Comment on wcd-2021-38

James Booth (Referee)

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-RC1, 2021

General comments:

I think this is an interesting study. The work is well written and thorough. It shows a pathway through which parameterized convection impacts ascending air in extratropical cyclones, and it shows that this can impact the forecast. It also affirms previous work showing that deep convection is occurring within the warm sector of the cyclones.

The one potential component that the authors might consider adding to the study is a graphical analysis of changes in the forecast that impact people, e.g., including detail about how surface conditions (winds, temperature or precipitation) differed for the forecasts from the different configurations.

At lines 465-466 you discuss the differences between PCMT and B85 being largely related to schemes' closures. I think this is a good point to make, and I think you might expand a bit on the discussion. One thing that I have started to consider (over the years) when thinking about these sorts of integrations is this:

In the runs with parameterized convection, it is likely the case that resolved convection still occurs some of the time.

Since the models all have the same resolution, I imagine that there are some times when both resolved and parameterized convection occur at the same time in the model integrations that have the large-scale convection parameterization turned on - perhaps in adjacent grid cells.

I wonder if PCMT more closely resembles NoConv (e.g., Figure 3) because the trigger mechanism for PCMT to activate is stricter than that of B85. (I dont know that it is, I am just speculating). This would mean that in PCMT we actually see the result of a combination of resolved and parameterized convection, whereas in B85 we see more influence from parameterized convection because the scheme is more likely to fire off before any resolved convection can take place. This might be something that you consider mentioning, or if you disagree, perhaps add some discussion stating your case.

Two other questions/comments, just for your consideration:

(1) For the sake of the objectives of this study, it is good that the circulation of the storms is not very different despite the different convection schemes. Presumably, the circulation in the tropics is much more sensitive to the switching out of the convection schemes. I am guessing that the short length of the integrations prohibits those differences in the tropics from impacting the storm dynamics significantly? Perhaps a comment on this would be useful to the reader, or maybe I am missing some detail about the model configurations.

(2) In multi-year integrations of a GCM, such a study is much more difficult. We tried it and found that when we changed the convection scheme we also had to re-tune the model. The changes to the tuning included changes to the microphysics which affected the storm dynamics.

Minor comments:

Lines 194-199: Here you discuss the methods of calculating the diabatic heating. Method 1: taking the derivatives of theta with respect to space and time. Method 2: using the actual diabatic heating tendencies created by the model. You state that method 1 is more accurate. This result surprises me, given that method two seems to have the exact information that you seek, as generated by the model. How do you calculate the accuracy of the two methods? Perhaps add a bit more explanation here - do you attribute the issue to the regridding, or are there other factors?

Line 231: For figure 1, the SLP contours look identical in the 3 integrations, is that the case? Perhaps my eyes deceive me and the SLP differ for the 3 panels?

Line 280: Just a comment: I am a bit surprised that the heating rates are positive even at the levels close to the surface, I expected rain evaporation to lead to negative values at the lowest levels. Perhaps that is only true in the stratiform precipitation regions.

Lines 284, 287: In these sentences, you refer to general areas as warm conveyor belt regions. So, is the terminology that you are using specifying a difference between WCB regions and WCB trajectories? I think this is a reasonable approach. But it might be useful for readers if you add some clarification in section 2.2.1, to specify that you are not exclusively using the word WCB to refer to a location where the air ascends by x amount in time t. Or maybe you do prefer this approach, in which case the phrasing at lines 284 and 287 is a bit confusing.

Lines 297-298: regarding these poleward locations with more diabatic heating in the runs with convection on, this is perhaps consistent with these models having more sustained ascent (as you mention on line 274). I wonder if might take this a step farther and say that the parameterized convection leads to a larger amount of poleward flux of heat? You don't necessarily need to investigate this - just an idea.

Lines 358- 370: I cannot tell from this paragraph whether you are of the opinion that B85 or NoConv more closely resembles ERA5 at this particular snapshot in time. For me, the pattern of PV at 25W by 60N in NoConv looks a bit more like ERA5 because it has a sharper meridional gradient, whereas B85 has a more of a NE to SW tilt in that region.

In lines 365-368, you discuss the 0.5-1.5 PVU ribbon in B85. As I see it, that feature is spatially continuous in B85 and therefore more distinct as compared to NoConv, and it is non-existent in ERA5. So, this also could be viewed as a region in which NoConv looks more like ERA5, perhaps.

Given the spatial resolution of the model however, one could question whether ERA5 is necessarily more correct, but if we assume that it is, then I feel that the issue of which is

more like ERA5 might a matter of opinion. All of which is to say, a bit more discussion here about which model integration you find to be most like reality would be useful I think. Or you can simply state what I said: it is a toss-up.

Lines 374-391: I appreciate that you point out times with B85 performs better (Fig 9), but other times when it performs worse (Fig 10)

Section 4.2: Figures 11 and 12 do well to compare with observations – always a challenge.
For me, it looks like the 3 integrations are all more similar to themselves than they are to observations. But perhaps not? I wonder if you can quantifiably check this with an RMS of the difference plots or something of that nature.

Line 485: If you don't mention it here, perhaps the in progress Wimmer et al paper will mention the idealized work of Boutle et al. 2011. Their Figure 4 tells an interesting story about convection, whether it is seen in the real world is to be determined.

Boutle IA, Belcher SE, Plant RS. 2011. Moisture transport in midlatitude cyclones. Q. J. R. Meteorol. Soc. 137: 360–373. DOI:10.1002/qj.783

Typos/technical issues:

Lines 93-94, you write: "This large-scale cyclone participated in the formation of a blocking over Scandinavia …"
The wording sounds strange to me. Should blocking be replaced by block? Or, leave the word blocking, but remove "a" that precedes it.

Lines 127-128, you write: "Surface oceanic fluxes are represented by the Belamari (2005)'s scheme …"
Remove the word the, or remove the apostrophe and the "s".

Lines 286-287, you write: "However, large differences between the three runs appear in the heating rates along the WCBs and not near the bent-back warm front."
Is the word "not" supposed to be in this sentence? If so, could you instead refer to the exact location of interest instead of saying not near the bent-back warm front?

Lines 338-339, you write:
"Besides as they go further away from the WCB outflow region winds become
more zonal and align more with the isentropic slopes as seen by comparing the orientation of the vectors with the slope of the 315 K isentropic surface near 36â□¦W, 300 hPa in Fig. 7a."
This sentence is a bit difficult to understand, and starting a sentence with Besides might be a bit too colloquial. But I leave to you to decide if you want to change it or leave it.

Lines 340-341: Verb tense issue, probably most easily fixed by changing "advection term is" to "advection terms are"