Response to RC1

We would like to thank the reviewer for their thorough reading of the paper, their valuable comments, and their recommendation to accept it for publication. The reviewer raised important points and made smart observations that improved the paper. When we do not implement one of their recommendations, we provide a clear explanation of our reasoning.

Below, we first respond to their major comments, then to their minor comments. We have implemented the technical corrections and omitted them from the response.

More changes than listed have been implemented to accommodate the suggestions by the other three reviewers. Also, figure numbers are shifted by one due to the incorporation of a new figure depicting a synoptic overview (now Fig. 1).

Response to major comments:

– In Oertel et al. (2025), and in this paper, a multidimensional Gaussian Process (GP) emulator is used to approximate model behavior. This approach assumes that input parameters, such as capacitance, vary smoothly and that the resulting model output — including variables like ice number concentration and mean ice particle size — responds in a continuous and predictable way.
Although internal nonlinearities exist in the Seifert and Beheng (SB, 2006) microphysics scheme (e.g., mass–size relationships, category transitions), these are embedded within the model physics and do not necessarily introduce discontinuities in the macroscopic output variables if each hydrometeor category is analyzed separately.
However, imposed minimum or maximum limits on ice number concentration may lead to plateaus or discontinuities in mean ice size, particularly when ice mass continues to grow while the number concentration is constrained. Additionally, ICON's saturation adjustment scheme limits supersaturation to 100% in regions containing cloud droplets or rain, potentially producing flat regions in the relative humidity w.r.t. water output fields. Given that a Gaussian Process emulator is applied, do these plateaus or discontinuities introduce challenges (if they exist in any of the output fields) for the emulator's ability to accurately capture the underlying parameter–response relationships?

While Oertel et al. (2025) use a Gaussian process emulator to approximate model behavior, we do **not** use one in this paper. We strictly analyze the model output from the 70 PPE members without additional processing. The results obtained from the GP emulator analysis in Oertel et al. (2025) are **not** used, discussed, or examined in this paper. Therefore, this question does not apply to this study. If the reviewer is instead referring to the use of random forest (RF) models, we point out that we do not use these to make predictions or interpolations, but only to identify important parameters and their interactions. Also, there is no indication that there are any plateaus or discontinuities in the data.

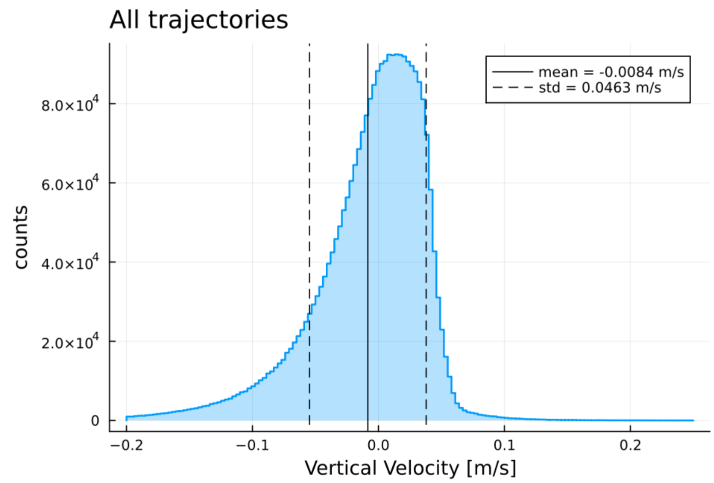
– In these ascending parcels considered within the WCB, the updraft speed is not zero. For example, when $w = 0$ and $Niri = 10^{-1}$ μm cm^{-3}, the relaxation time can be as large as 10^4 s (Fig. 1). If $w = 1$ m/s, the relaxation time can be reduced by approximately an order of magnitude under upper tropospheric conditions. For larger values of $Niri > 10^1$ μm cm^{-3}, the impact of $w \leq 1$ m/s becomes negligible (e.g., Korolev and Mazin (2003)).
Assuming $w = 0$ for all analyses of ascending parcels may lead to severely overestimated the ice phase relaxation times, particularly for fast-ascending trajectories.
It is stated in Line 492 that $w \approx 0$ for parcels taken at the end of ascent, but later, for example in Section 4, fast ascending trajectories are analyzed together with relaxation timescales. It is therefore recommended to explicitly include w in the relaxation timescale formulation to make this analysis more robust

