

Weather Clim. Dynam. Discuss., referee comment RC2
<https://doi.org/10.5194/wcd-2021-38-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on wcd-2021-38

Anonymous Referee #2

Referee comment on "The impact of deep convection representation in a global atmospheric model on the warm conveyor belt and jet stream during NAWDEX IOP6" by Gwendal Rivière et al., Weather Clim. Dynam. Discuss.,
<https://doi.org/10.5194/wcd-2021-38-RC2>, 2021

General comments

This paper presents a comparison between three simulations of a mid-latitude storm using two different convection parametrisations and no parametrisation at all. The results are useful to understand the impact of the parametrisation formulation on the evolution of the upper tropospheric flow including the jet stream. While there is a school of thought that advocates the increase of resolution in numerical simulations to avoid the need of convective parametrisations altogether, the topic remains relevant as a more efficient use of resources might still come from improving lower resolution models by improving parametrisations (including convection). Therefore I believe this paper makes an important contribution to a topic that remains largely unexplored.

The article is clearly in scope for Weather and Climate Dynamics, well-structured and well-written. However, after reading the paper I was left wondering whether the inclusion of two parametrisations was necessary at all. From the results, it is clear that the main differences arise between B85 and NoConv and that PCMT exhibits an intermediate behaviour. Thus, I wonder whether a better approach would be to concentrate on the first two to highlight these differences without the distracting element from PCMT. Otherwise, I'd encourage the authors to motivate further the need to present the three simulations throughout. A second main comment is related to the clarification of large-scale heating and other contributions. I add more details in this regard (among others) in the specific comments below.

Given these comments that in my opinion require attention, I can recommend the paper for publication in WCD, but only after these comments have been addressed.

Specific comments

L194-203: Please, give more details on the centred finite-difference scheme used. For example, were these differences computed between two output times or online as the model was run? Please also add more detail on the meaning of large-scale heating (also relevant for L14 in the abstract) and how this is different from parametrised heating. Looking at the Supplementary figures I get the impression that what is referred to as large-scale heating is the heating due to the large-scale cloud parametrisation (or microphysics). Is this correct? It would be good to clarify this and the other terms in the paper.

L244-245: Regarding trajectories, sometimes differences are better appreciated if the trajectories are plotted with respect to time of maximum ascent so that they are all in similar ascending stages. Plotting as a function of actual time means that trajectories at different ascent stages are averaged together. For example, the enhanced increase and decrease of PV might be due to differences in timing.

L280-282: I'm not sure I agree with the comment on the large difference between the three runs. If differences between the three is to be maximised, I would have chosen 3 UTC 2 October. Another period that seems interesting is 9-18 UTC 2 October during which only NoConv exhibits strong ascent. I think this might be an example where presenting only B85 and Noconv would lead to clearer presentation of results (see General comments).

L316-318: Can the heating decomposition be explained further? Does the finite-difference heating include the parametrised heating? See also the comment to L194-203.

Technical corrections

L18-19: As written, the sentence could suggest that B85 is realistic for longer than the other simulations. A simple reshuffling of words would make it clearer: "... that simulation becomes less realistic than the other ones at forecast ranges beyond 1.5 days".

L72: Change 'resolved' for 'resolve'.

L240: '...slightly higher mean temperature...' How statistically significant is this result?

L261-262: I can clearly see the more important tightening between c and a,b, but I think this is less clear between a and b.

L263-264: Clarify that the ascent considered to classify and count the trajectories is instantaneous ascent. Is it centred at the time indicated?

L267-269: Is the dip in the number of moderately ascending trajectories in B85 between 1-3 UTC 3 October as important as the enhancement at 12 UTC 2 October? In my opinion the number of trajectories in B85 is consistently lower than in the other two simulations.

L300: '...significantly larger for B85...' The term 'significant' is usually restricted to denote statistical significance. Has this been tested in this case? Is there any way of adding error bars to the curves in Figure 2d? It might be worth normalising by number of trajectories as well (see comment to Figure 2d).

L332-335: I don't fully understand the explanation on the shift between the negative PV area and the trajectory positions, as the trajectories would include the advective effects by the non-divergent wind. Perhaps starting trajectories from the negative PV regions would be useful to clarify these results.

L360: I think 'boundary' would be a better term than 'limit' in this case.

L423: To make the conclusions self-contained, give more details on the cyclone here (name, dates).

L427: To make the conclusions self-contained and clearer, it would be worth including the questions here as well.

L428: 'When parameterized deep convection is inactive...' It might be worth rewriting. As it is the phrase could mean 'When a convection parametrisation is present, but is not being triggered...'.

L440: 'large-scale heating' This phrase might need to be modified in response to other comments and heating definitions (see e.g. comment to L194-203).

L454: Delete 'in B85' after 'more realistic'.

L456-458: These two sentences are slightly confusing and appear contradictory: Are the more active dynamics realistic, but too strong? Please rewrite.

L493-496: The last paragraph seems incomplete in the sense that it only gives very partial information on the contents of a future paper. In my opinion, it could be omitted.

Figure 2 and others: Use of green and red might not be ideal for colour-blind readers.

Figure 2d: While the total number of trajectories is similar, I wonder if it would be a fairer comparison to present this as a proportion of total number.

Figure 3: Do the bold solid and dashed lines need to be coloured? Black or similar would make them clearer.

Figure 6: One difficulty when comparing forward trajectories is that the number of trajectories at each location will depend on previous stages in their evolution. Perhaps it would be fairer to start the same trajectories at the point of interest and see their evolution towards and from that point in time.

Figure 8: What do the white areas represent? Are they just regions off the colour scale?

Figure 11: You could make the comparison between panels easier by adding lines indicating the span of panel (b) in panels (c-e).