

## Response to RC 1 and RC2

Effects of Model Grid Spacing for Warm Conveyor Belt (WCB)  
Moisture Transport into the Upper Troposphere and Lower  
Stratosphere (UTLS)—Part I: Lagrangian Perspective

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### Reply to RC1: Major Comments

The current format of the Conclusions and Discussion section is heavier on the Discussion side and could use an extra paragraph of higher level conclusions. You list the main findings in 1-2 sentences (drier WCB outflow; more pronounced frozen-phase microphysics, stronger frozen precipitation, etc. for the higher resolution simulation) in the Abstract, and I was hoping to see a mirroring paragraph of these high level concluding remarks with some more detail also in the Conclusions and Discussion section. Currently, the rather detailed discussion/conclusion ends at line 600 and continues with placing the study into a larger context and the limitations paragraph. I would recommend adding a paragraph around line 600 that includes the key findings mentioned in the Abstract with some short elaborations. This would greatly benefit the readers who start from the Conclusions section and would be drawn in to read the full study.

*Thank you for the excellent suggestion. We have added a short paragraph in line 600 ff briefly summarising the key findings. Please refer to the new manuscript for the altered text (lines 616-625 in the revised manuscript).*

### Reply to RC1: Minor Comments

- The authors state that the current work is a follow-up study to Schwenk and Miltenberger (2024), pointing out the similarities in the methods and also partly in the results. The presentation of the research question and the hypothesis (the paragraph around line 110) also mentions the earlier study, and I recommend making this the first place where the reader is explicitly informed that the current study is a follow-up study.

*We have altered the text in that paragraph, so that it becomes clearer that this is a follow-up study. The new text can be found in lines 112-113 of the revised manuscript.*

- The end of the sentence on lines 128-129 and the beginning of the sentence on line 129 contain the same phrase: 'this study'. I recommend rephrasing these sentences to eliminate any confusion about which study or paper is being referred to.  
*The two sentence have been rephrased see line 131 of the revised manuscript.*
- The second criterion for the WCB trajectory selection algorithm ("... and second, the trajectories must lie within two specified longitude-latitude regions at two distinct times, determined using Eulerian cloud cover and sea-level pressure data from our simulation.") would benefit from an additional sentence or two that elaborates on how the selection of regions was based on the given Eulerian fields, as this appears to be different from Madonna et al. (2014) and Oertel et al. (2023).  
*The region was constrained manually by looking at different snapshots of sea-level pressure and cloud cover to avoid including trajectories that belong to adjacent MCs-type systems in the south-western part of the domain. This approach is not feasible in a climatological study like Madonna et al. (2014) and was not required in the study by Oertel et al. (2023) due to different meteorological conditions. We added a sentences in line 181ff to better reflect the subjective approach taken here.*
- Figures 2 and 4 might benefit from additional vertical lines that would indicate the mean and/or median values mentioned in the text for the corresponding figures.  
*We have added vertical lines to Figure 2. However, in Figure 4, there are enough histograms that adding vertical lines would lead to a very messy and difficult to interpret figure.*
- The sentence on line 282 seems to be missing a verb.  
*Changed to "... trajectories in the nested simulation ARE ON average 24 h old ..."*
- The figure caption for the Figure 5 subplots do not match the actual subplots.  
*Thank you for pointing this out, the caption was referencing an older version of the figure and we have corrected this.*
- The subsection 'Hydrometeor Content' in Section 3.2 contains an incorrect figure reference on line 339 and a non-existent figure reference ('Fig. 6g') on line 352.  
*Thank you for pointing this out, we have fixed the figure references.*
- Add the word 'trajectories' after the value '0.5%'. The values are very different; however, in the current wording, the 0.5% could refer to either trajectories or the PE value.  
*Thank you for pointing this out, we have reworded this sentence to make it more clear.*

## Reply to RC2: Specific comments

- P5, L149-151: The choice of two-way coupling introduces a significant challenge for attribution. Because the nested grid feeds back into the global domain, the “nested” setup will naturally diverge from the “global” control, eventually resulting in different synoptic states, in particular towards the end of the simulation period. Consequently, it is intricate to tell whether the reported differences are a direct result of increased resolution or simply a byproduct of this dynamical divergence. I would like the authors to clarify their rationale for this setup, as this could be avoided by employing a one-way coupling where the synoptical state in the global domain is identical between “nested” and “global” setup.