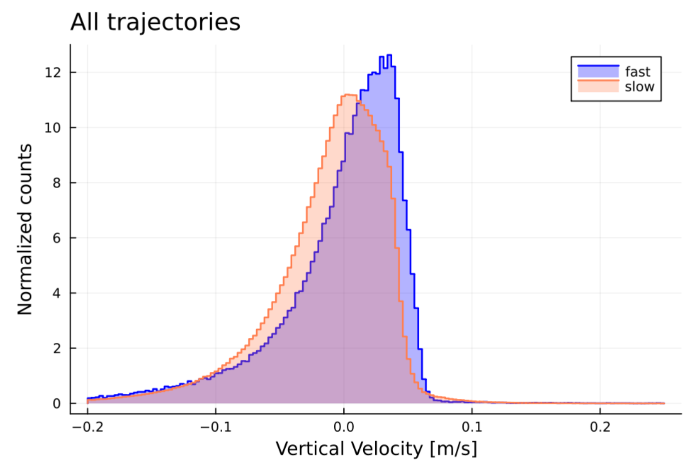
The reviewer raises an important point, but we argue that no changes are required.

First, in this paper, we examine the relaxation timescales of trajectories at the "end of the ascent," which, for each trajectory, is the point at which it has **completed** an ascent of 600 hPa and is **no longer rising faster than 8 hPa/h**. In the hour after this point, the mean vertical velocity for all trajectories is -0.0084 m/s, with a standard deviation of 0.046 m/s (see left figure below). For the fast ascending trajectories the vertical velocities in the hour after the end of the ascent are on average -0.0048 m/s, with a standard deviation of 0.054 m/s (right figure below). They may have high vertical velocities **during** the ascent but are no longer ascending when examined. Therefore, modifications to the relaxation timescale to incorporate the vertical velocity will be minimal.

However, even if neglecting vertical velocity would lead to biases, we point out that we do not use the relaxation timescale for any calculations or further analysis, but only as a **diagnostic** to explain the differences observed in relative humidity. The correlation between the saturation timescale and relative humidity is clearly shown in the paper. Therefore, explaining RHi using the relaxation timescale is plausible, regardless of a potential bias.

Therefore, we see no need to include vertical velocity in the relaxation timescale formulation. We hope the reviewer now shares this opinion.





– Line 430: "The mean maximum mixing ratio for snow (max(qs)) during ascent also increases with CCN (Fig. 5 e), presumably because in the two-moment microphysics scheme riming contributes to the mass growth rate of snow,...". The reasoning here may not agree with how riming affects particles in SB. In this case: When riming occurs it can have a two fold effect on ice and snow. If ice or snow rimes with raindrops it is converted to graupel else if it rimes with cloud droplets it remains either ice or snow unless it exceeds a critical rime mass threshold depending on some tuning parameter. Typically snow is considered as pristine and therefore is converted quite rapidly to graupel (by assuming a low space filling constant). If riming on snow occured more often then qs would most likely decrease because of this conversion, not increase, right?
I would suggest the process look more like the following: Increased CCN concentrations lead to more numerous but smaller cloud droplets, which slow the formation of raindrops. This, in turn, reduces the collision efficiency between ice or snow particles and cloud droplets, slowing the conversion to graupel. As a result, snow remains more abundant.

We thank the reviewer for this idea, which is indeed a much better explanation than ours. We have adopted this and now write the following (Line 465): “**The mean of the maximum mixing ratio for snow (max(qs)) during ascent also increases with CCN, while for graupel (max(qg)) it decreases (Fig. 6 e). We explain this as follows: increased CCN**

concentrations lead to more numerous but smaller cloud droplets, which slows the formation of raindrops. This, in turn, reduces the collision efficiency between ice or snow particles and cloud droplets, slowing the conversion to graupel. As a result, snow remains more abundant and graupel less abundant."

– Line 394: I follow this reasoning, but a similar argument applies to lower INP scaling: with fewer activated INPs, there is less competition for available vapor, resulting in larger mean ri. This in turn enhances the conversion from qi to qs. Some aspects of this reasoning might become clearer, and confirming the authors explanation, when qs is also plotted in reference to Fig. 4d, e, and f

This is a smart observation. We have checked our data and found that when INP is small, the snow content at the end of the ascent does slightly increase, as hypothesized by the reviewer.

However, the amount of snow at the end of the ascent is very small (qs is on average 10^{-3} g/kg and Ns on average 500 1/kg, compared to $5*10^{-2}$ g/kg for qi and $2.5*10^{6}$ 1/kg for Ni). This is because of the large abundance of ice crystals and because most snow particles precipitate by the end of the ascent. Therefore, the effects of the INP scaling factor on the ice content at the end of the ascent are still dominated by the effects we describe (the sheer amount of ice crystals drown out the process described by the reviewer).

