Response to RC 3

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

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General comments

This is the second part of a two-part paper looking into moisture transport in a warm-conveyor-belt (WCB) case study. This paper in particular presents results from an Eulerian point of view. The paper's topic is within ACP's scope. The study consists of the analysis of two simulations of the same case at two (three) different resolutions. One simulation (at 13 km grid spacing) includes a convection parameterisation scheme; the second simulates convection explicitly. The study also describes a new method to determine the location of a WCB from an Eulerian point of view. While this method makes sense in general, I point out some aspects that require clarification. The analysis is generally sound. However, I am not fully convinced by the last part of the analysis, as I detailed below in my Specific Comments section. In my view the manuscript also requires some work to tidy it up in terms of statistical analysis as well as organisation and formatting that although minor when taken all together might represent more substantial changes. Therefore I cannot recommend the publication of the paper in ACP in its current form.
Thank you for your detailed comments and suggestions for improvement.

Specific comments

- L86-90: Please expand on the discussion on the influence of the convection parameterisation scheme on the trajectories in the low resolution simulation. Why is the vertical motion of the trajectories not being influenced by the parameterisation of convection? Do you simply mean that the scheme does not output a momentum tendency? In my view the vertical motion is still affected, albeit indirectly, by the scheme by modifying the atmosphere's thermal structure. Then, I don't understand your next sentence: "In particular, the latter means we are only able…". Isn't it contradictory to say that a WCB trajectory would be able to see the impact of

convective moisture transport if the trajectory passes over a grid-point where the convection scheme is triggered when it was just stated that the vertical motion of the trajectories are not influenced by the convection parameterisation? I think this paragraph needs rewriting to make it clearer.
We have reformulated this paragraph to make it clearer. The trajectories are based on grid-scale velocity fields, therefore they see the momentum tendency from the convection paramterisation, but do not experience convective vertical displacement which is not present in the grid-scale wind field as the mass exchange is assumed to take place instantly (one timestep). Aside from the momentum tendency, the convection scheme redistributes water (and energy) in the vertical by producing tendencies for the water vapor and ice (and temperature) fields. These tendencies impact the grid-scale field of these variables and therefore trajectories passing through columns, where the convection scheme is triggered see the tendencies in the humidity (temperature) from the convection parameterisation but not the convective motion itself.

- L166-170: I really don't understand step 2. First, you discard all grid points with q_v less than a custom threshold, but then you say that this allows the mask to 'crawl' forward. How can the mask 'crawl' forward ('grow'?) by removing grid points? I would expect the exact opposite effect! Then, why do you need to iterate? Why don't you simply fix the threshold, or does this threshold depend on the iteration? Please, expand your explanation on this step.
  In the first step, the zero'th order mask is made. In the second step, this mask then goes through a looped process. In each loop, the mask is dilated by one step, producing a union of all points in the mask and their immediate neighbors. Then, any points with qv below the threshold are discarded. Thus, the mask will have grown into a region where qv is high enough, but not grown into a region where it is not (the mask can "hit a wall" in one direction and keep growing in another). The loop then proceeds. We have made this more clear in the text.

- Section 5: I'm not fully convinced about the analysis presented in Section 5. I agree there is a link between OLR and the amount of moisture that is placed at UTLS by each simulation. However, I'm not quite sure that this can be uncovered by analysing CTT and OLR separately. Even though this is not stated anywhere in the text, I understand that OLR is output by the model. What about CTT? Is it not possible to output CTT? I agree that the method to find CTT is sensible, but it will still introduce biases when compared to the model's thermodynamics. I'm very surprised about the functional relationship between OLR and CTT (Fig 18). I would have expected a more monotonic relationship. After all, OLR is used to estimate cloud height.
  The reviewer is correct in assuming that OLR is a model output, while we diagnose CTT ourselves. CTT does not exist as output variable from the ICON model in the NWP configuration that we were using for this study.

The relation of CTT to OLR as done in the paper to uncover a physical link between the two is of course a strong simplification of the actual radiative transfer that occurs in the model (and nature). Radiative transfer (in the one-stream model used in ICON) is not only impacted by CTT (however it is determined), but many other features such as the vertical distribution of condensate mass in the upper part of the cloud, the vertical distribution of water vapor and other greenhouse gases (as well as aerosols). These additional information is typically also used when estimating cloud height from OLR observations with satellite. Therefore it is not to be expected to have monotonic relationship between OLR and CTT, albeit there is a strong impact of CTT on OLR - and this is visible by the overall trend in Fig. 18. We think the most counterintuitive finding in Fig. 18 is the sharp increase in OLR for very low clouds, but think this is related to these clouds typically being very tenous and therefore to be likely semi-transparent to longwave radiation.
We include some of this discussion in section 5 of the revised manuscript to enhance clarity. However, there is no straightforward way to improve on the diagnostics used in the paper.

