Replies to all Referee Comments on "Divergent convective outflow in ICON deep convection-permitting and parameterised deep convection simulations. " [1]

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1 General

We thank both referees for reading our manuscript and formulating their summary and feedback [2, 3]. The manuscript will be revised drastically (especially the introduction), since the reviewers have noted that a lot of information is buried and convoluted: this is also clear from their written reviews, which means that for improved readability, the introduction is the starting point to make the entire manuscript more accessible. Furthermore, we will work on the existing analysis and work on the presentation of the results and discussion to more clearly state the main points. The new manuscript will be heavily revised along these lines.
In particular, we will add a figure illustrating the current conceptual understanding of divergent outflows from high resolution large eddy simulations, which will (hopefully) bring the reader in a more convenient way to the starting point: the hypotheses. Furthermore, another figure illustrating the workflow (and emphasizing the key points thereof) will hopefully enhance the clarity and structure of the methodology (Section 3.2, mostly 3.2.1). The presentation of the results and the discussion will be strongly restructured and rewritten, but mainly using the existing figures.

2 Referee 1: reply to RC1 [2]

1. *'" please make sure from the beginning, already in the Abstract that you are dealing with Ensembles (otherwise reader, seeing just one case would think this is not significant)"'*
   We will emphasize this more in the updated manuscript.

2. *'"your sample size is still very small. From the modelling point of view it should not be much work to add more days - but dont know how much additonal work it is for your classification method. If you can do so, please add at least a few days with larger systems, this would be easily achievable for US systems or African squall lines with large and long-lived systems, but you can also add more days over Europe with prefrontal convection. You can still keep your main figures for Germany but for the scatter plots this would be extremely helpful. As it is I dont think you can quantitively talk about CMT or divergence in unorganized vs 2D squall line or Derecho type convection;*
   As said above best would be to add more samples (days in Europe US or Africa) and also to rewrite section 6 and adapt all Figure from Figure 6"'
   We agree with the reviewer that analysis of a larger variety of convective systems would be very interesting and would strengthen the insight gained. Unfortunately this involves a significant amount of computational and manual efforts, that we are not able to conduct within the scope of the present manuscript. In particular for our current analysis methodology, many manual analysis steps are required to process the data: the ellipse fitting process is automated, but the selection of convective systems is not. This means that one would have to go through a dimension of ensemble members and cases/regions/set-ups to compare, before the dataset can be finished. It may be possible to further automate those processes, but at this point this is not feasible. In the present manuscript we therefore want to focus on presenting the analysis methods and some first results as to the variability of upper-level convergence in a real-case NWP simulation.
   For the future, we have several ideas for re-applying the method to a large-spectrum of cases. In the manuscript we already mention that an ensemble with even more strongly organised squall lines at mid-latitudes would be particularly suitable for the first further assessment. Furthermore, we have

other plans to further investigate (mesoscale) convective systems with our methodology, and would also encourage others to do so. However, it needs to be clear that just the modelling centers are likely have the datasets in their archives that could allow them to do a very comprehensive assessment of outflow climatologies.

3. *'" instead of using precipitation, could it be more useful to use low values of OLR or BT to charcaterize outflow, this might produce smoother results and better identify the upper level circulation?"'*
In previous work on large eddy simulations [4], we have discussed that the upper tropospheric divergence is produced right at the location of the precipitation cell under conditions of storm relative winds that do not slant/tilt the convective system overly. In other words, the anvil (or OLR fingerprint) is not the first feature to map, when investigating the divergent outflows, but the updraft location is. In this work, rectangular horizontal masks of integration have been set such that they explicitly match the precipitation cells AND the divergent wind source, namely the region of divergent acceleration of the flow. The divergent winds produce the outflow and the divergent wind source does not move along with this flow. However, slanting/tilting of convective updrafts in the model can have an effect on the displacement of the outflow, with respect to surface precipitation. This effect of tilting has been taken into account by choosing a sufficiently large mask for the computation of divergence and surface precipitation(sometimes important for ICON-PAR indeed: as the high percentiles of vertical velocities at the 13 km grid-scale are much lower [i.e. up to 1-2 m/s] than in ICON-PER [sometimes 10 m/s], this contrast results in horizontal large horizontal displacements in ICON-PAR during the "convective" overturn). It has been discussed in [4] that if we extend the mask too far beyond the core area of precipitation, that subsidence is probably going to compensate for some of the divergent winds.