However, the process proposed by the reviewer does explain the tail to larger ri for small INP. We have therefore added the following statement at the end of the paragraph titled "Impact of INP on ice properties at end of ascent" (Line 448):
"**The tail towards larger ri for small INP can be explained by the fact that when fewer ice crystals form during the ascent, there is less competition for available vapor, allowing for individual ice crystals to grow larger. This also enhances the conversion from ice to snow, which is supported by the fact that on average, qs at the end of the ascent slightly increases when INP is small (not shown).**"

We have not added another Figure showing the effect on snow, because the snow content at the end of the ascent is negligibly small, and we think that this would broaden the scope of the Paper too far.

Response to minor corrections (excluding technical corrections):

– Line 27: Instrument uncertainties remain, with calibrated measurements typically accurate to within 5–10% in supersaturated regimes (Petzold et al., 2020). Can it be added to highlight uncertainty with instrument

We have added this information on instrument uncertainties, citing Petzold et al. 2020 in line 28.

– Line 42: "tropopause region has increased on average from 2011 to 2020 compared to the 1980s". Can a more specific value be added?

We have contacted the authors and revised this statement to more accurately reflect their findings. They do not focus on changes in the transport of substances but instead on changes in the vertical transport of air-masses. Therefore, the revised sentence now reads (Line 42): "**[…] but a historical climatology by Jeske and Tost (2025) found that the height of convective outflow has shifted to lower pressures from 2011 to 2020 compared to the 1980s**." A more precise and quantitative statement is not possible given the findings from their paper, which conducts a largely qualitative analysis.

Line 48: "key contributor to extratropical UTLS moisture". By how much do WCBs contribute?

The measurements by Zahn et al. 2014 did not enable a quantitative analysis of how much WCBs contribute to UTLS moisture, only the qualitative conclusion that they play a large role overall. However, ongoing climatological research by Ziyan Guo at the University of Mainz has found that in December, January and February, the WCB moisture transport by grid-scale advection accounts for up to 13.8% (23.3%) of the total water (condensate) transport into the upper troposphere above the North Atlantic and Pacific ocean basins. At this time, the publication has not yet reached a stage where we can cite it. **We have therefore, regrettably, not changed this line (yet). If the publication in question is published during the typesetting stage, we will add this information and citation here.**

– Line 59: "various atmospheric constituents". Be more specific and replace the phrase with ...water vapor, hydrometeors and aerosols..

The sentence now reads (Line 68): "**In addition to producing precipitation, WCBs transport considerable amounts of energy as well as water vapor, hydrometeors, aerosols and trace gases across latitudes, and influence large-scale weather patterns […]**"

– Line 64: What is meant by the "incorrect representation of WBCs"? What is misrepresented? The location, vertical extent, cloud dynamics, cloud microphysics or water vapor transport?

Thank you for pointing out this sentence that could be more precise. We have changed it to better reflect what we want to convey and it now reads (Line 74): "**Given that WCBs exert such wide-ranging influences, forecast skill is highly sensitive to how well models capture their path, vertical extent, diabatic-heating profile and mixed-phase cloud evolution, and any uncertainty in representing these aspects can quickly degrade predictions of cyclone intensity, heavy-precipitation placement, downstream wave development, and even heat waves (Pickl et al. [...]**"

– Line 70: Cloud overlap can also change a cooling effect into a warming effect during the day (Johansson et al., 2019)

We have added this information. The sentence now reads (Line 81): "**As the WCB progresses northward, high-level frozen clouds have a warming or cooling effect depending on solar insolation, cirrus optical thickness, and potentially cloud overlap (Krämer et al., 2020; Joos, 2019, Johansson et al., 2019).**"

– Line 73: Is the warming effect the average when considering day (cooling) and night (warming)?

Yes. We now state (Line 85): "**… that has an average net warming effect.**"

– Line 105: "Supersaturation" is a continuous variable. Change the sentence to: "otherwise, greater or more widespread supersaturation would be produced."

This is a good suggestion that we have implemented as is suggested (Line 118).

– Line 118: If the cloud is glaciated then CCN becomes less relevant because of the Wegener-Findheisen-Process. In this case would CCN matter? Before glaciation CCN may be very important

We have added clarity to this statement by modifying the sentence that follows immediately afterwards, which now reads (Line 129): "**For instance, an increase in the capacitance could accelerate ice particle growth, while higher concentrations of CCN can lead to more and smaller cloud droplets in the liquid phase, which can in turn increase the number of ice crystals in the frozen phase and enhance vapor conversion.**"

– Line 165: I realize that is given in previous papers, but it would be good to have a very short one paragraph overview here with Figure 1 from Oertel et al. (2025).