*Thanks for this insightful comment. We have chosen the two-way nested set-up to be able to capture the full WCB outflow in our simulation. The highest resolution nest contains mainly the ascent region of the WCB, which is where embedded convection will occur and where high-resolution will most likely make the largest difference. It is computationally too costly to cover the entire domain with this highest-resolution and likely physically not necessary. We therefore choose to have the WCB outflow in coarser resolution in a two-way nested configuration so we can investigate the impact of the convection-permitting physics on the WCB outflow. This would not be possible with a one-way nested configuration. Indeed the later may introduce numerical issues at the lateral boundaries, if the system is not fully encapsulated in the highest-resolution domain.*

*However, we agree that is mandatory to check how far the global and nested simulations have divergent in terms of the synoptic-scale meteorological features by the end of the simulation. Fig. 1 shows the mean sea-level pressure and the column integrated condensate content at the end of the two simulations (both in the global domain, i.e. the one with the largest grid spacing): The simulations are similar in the large-scale configurations with only minor shifts in the synoptic-scale pressure pattern across the North Atlantic and Europe. There are more pronounced differences in the cloud field, particular in the region of the WCB outflow and the cold-frontal cloud structure. However, we think this is mainly due to the difference in the representation of convection due to (i) the similarity of the synoptic-scale flow pattern and (ii) the fact that WCB outflow parcels will have started to rise about half-way throughout the simulation, where the simulations have diverged even less than seen in Fig. 1.*

*To more clearly motivate our choice of model configuration and the implications for attribution we have added a few sentences in the model description (see lines 156-159 and 170-175 in the revised manuscript).*

- A potential concern regarding the comparison between the “nested” and “global” simulations is that the trajectories are evaluated at different horizontal resolutions. Without a scale-aware framework, it is difficult to determine if the reported differences are due to resolving finer-scale or

merely methodological. To isolate the added value of the finer resolution, I recommend that the authors coarse-grain the nested trajectories by averaging them within the spatial footprint of the global grid boxes. If the discrepancies persist after this upscaling, they can be more confidently attributed to the non-linear effects of resolving finer-scale processes.

*It is true that the instantaneous values as seen along the trajectories are derived from Eulerian fields of different resolution. However, we think that the distribution of values still reflects systematic differences between the model simulation, because if the high-resolution simulation would only add "noise" one should see mainly a broadening of the distributions instead of the systematic shifts identified in our analysis. However, we agree with the reviewer that the resolution difference of the fields is one of the aspects of the Lagrangian analysis that limits the generality of the results obtained in the present analysis. To address these issues, the paper has a companion Part II paper, which investigates the WCB outflow properties in an Eulerian coordinate system (there we take care of coarse-graining the fields in an appropriate manner). The findings of the Eulerian analysis are similar to the ones reported in Part I, which corroborates our hypothesis that the systematic shifts in the distribution of WCB properties are not due to simple higher resolution of the fields in one of the simulations. The proposed method of coarse-graining trajectories after the simulation to the global resolution may introduce biases if the trajectories are not domain-filling (which they are not) as certain regions with e.g. convergent flow features may be overrepresented. Also trajectory output is only available every 30 min, which further limits the domain-filling nature of the trajectory data-set. Probably the correct way to avoid the impact of resolved finer-scale structures would be to coarse-grain the fields before computing the value of the trace variables, which however would need to be done during the simulation. This is technically challenging (not least because of the domain decomposition used in ICON for parallel computing) and not possible within the scope of the revisions of this manuscript. For the reasons detailed above, we refrain from coarse-graining the trajec-*