However, we do think that OLR can have an additional small impact on the divergence (less than 20%). But the leading order terms will be the precipitation (or, more specifically, its latent heating pattern) and convective organisation and aggregation. So there could be very small benefits of accounting for long wave radiation. Our point is however that, firstly, the main diabatic source of divergence, and the heating rate plus the role of organised convection therein, should be quantified, before much smaller terms are accounted for.
In summary, defining the extent of the convective cells by using OLR has intentionally not been done to avoid the near cloud subsidence effects and concentrate on the divergence of the main cells, which are better characterised by the precipitation latent heating and therefore the precipitation field itself.

4. *'" I am unsure it is helpful to normalize momentum transport by precipitation, yes it is natural to normalize divergence by heating but momentum transport should be normalized by shear"'; '"l12-13 "as opposed to expectations, CMT". I think this part also in result section should be revised or could be dropped in absence of sufficient cases/samples with shear;"' and '"l195-200 normalize CMT with precipiation, see main comments"'*
The hypothesis that CMT does not have a significant impact on upper-level divergence is currently based on LES results. We therefore believe it is a valid / interesting point to quantify the relations of CMT and upper-level divergence in a real-case scenario. We therefore keep this section but more clearly state that a larger sample of convective systems is need to firm up conclusions.

The choice was made was based on

- The work of [4] (no direct impact of convective momentum transport on the magnitudes of divergent outflow) and
- the fact that we investigate one case, where shear profiles are very similar to each other (a domain of only a few hundred by a few hundred kilometers and less than half a day are covered).

In Figure S3 of the supplement it can be seen that the CMT diagnostic and precipitation rate are strongly correlated. Therefore, to see potential impacts on the conditional mass divergence/outflow that might exist (we start with the hypothesis that there is none, based on [4]), the co-variability of the CMT diagnostic and precipitation rate has to be removed: no precipitation results in no CMT.
Now, we can test the effect of CMT on divergence with the null hypothesis that there is none. And given the statistical coherence and strong correlation, this null hypothesis has to be rejected. However, future inspection of more extensive datasets might change the conclusion and this will be discussed explicitly.

5. *'"I found section 6 of your manuscript not convincing and not well written, please redo"'*
We will work on strong modifications of the sections 5 and 6, following up on the lines of modification in the introduction and methods section of the paper. The new results sections will tie in much more closely with the conceptual perspective to be introduced in the introduction.

6. *'" also in Figure 6a the divergence is so scattered, in the explicit run, corresponding to small scale systems="noise" in Figure 5b that I dont think it makes sense to discuss linear regressions (couloured lines) as a function of lead time or system evolution. In that sense it would even make more sense to discuss better the parametrized runs with larger outflow"'*
The regression analysis is not applied to suggest any form of linear relation here. It is used to obtain designated statistical diagnostics for the fact that we find the green, red and blue systems in Figure 6 at the lower end of the distribution of divergence to precipitation rate ratio D. The diagnostics provide a quantitative interpretation of where the three convective systems are located in the divergence-precipitation rate space.
Furthermore, we apply a low-pass filter to the windfields from the convection-permitting simulation (removing all wavelengths smaller than 45 km) to remove the gravity wave "noise". Thereby, the convection-permitting and convection-parameterised simulations obtain roughly the same effective resolution in the divergence field. After this operation, the divergent outflows are well intercomparable and there is no problem of small-scale noise in convection-permitting simulations (lacking in the parameterised configuration).

7. *'"Specific points: l12-13 "as opposed to expectations, CMT". I think this part also in result section should be revised or could be dropped in absence of sufficient cases/samples with shear;"' and '"l195-200 normalize CMT with precipiation, see main comments""'*
We refer to the answer above, on CMT, point #4 in this reply.