After consideration we agree that this will help readers better conceptualize the case study without having to consider additional literature. We therefore have added a short paragraph as well as an additional plot (now Figure 1) that shows the synoptic evolution during the case study period using ERA5 data.

– Line 219: Replace the sentence with "In the PPE, the scheme is modified to allow supersaturation to develop under

conditions of strong vertical velocity."

This is a good suggestion that we have implemented as is suggested (Line 243).

– Figure 3 caption: Keep the caption between Fig 3 and 4 consistent... is it median qi, median Ni median ri or only qi, Ni, ri? I believe it should Fig. 4's description is incorrect.

We have made sure that the figure captions are correct and consistent (now Figures 4 and 5).

– Line 444: I don't fully understand this hypothesis. Could you please provide one or two additional clarifying sentences? Does it suggest that stronger convection over higher SSTs would lead to increased Ni and qi? If so, it seems that the primary driver would not be the SST itself, but rather the associated increase in vertical motion

Yes, we believe that if the WCB had more convection, we would also see a greater change in convective activity for simulations with high and low SST, which would would then influence Ni and qi more strongly than in our WCB case. **We have rewritten this entire paragraph (Lines 471 – 485) and hope that we make this point more clear.**

– Line 551: It is a interesting idea. Can you plot the cloud droplets numbers concentration in each case just before the homogeneous freezing temperature? E.g. are there more cloud droplets available for CAP < 0.4 and INP scaling < 0.5? If so, this will solidify your reasoning

Because the physical processes in the model are represented on discrete vertical levels, and because the vertical interpolation required to transition from an Eulerian to Lagrangian perspective when analysing cloud droplet number mixing ratios just before reaching the homogeneous freezing temperature is not straightforward, we refrain from showing this analysis. However, we can solidify our reasoning as follows: the only physical process that can produce ice-crystal number mixing ratios in the order of $10^7 - 10^8$ kg^{-1} is homogeneous cloud freezing. Heterogeneously produced ice-crystal number mixing ratios cannot exceed the number of INPs, which is on the order of 10^4 kg^{-1}. We have added this information to support our conclusions on homogeneous freezing the first time we talk about the process in Line 417:
"**This hypothesis is supported by the fact that the only physical process producing ice-crystal number mixing ratios in the order of $10^7 - 10^8$ kg^{-1} is homogeneous cloud freezing. Heterogeneously produced ice-crystal number mixing ratios cannot exceed the number of INPs, which is on the order of 10^4 kg^{-1}.**"

– Line 508: If one further assumes that the parcel is subsaturated, or that collisional growth is negligible and no conversion to snow occurs under supersaturated conditions, right?

That is correct and should be addressed. We have expanded on this sentence, which now reads (Line 662): "**However, the absence of higher median Ni 5 h after the ascent suggests that these small ice crystals sublimate. The alternative explanation to sublimation is collisional growth, which is proportional to the particle number concentration ($\sim 10^5$ kg^{-1}),which is much smaller than in a typical mixed-phase cloud ($\sim 10^8$ kg^{-1}), and the sticking efficiency, which is temperature dependent and very small at these low temperatures (Seifert and Beheng, 2006, 2001). Therefore, we find sublimation the most likely explanation, given the large fraction of trajectories that are sub-saturated with respect to ice during this time (Fig. 16 c).**"

– Line 692: How would one constrain realistic values for CAP if it is a function of the habit of ice crystals which keeps changing depending on the state of the environment?

That is a good point. We modify this statement to not suggest that more *realistic* values could be identified, but instead more appropriate values, which might not be realistic by themselves, but would produce more realistic *results*. The sentence now reads (Line 763): "**[…] may help in selecting appropriate values for CAP and the INP scaling factor for WCB flow configurations.**".

– Line 704: In the introduction Earth’s Radiation budget is mentioned and the sensitivity to vapor content in the UTLS. How does the results presented here have an impact radiation

Sadly, the original study by Oertel et al. (2025) did not focus on radiative effects and therefore did not write relevant variables to file. Therefore, we are not able to investigate what the impact of our findings are for the radiative effect of WCBs. Nevertheless, it is important to point out that this could be a topic for future research, given the large differences we found in UTLS moisture conditions between PPE members. We therefore add an item to the final outlook bullet-point list following the point discussing the implications for geoengineering (Line 771): “**Following this, future studies could investigate how the different UTLS moisture conditions produced by the PPE members influence the radiative effect of WCBs.**”