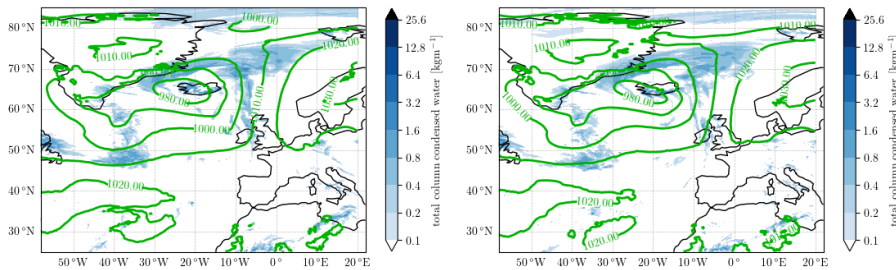


Figure 1: Mean sea-level pressure and column integrated condensate content at the end of the (a) global and (b) nested simulation, i.e. 28. 09. 2017 00 UTC.

*tory data-set in the manner suggested by the reviewer. However, we added a sentence alluding to the difference in resolution of finer-scale structures in one of the simulations in the discussions part of the paper (see lines 635ff).*

## Reply to RC2: Minor Remarks

- P11, L281: "... at one ..."; I assume you mean "... are on ..."  
*Correct, thank you for pointing this out.*
- P12, L290-292: Here, the authors state that the underlying distributions are different between the two setups, but you nevertheless report mean values. As the mean is a parametric quantity that is dependent on the underlying distribution, using means to compare the quantities is only valid if the underlying distributions are equal. I would therefore refer to reporting mean values, but rather median values, as they are independent of the underlying distribution. Please check for further occurrences in the manuscript.  
*Yes, the median should be preferred if the distribution shape strongly differs. However, while the distributions due change somewhat between the two simulations, they are still similar enough to not introduce systematic biases when comparing the means instead of the medians. For the example given here, the mean  $q_v$  are 0.216 g/kg and 0.212 g/kg for the global and the nested simulation, respectively. The median  $q_v$  values are 0.171 g/kg and 0.155 g/kg. While the median values is smaller than the mean, the relation between the simulations is not changing. This further corroborated by the closeness of the mean and median values in the pressure-binned data (e.g. Fig. 5 b) that is already included for all variables of interest in the paper. However, we added the median values to the text (on top of the mean values).*
- P15, L362-363: "... increasing ..."; change to "...increases..."  
*Done.*
- P19, L472-473: I would also see a second effect that might cause the stronger graupel production. Due to the higher vertical velocities, saturation with respect to water can be more easily sustained, which might to some extent compensate for the depleting effect of the Wegener-Bergeron-Findeisen process for liquid hydrometeors, thereby causing higher graupel production.  
*Thanks for the insightful comment. Higher vertical velocities may indeed influence the efficiency of depletion of supercooled liquid water due to higher condensation rates. Indeed higher vertical velocities could also result in a larger number of cloud droplets being transported to the mixed-phase region due to smaller  $N_c$  and a different ratio of the lifting timescale*

*to the timescale of rain production by collision-coalescence. Higher condensation rates are indeed identified in Fig. 8 for the ascent region. Also, higher  $q_c$  and  $N_c$  values are found in the nested simulation in the lower part of the mixed-phase region (700-800 hPa), while they are almost identical at higher altitudes (Fig. A14 a and b). However,  $q_r$  and  $N_r$  are also slightly larger in the lower part of the mixed-phase region making the conclusions less clear.*

*We have added the alternative hypothesis for the enhanced riming rates as suggested in lines 486ff of the manuscript.*

- P26, Fig. A1: For better orientation, I would ask the authors to unify the geographical extent and add coastlines.  
*We have updated the figure as requested.*
- P22, L516: "... below pressures of above 500 hPa ..."; It is not fully clear to me what the authors mean here.  
*Thank you for pointing this out, we have removed the word "above". The trajectories remain below 500 hPa.*