8. *'"l19 "initial condition flow variability". In you manuscript you actually use physics perturbation"'*
Yes, both are used. The physics perturbations are used to further confirm earlier results [4]. The new element is the initial condition perturbations, in the convection-permitting set up: we want to focus at the role of initial condition variability. This is also where the hypotheses of the work are focused on. Indeed ideally both types should be mentioned here, even if we think one of them is more relevant, for instance, to predictability and the novel elements of the manuscript than the other.

9. *'" l76-86 you formulate here 3 hypotheses. you might drop some of these in absence of larger samples and systems, as the main (also expected outcome) is a broadly linear relationship of divergent outflow and precipitation or better mass flux which should also hold for larger area (opposed to individual system)"'*
The convective aggregation mechanism [4] shows that the linear relationship does not effectively hold at high precipitation intensities in LES. The work here confirms that the same is found for convection-permitting simulations. The simple linear relationship holds for parameterised deep convection only.

   We may have to substantially reformulate and repeat some of the points on the significance of the convective aggregation mechanism of our previous studies to make it more clear, that the aggregation can lead to a non-linear mean divergence - mean precipitation rate relationship in convection-permitting simulations.

   By revising the introduction drastically, and the rest of the manuscript accordingly, we hope to address the shortcoming of buried main results in the manuscript. This should lead to a clearer overview of the main points and how to find them, in terms of scientific analysis.

10. *'" l188 "satellite systems" avoid this expression as "satellitee" misleading here"'*
We will change the wording. We mean accompanying, smaller, convective clouds of the main systems.

11. *'" l195 "vertical integral of CMT over all levels below". Not sure this is intended as over the whole troposphere CMT integrates to zero so in that case if you decide to keep CMT you might need to consider upper and lower troposphere separately "'*
This is exactly what we intended to do, but it seems that our formulation did not pin it properly to

the point. We split up the troposphere into the part of the lower- and mid-troposphere (below 315 hPa) and the upper troposphere and integrate over the two parts separately.

12. *'"l227 you migth add reference to https://doi.org/10.1002/qj.4185 here as actually the organisation is not so simple and sometimes too spotty in explicit compared to parametrized convection Also you might add refernce in the text to studies that explained that the convective outflow only has an effect to the circulation and subsequent predictability if the outflow occurs in the vicinity of the jet"'*
Thanks for your suggestion. We will consider adding this reference along with some discussion of the potential impact of organisation biases. However, we would also like to use this opportunity to point out that organisation biases in itself are not a problem: we aim to show with diagnostics which processes play a role. In other words, the first question is which processes are acting and are represented in the model. The follow-up question would consequently be, whether there is a bias or not compared to the real-world in the representation or activity of the processes ICON, and possibly other models. We focus on the first question in this work.

13. *'" l297 "increase over time" this looks like the diurnal cycle evolution"'*
The diurnal cycle is not the main actor, as we deal with a strongly dynamically forced case of convection. However, of course we cannot rule out that the diurnal cycle might have an impact. Convective organisation and aggregation is influenced by the diurnal cycle, but any organisational type of convection could in principle occur throughout the diurnal cycle. The life time of convective clouds could be of more significance, as it may be suggested by our results (Figures 4 and 6a in the initial submission, although this remains merely speculative). Based on the available evidence, we think that the diurnal cycle is of minor relevance compared to the lifecycle of the convective cases in the considered case.

14. *'" Figure 5: add (a), use same legend bar for (b) as for (c) and (d). Here and in Figure use everywhere units of [1/s] for divergence (not multiplied by density in some places, eg Figure 6) and scale legend bars by 1.e5"'* Figure 5 cannot use the same color bars for each panel, as parameterised deep convection and convection-permitting simulations have very different amplitudes in their divergence signal (which would make the patterns in one of the two configurations invisible). The convection-permitting divergence in Figure 5 (b) is still unfiltered (before the filter operation, applied to obtain figure 6 for convection-permitting configuration) and hence much stronger, but we focus here on the spatial distribution of strong ensemble variability. It does not matter whether it is in mass units or in inverse time units in Figure 5: we look at 250 hPa and the density is nearly the same everywhere.
However, once we want to compare outflow strengths, it is important to understand how much mass is transported in each case. Therefore, we need mass/density units in Figure 6!
This will be discussed explicitly in the updated manuscript.