- The analysis will benefit also from my recommendation to conduct significance tests on all your statistical results. Several arguments seem to rely on differences between distributions or means but it is difficult to gauge how important these differences are without a statement on significance.
We refrain from doing significance tests, which are not straightforward for the complex distributions shown here. Also we think the impact of the differences we show here depends on the highly non-linear growth of the differences via moist flow dynamics and radiation interactions, which will not be captured by statistical significance testing. We are currently working on a project, that tries to estimate the impact of differences as described in the current manuscript for the further evolution and climate state of the extra-tropical atmosphere, which is not straightforward and definitely beyond the scope of the present paper. Also for robust statistical significance tests one would like to rely on more than one case.

- I'm not sure it is correct to talk about interactions between q_v and OLR at pressures that are not the top of the clouds (or the surface in the case of clear-sky conditions). If the interaction occurs at other points, then it is not outgoing longwave radiation.
The outgoing longwave radiation will be modified by the gases and particulate matter (mainly aerosol) in the atmosphere above the surface (either Earth surface or cloud top). For example, longwave radiation emitted by clouds in the planetary boundary layer will be modulated by the distribution of water vapor and other greenhouse gases in the free troposphere, stratosphere etc. Therefore the question of co-location of clouds and water vapor gradients matters only to the degree that water vapor distribution below the cloud top is of little relevance to the OLR. Outgoing longwave radiation is typically defined at top-of-atmosphere as is used in this study.

Technical Comments

- L6: Add 'we' after 'Here'.
  Done.

- L14-15: The quoted implication seems limited in scope. Are there any broader implications, e.g. considering the Earth's planetary energy budget?
  added "the impact of WCB's on Earth's radiative budget [...]"

- L40: Reference formatting: replace semi-colon by 'and' in between Oertel et al. (2025) and Schwenk et al. (2025).
  Done

- L90: Add 'to' between 'is' and 'expand'.
  Done

- L121: 'Neighboring grids interact through a two-way coupling'. But this is only for R03B08 and R03B09, or is R03B07 also included? A table with the features of the simulations or the domains would be useful to explain how the simulations fit together. For example, I'm not clear on whether the 6.5km grid covers the whole area inside the red rectangle in Fig 1b or only the area between the red and the black rectangles.
  We have added "All neighboring grids" to make clear that each grid interacts with its neighbors. Have also added a reference to Figure 1 and which areas are covered by which grid.

- L127-129: The sentence 'In contrast to the nested simulation...' is a repetition of what was stated just before it and therefore can be deleted.
  Done

- L132-133: Format: Put the references to Miltenberger et al. and Oertel et al. between brackets.
  Doen

- L138: I don't understand what is meant by 'within a visually defined region at a specific time.' When is this time? And how was the region visually defined?
  We now state: "and be within a region defined by visual inspection of the cloud and moisture fields at a specific time."

- Figure 1 caption: The caption refers to nested domains 2 and 3, but these have not been defined anywhere in the text. Instead the designations RXXBYY have been used. Decide on one nomenclature and use it consistently throughout the manuscript.
  We have added the information in the figure caption and use domains 2 and 3 throughout.

- L175: TF2 and TF3 are first mentioned here without explaining what they are. I know after reading the manuscript that they are going to be defined later, but I recommend to re-organise the manuscript so that it follows a logical order.
  Good catch. The sentence works well without referring to TF2 and TF3: "The masks that we produced for both simulations ~~during TF2 and TF3~~ are visually very similar in terms of area"

- L176: 'The Area and volume of the masks...': Is the area referred to here the horizontal area? Is the volume achieved by integrating over pressure levels?
  We have added clarification. The area is the geographical area and the volume refers to spatial volume.

- L184: Delete 'that across'.
  Done

- L190-191: The statement 'If there are too few trajectories at a given pressure level, regions of the WCB could still be excluded, even with dilation.' contradicts the one in lines 180-181: 'In theory, one trajectory per pressure level could suffice to fill the WCB area at that pressure level through dilation'. Perhaps it would be enough to explain why or how the conditions of the theory are not satisfied.
  Good point. We make clear that it is due to the fixed amount of dilation steps, that too few points might cause the area not to be filled entirely.

- L211: You defined three time frames, but only gave any details of two of them. What is the third TF? I'm not sure 'time frame' is a very fortunate name. 'Frame' is an instant in time, rather than a period or a stage.
  We have added a definition of TF1 and noted that it is not further relevant for this study.

