Review of manuscript egusphere-2025-5631 submitted to Atmospheric Chemistry and Physics (ACP).

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

by Cornelis Schwenk and Annette Miltenberger

General comments:

This study uses ICON simulations to investigate the role of horizontal grid spacing in moisture transport by warm conveyor belts (WCBs) into the upper troposphere and lower stratosphere (UTLS). I find the Eulerian framework to define WCB ascent and outflow layers both interesting and novel. The results provide valuable insight into the radiative implications of WCBs when moving toward higher model resolution. Overall, the manuscript is well written, the figures are of high quality, and the topic is highly relevant for the weather and climate community and of clear interest to ACP readers. However, there are several points that, in my view, should be addressed before the manuscript can be considered for publication. These issues are detailed in the specific comments below. In particular, I am concerned about (i) the fact that this manuscript is presented as Part II of a two-part study, while Part I is still under review, and several key methodological aspects and interpretations are repeatedly referred to the companion paper, and (ii) the attribution of the results to the convection parameterization when the simulations differ simultaneously in grid spacing and in the treatment of convection. Given these points, I recommend **major revision.**

Major and specific comment

a) Dependence on a companion paper under review: I have concerns about the fact that this manuscript is presented as Part II of a two-part study while Part I is still under review. The present manuscript relies on and repeatedly cites the companion paper for key methodological aspects and interpretation. This is unusual and, in my view, problematic for the evaluation of the present work as a stand-alone contribution. I would recommend either (i) making this manuscript substantially more self-contained, (ii) postponing submission until Part I is accepted, or (iii) providing a clear justification and summarizing the essential elements from Part I that are required to assess the results here.

b) Description of the experiments: The description of the model experiments is rather limited. While I am aware that the experimental setup is described in a separate publication, I think the present manuscript should include more information to allow the reader to properly assess the results. In particular, I would expect at

least a concise description of the vertical resolution and some indication of known biases or limitations of the simulations.

c) Convection versus grid spacing: The authors largely attribute the differences in WCB properties to the treatment of deep convection. However, the simulations differ not only in whether deep convection is parameterized or explicitly resolved, but also in horizontal grid spacing (13 km with parameterized convection versus 3.5 km with explicit convection). This makes it difficult to disentangle the respective roles of resolution and convection treatment. The current comparison therefore mixes two effects. Ideally, this would require experiments at the same horizontal resolution with different convection treatments, or at least a more careful and explicit discussion of this limitation. As it stands, it is not clear whether the reported differences should primarily be attributed to the convection scheme itself or to the change in resolution.

d) Reference dataset / evaluation against reanalysis: Related to point (b), while the focus is on comparing two ICON configurations, it would strengthen the paper to include a reference against a reanalysis such as ERA5. This would help to put the results into context and to assess the magnitude and sign of possible biases in the different simulations, rather than only discussing differences between model configurations.

Minor and technical corrections:

-Line 5-6: It may be worth mentioning explicitly that a 6.5 km simulation is also produced.

-Line 17-18: In “the most important positive climate feedbacks...”, consider giving one or two concrete examples.

-Line 25: There is an extra pair of parentheses in “((Guo and Miltenberger, 2025))”.

-Line 59: Part of the question “question: Can low-resolution, convection-parameterizing simulations (which are typically used for global forecasting and climate predictions) adequately represent WCBs,…” has already been addressed in previous studies (e.g., Dolores-Tesillos et al., 2025; Joos, H., et al., 2023). This should be acknowledged and briefly discussed.

-Line 71: It is uncommon to cite a paper that is still under review; I would not recommend this. See major comment a).

-Line 75: What is the vertical resolution of the simulations? This likely matters for air-parcel ascent and should be discussed.

-Section 2.1: The description of the simulations lacks detail. Since the study focuses on vertical transport, I would expect a clearer discussion of the vertical resolution

and how it changes (if at all) with horizontal resolution. Is the vertical resolution scaled with the horizontal grid spacing? If not, how is this choice justified?

-Line 116: This is unclear to me. Do you first run a global simulation and then perform additional simulations with two nested domains? Please clarify the experimental design.

-Line 131: The “online-trajectory module” is mentioned, how does this differ from LAGRANTO (Sprenger and Wernli, 2015)? A brief comparison would be helpful.

-Line 138: “within a visually defined region at a specific time…” is unclear. What exactly is meant by “visually defined”? Please clarify or rephrase.

-Line 158: The term “patchy” is vague here; please consider using a more precise term.

-Line 166: I am not familiar with the “dilated” procedure. Please explain what is meant.

-Line 169: The word “crawl” is unclear in this context; please rephrase or explain more precisely.

-line 175: TF2 and TF3 are not defined when first introduced.

-Line 184: How exactly are these fields “collapsed”? Please clarify the procedure.

-Line 193: The word “muddy” is unclear; please replace it with a more precise term.

-Line 207: I suggest changing “WCB detection” to “Eulerian WCB detection” for clarity.

-Line 209: “rely on pre-training a machine learning algorithm on…” the wording suggests the model needs to be trained repeatedly; please clarify or rephrase.

-Line 209: “variables, but instead only requires qv.” However, trajectories are still required. This sentence should be clarified.

Line 16-17: It might be useful to compare the 3 km and 6 km simulations directly to better isolate the role of resolution.

-Line 228: I suggest replacing “greater” with “larger”.

-Line 232: The parameters Ni, qi, and ri are not defined here.

-Line 293: “convective precipitation over the preceding 15 min exceeds 0.01 kgm−2.” The choice of this threshold needs either a reference or a brief justification.

-Line 394: “10-7 kgkg-1”, please clarify where this threshold comes from.

-Section 6: I found this section somewhat confusing, as it reads more like a conclusion. I suggest splitting it into two sections: one for discussion (comparison with previous studies, implications, limitations) and one for conclusions.

-Line 459: Could your approach be used to validate ELIAS 2.0, or conversely, could ELIAS 2.0 be used to validate your method? This might be worth discussing.

-Line 469: A closing parenthesis is missing at the end of the sentence.

References

Dolores-Tesillos, E., Martius, O., and Quinting, J.: On the role of moist and dry processes in atmospheric blocking biases in the Euro-Atlantic region in CMIP6, Weather Clim. Dynam., 6, 471–487, https://doi.org/10.5194/wcd-6-471-2025, 2025.

Joos, H., Sprenger, M., Binder, H., Beyerle, U., and Wernli, H.: Warm conveyor belts in present-day and future climate simulations – Part 1: Climatology and impacts, Weather Clim. Dynam., 4, 133–155, https://doi.org/10.5194/wcd-4-133-2023, 2023.

Sprenger, M. and Wernli, H.: The LAGRANTO Lagrangian analysis tool – version 2.0, Geosci. Model Dev., 8, 2569–2586, https://doi.org/10.5194/gmd-8-2569-2015, 2015.