15. *'" Figure 6a, coloured slopes not convincing. Figure 6 b more convincing, but why is in Figure 6b shallow (magenta) so different form no convection run (organge)?"'*
The coloured lines in Figure 6a a show the evolution of individual convective systems in the shown parameter space. In that respect we are not sure, what the issue of the reviewer is as we cannot change the data-base. We will make it more clear what the colored lines represent in the revised manuscript.
In Figure 6b the magenta data points are off, because they have a deep convection parameterisation switched off and the outflow is created from shallow convection instead, maximizing near 500-600 hPa. Therefore, this physics perturbation is slightly unrealistic, but it shows that the outflow levels can be altered by this unrealistic setting of the parameterization (e.g. probably as a result of unrealistic parameterisation mixing and entrainment processes and cloud system geometry, but this remains speculative).

16. *'" l439-440 Why do you say "larger-scale flow feedbeck in PAR likely not accurately represented"? Isn't supported by anything in manuscript and how do you know that PER effect is not overestimated etc? "'*
It is found that the ICON-PAR simulations only follow the upper range of mass divergence to precipitation rate ratio (D in our work) of the convection-permitting ICON, which initially occurs at low precipitation rates for the convection-permitting configuration (in this comparison, a correction for differences in outflow layer thickness is considered to make both configurations comparable!).
[4] shows that collisions of divergent outflows cause sub-linear increases of divergent outflow with increasing precipitation intensities, in LES. Here, it is investigated whether outflow collision effects are also detected in the patterns of deep convective outflow intensities in ICON. The hypothesis on the

presence of this convective aggregation mechanism on divergent outflow variability is supported by our data and shows that the divergent outflow increases with precipitation rates in a sub-linear fashion, towards high precipitation intensities. The same is found in LES [4]. The ratio D in the linear dependence for the parameterised deep convection configuration in ICON overlaps with the high range of divergent outflows (and D, there) in the LES configuration. Nevertheless, both the convection-permitting ICON and the LES do not maintain these high values of D up to higher precipitation intensities: the values of D decrease at these precipitation rates.
Therefore, since D in ICON-PAR turns out to be practically constant (linear relationship of precipitation rate/divergence), both LES and convection-permitting ICON analyses suggest that the diabatic warming by precipitation "pushes" too strong outflows in ICON-PAR (when statistically averaged over areas of high and low precipitation intensities): constant D up to high precipitation rates are not found when convection is resolved better, so the high D suggests a positive outflow bias. Therefore, both LES and convection-permitting ICON suggest that in ICON-PAR a too strong feedback from convective clouds towards the larger-scale upper-tropospheric flow is represented at high precipitation rates.
This effect is described in this paragraph and its description will be improved in the revised manuscript (along the lines of the discussion here).

3 Referee 2: reply to RC2 [3]

17. *'"Major Comments: 1. Introduction: Why is the introduction centered around predictability? This isn't really the topic of the study. I recommend centering the introduction around properties of convection; or focus on the predictability aspect in your results. In a similar vein: you need to better motivate why convective outflow is worthy of study. I also don't really understand what you mean by "flow variability" in the introduction in the context of this manuscript. Sensitive dependence on initial conditions? Variability in terms of space and time? "'*
We agree that the choice made for the focus in the introduction is not the best possible choice. However, we have had the following reasons to use predictability as starting point:

- This has been done because predictability is the motivation to do our investigation.
- The authors do not fully agree that sensitive dependence on initial conditions and variability in space and time are completely unrelated with regards to the posed hypotheses. Since different modes of convective organisation may occur in close proximity during some weather events of e.g. highly organised deep convection (e.g. supercells and squall lines), divergent outflows may also vary very strongly in close proximity. And building supercells versus very long squall lines are suggested to end up in very different D regimes in our work, based on [4]. The ICON simulations also reveal some variability in terms of convective organisation, as needed for the investigation. Although strong very extensive squall lines of hundreds of km do not occur (see discussion, Section 6, of the initial submission). As a consequence, small changes that create a supercell at location x or a nearby long squall line at location y may push the upper atmospheric flow differently.
It is along this line of thinking that the concepts of flow variability as a result of initial condition perturbations or in space and time all get related to the strength and sensitive timing of convective aggregation and storm geometries, as suggested in [4].
It is exactly here that differential outflow could affect predictability of the flow through the two main mechanisms, the first and second hypothesis.

In the revised manuscript, we would like to move the focus towards the convective organisation, representation and the conceptual understanding from previous works needed as a starting point for our hypotheses. With the latter, we would tie the work much more strongly to the previous works that are the most relevant. Furthermore, we will aim at a much more reader-friendly manuscript structure. However, as this manuscript does not deal with the impact of the convection on the flow-variability itself, the rewritten introduction will mention it mainly as a motivation and shift the focus more towards properties of convection as suggested by the reviewer.

18. *'" 2. Methodology: You're tracking convective systems in the convection-permitting simulations, but you're calculating box averages in the simulations with parameterized convection. This is not a fair or consistent comparison and the results aren't really apples-to-apples comparisons. I recommend focusing on box averages also in the case of PER so you can better compare with PAR."'*
We aim at modifying the methodology section in a way that this point will become more clear: In

both simulations box averages are calculated, and the difference in effective resolution is corrected for. Hence the comparison is fair and consistent. The tracking happens in parallel to the boxes, to fit ellipses and estimate geometry of the convective systems in the convection-permitting simulations. Additionally, we plan to add a workflow-type of figure to described the analysis method in more detail to improve its clarity and the revised manuscript will be modified accordingly.

19. *'" 3. Presentation of figures: As one of the goals of the paper is to compare properties of convection-resolving and parameterized-convection runs, I'd recommend comparing PER and PAR in each individual figure. Fig. 5 is an example where you sort of do this, but in an awkward way (tracks of PAR in Fig. 5a, divergence of PER in Fig. 5b, divergence of PAR in Fig. 5c,d without a clear correspondence to Fig. 5b). A direct comparison between, such as directly comparing Fig. 5b with 5c, would help the reader immensely with digesting the results in terms of what the differences between PER and PAR are. Along similar lines, Fig. 6a and b are difficult to compare because of different axes and markers, making understanding difficult."'*
We agree that the differences between the panels should be clarified and we will improve this in the revised manuscript (see also points #14 and 15 in this reply, to the first reviewer).
Similarly, Figures 6a and 6b are integrated over different vertical levels, because the outflow altitude varies between the two panels (thickness of 200 hPa in convection-permitting runs vs. 250-260 hPa in parameterised configuration!). Therefore, the comparison in one panel would be unfair and suggest conclusions that are not justified. To avoid the suggestion, the two configurations are separated into two plots. We would like to point out this motivation to outline the figure 6 as it was, in the initial submission.
We will work on a clever solution, e.g. to include a line for a weighted average of ICON-PAR in the ICON-PER plot, or some addition along similar lines. This would further assist bringing across the point of Figure 6.

20. *'" 4. Sections 6 and 7 have the biggest potential to bring out some new findings, but in the current manuscript are rather short. I recommend expanding these sections and using them to "build" the paper. Important questions that can be asked are, for example, does the difference in organization between PER and PAR lead to fundamental differences in CMT? How does this matter for predictability? Does it matter at all? This seems to be part of your motivation (L516: "This close connection may lead to perturbation growth in a forecast or spread in an ensemble")"'*
Thank you for these insightful suggestions. We will consider them in the revision process. We think that this point/opportunity will (at least partially) be addressed automatically when we try to create a strong connection between the updated introduction and the updated discussion section, when the connections between acceleration mechanisms, convective aggregation and organisation, the outflow divergence and potential effects on flow predictability are explained in a considerate, clear and accessible way across the updated manuscript.