- L214: It should read 'both simulations'.
  Done

- L215: From the times defining the TFs they overlap. Is this intended or is there a typo somewhere?
  Thank you for pointing this out as it was a mistake. TF3 begins at 10:00 UTC not 00:00 UTC. Fixed.

- L223: 'More chaotic' (also in L463). 'Chaos' has a more or less precise definition involving, e.g. sensitivity to initial conditions. Is this what you mean here? Otherwise, it's probably best to avoid the term.
  Good point, we have dropped the term "chaotic"

- L223: '2017-09-23' I believe ACP has a specific format for times and dates. Please refer to the journal's guidelines.
  This will probably be covered by the typesetter, but thank you for pointing it out.

- L232: Define all symbols such as N_i, q_i and r_i when they first appear in the text.
Done

- L234: The figures are discussed in a different order to that in which they appear in the manuscript. Re-order them.
The figures have been reordered.

- L235-236: How realistic are the peaks at low values of r_i? The one at 250 hPa (Fig. 9b) gives the impression to be related to some sort of threshold as values suddenly drop from high to zero.
This comes from the simplified microphysical parameterization scheme used within the convection parameterization and is discussed in the text.

- L260: Rewrite by substituting 'they' for 'both simulations'.
Rewritten for clarity

- L272: 'patchiness' could be replaced or perhaps described further as containing small-scale structure.
Done

- L276: Remove 'This is an interesting result for those who study supersaturated regions in UTLS (and for instance their fractal characteristics).' Or expand on how this is interesting and add references.
Removed.

- L282: tau_sat,ice has not been defined.
It has been defined further up in the text (line 247 in the old manuscript).

- Figure 5 (and all histograms): How have the distributions been normalised?
Normalizing historams is a standard option for any coding language (normalize=True). Each bin count is divided by both the total number of samples and the bin width (height_i / (N bin_width))

- L296: 'As these areas account for a substantial proportion of the data points' Where is this shown or how do we know this?
11% of grid points are convective, as stated a couple of sentences before. We have added clarification.

- L296: Avoid 'it is obvious'. At least it is not so obvious to me. In fact, why then does the red distribution not dominate?
We have removed the term "obvious". The red distribution is from a different simulation than the black distribution, so we do not know what the reviewer means.

- L327-328 (and all comparisons between histograms and statistics): Include some statistical significance analysis in all comparisons between histograms and statistics (mean, median). Otherwise all assertions about

which one is larger become very subjective. They may appear different but if that difference is not significant then the interpretation becomes more complex.
For non Gaussian distributions (especially for logarithmically spaced data) it is not trivial to add confidence intervals. We believe that any time we make a strong statement about the differences in distribution concerning their magnitude, the values and histograms provide enough basis for the statement to be true. Also see our comments to specific comment 4 regarding physical impact of differences versus statistical significance testing.

- L368: The figure for N_i is included in the main manuscript but that for q_i is only in the appendix. What criteria was taken to decide what figures to include in the main text and what to relegate to the appendix?
The main consideration was that N_i is also shown in the main text for discussion of TF2 results. In order to limit the (already relatively high) number of figures in the manuscript we decided to not show q_i in the main manuscript for TF3, as it is also not shown for TF2. Also their is no new effects compared to the discussion of r_i in TF2.

- L368: Can the differences in histograms between simulations be an artefact of the mask construction method? It looks like the trajectories cover more fully the Eulerian mask in the nested simulation.
Great care was taken in the construction of the masks, as pointed out in the methods section. However, we have pointed out that this can in fact be the case, although we deem it unlikely.

- L371: 'not observed' Do you mean 'not included' or some other term? You are not talking about observations here.
We reformulate: "However, one possible reason for the discrepancy in UTLS ice content between the Eulerian and Lagrangian analyses is that many in-situ cirrus clouds are probably formed in the WCB outflow and not picked up by the online trajectory calculations"

- L410-411: Is there a functional relationship between OLR and CTT? If so, what is it?
Yes. The temperature of cloud tops determines the amount of energy radiated by the atmospheric column, given the Stefan-Boltzman law of radiation. However, as pointed out in our reply to your last two "specific comments" CTT is not the only component in radiative transfer calculations. We have expanded the text in section 5 to enhance clarity as outlined in our response to your specific comments..

- L435: It should read 'chosen'.
Corrected

- Figure A2 caption: Do you really mean heatmaps or simply maps (horizontal cross-sections)?

Heatmaps is a common term for the presentation of data in this way. However, we have adjusted the wording of the figure caption.