by the coarse-resolution model. ... It almost appears as if the situation

was so prone to the development of an intense storm that different physical pathways exist that can lead to similar cyclonic developments and that the choice of pathways is resolution-dependent." In other words, they show that in coarser resolution runs, an increase of dynamical terms may happen that compensates the decrease of diabatic terms".

In our case, we found an increase of the peak values of both the dynamical and diabatic terms when the resolution is increased as seen in Fig. 9. However, when spatial averages are made such an increase is less visible because the higher values cover a smaller area in high-resolution runs and this may explain why in Fig.7 we do not see a net increase of the averaged fields from low to high-resolution runs.

To conclude, after checking the content of the two papers, we did not change the text of our paper.

Technical corrections:

P. 8, line 180 and 182. You use the word extratropical without dash and then with dash. Please select either of them and be consistent throughout the whole manuscript.

Thank you for spotting this, we now use the term extratropical (without dash) throughout the manuscript.

P. 9, line 207: "The difference occurs 18h into the hindcasts". I suggest change it to "The difference occurs 18 h after the start of the initialization" or something similar.

This has been changed to ''The difference occurs 18 h after the start of initialisation'' as suggested.

P. 11, line 243 and 251. Add space between "Figs." and the number.

Thank you for spotting this, spaces have now been added in the relevant locations.

P. 13, line 257 I'd remove the word "and", and add comma.

Thank you, this change has now been done.

Response to Anonymous Referee #2

Suggestion: minor revisions.

Lines 4,5: Sorry I missed this in the first round: what is the “c.”? Why not just write it out to avoid any confusion?

The “c.” refers to circa. However, to avoid further confusion we have switched to approximately.

Line 11: You might consider extending the other reviewer’s acronym suggestion to the abstract and simply use ARPEGE and LMDZ at lines 11, 14, 15, and 16. It will be clear what you are referring to even without a “hereafter” at line 3. But, I leave it up to you.

This decision has been made to be explicitly clear about the models (for many people ARPEGE on it’s own means something different to it being defined as the climate model configuration). Therefore, in the abstract we decided to keep the full length versions within the abstract.

Line 66, You write:
Q3 Are there any differences in the diabatic processes related to microphysical properties between the different models?

This sentence might need to be cleaned up. It could mean:

Are there any differences in the diabatic processes related to differences in microphysical properties between the two models?

Or it could mean:
Are there any differences between the two models’ diabatic processes that are related to microphysical properties?

Or maybe it is something else?

Thank you for pointing out this potential area of mis-interpretation. Your second phrasing of the question captures the intended meaning of the question so we have rephrased Q3 as such in the manuscript.

Figure 13: This schematic is ok. The upper-level dynamical forcing shapes leave me a bit puzzled, especially at step 4. But it’s your schematic, so you can decide how you’d like to keep it.

The shapes of the upper-level forcing were to represent the shape of the PV filament in steps 2 and 3, and in step 4 changed to represent the approximate shape of the geopotential height contours representing the successive troughs. The arrow at this stage represents the direction the troughs are moving in. To aid in interpretation (of step 4) we have clarified the figure caption. The figure caption now reads:

“A schematic of the Stalactite Cyclone. 0) the Mesoscale Convective System that initiates the Diabatic Rossby Vortex (1) that travels along the blue arrow. The northern precursor (2) with upper-level PV cut-off that moves towards the diabatic Rossby vortex and initiates a roll-up between the two precursors at cyclogenesis to create the Stalactite Cyclone (3). Explosive deepening occurs as a result of strong diabatic heating throughout the column and the interaction with a series of embedded upper level high PV regions (the upper-level forcing here is depicted in the form of successive troughs in geopotential height moving in the direction of the white arrow; 4). Flight observations (5) indicate that ice water content is underestimated and so could have impacts on the diabatic heating and evolution of the cyclone.”