21. *'" 5. Section 7: I am not sure how the "Dimensionality hypothesis" fits into this study."'*
The "dimensionality hypothesis" comes into play, because we study the variability in divergent outflows from convection and we have shown that this variability can largely be explained by the dimensionality of the convection [4]. However, we have not sufficiently well clarified the connection to our earlier work. This will be improved in a revised version. In particular part of the introduction will be dedicated to explaining theses concepts, providing better context and thereby improve the presentation of the scope of this work.
Part of the study design is that the "dimensionality hypothesis" can (presumably) be thoroughly represented in convection-permitting configurations, which is why we zoom in on the convection-permitting simulations in section 5. However, it is not present in the configuration with convection parameterisation. We agree, that in the initial manuscript version this may not be sufficiently clear to readers, since the connection to [4] was not made clear enough. Therefore, it was difficult to understand the main concepts.

22. *'" 6. Conclusions: Now it seems like you're talking about things that haven't been shown before...for example, "The outflow is responsible for major ensemble spread in the divergent part of the upper-tropospheric wind during a convective event." I don't find any figures showing ensemble spread? It seems buried somewhere but I can't find it. "Using simulations at coarser resolution probably implies assuming a (near-)linear relationship between the outflow and net latent heating." – Does this refer to*

Fig. 6b? "'

1) This is revealed by the combination of 250 hPa divergence and precipitation patterns (also, more details on the parameterised simulations regarding this aspect are found in the supplement). It is just in the ensemble spread of our very specific setup, where divergence in Figure 5b, c, and d reveals this spatial pattern, closely connected to the precipitation systems. Large spread in divergence at 250 hPa spatially coincides with the proximity of strong deep convection. They confirm the earlier suggestions based on other studies. 2) This does refer to the difference between Figures 6a and 6b indeed: in a parameterised convection configuration, there is little spread on top of the linear divergence-precipitation rate relationship. Even if we have a "convection-permitting" configuration in at 13 km, this turns out to not change. However, once cloud scale processes and gravity waves are represented better (1 km resolution), we get a large envelope of variability on top of the (here: somewhat non-linear) relation between precipitation rate and mass divergence.
We will clarify both of the main points, mentioned above, as key findings, after the revisions.
The conclusions will be rephrased to make the connection to the results more explicit / obvious.

23. *'" Minor Comments: 2. L26+: You should specifically state the hypotheses. It's unclear what you're after. "'*
They are stated in lines 76-87 of the initial submission. But after a revision, we will carefully make sure that the hypotheses move to a very prominent place and that the introduction of the conceptual ideas behind those hypotheses is thoroughly explained.

24. *'" 3. L30: "based on their linearised gravity wave adjustment model an idealised expression of outflow strength from deep convection was constructed (Nicholls et al., 1991)" – Not clear what this means.*
4. L32: What do you mean by the "slope of dependency"?
5. L35: What is "Outflow dimensionality?"
6. L36: " idealised point ("3D") and idealised line ("2D") sources" – I don't understand this concept."'
As mentioned above in these replies, the conceptual model based on our earlier work [4] will be to be added in thoroughly, with a neat explanation. We agree with the reviewers that this was not done in a sufficiently clear manner in the current manuscript version, which strongly reduced the understandability of the presented research and its scope.

25. *'" 1. L1: What kind of event? This should be made clear (i.e., "convective event")*
7. L39: "such behavior" – What behavior?
8. L134: What day were the PER simulations initialized?
9. L138: You should state somewhere before that this is an ensemble study.
10. L141: What is the "alternative surface tile dataset"?
11. L141: "20 member initial condition ensemble closely" — Does this refer to PER or PAR?"'
16. Fig. 6a. The caption says "divergence-latent heating relationship", but the figure shows divergence vs. precip rate. Please reconcile.
We thank the reviewer for these suggestions; all points will be clarified in the revised manuscript.

26. *'" 12. Methodology (L190+, conditioning on precipitation rate): Do you take into account that the precipitation reflects atmospheric processes from some time before the instantaneous CMT is calculated? What I mean by that is that precipitation takes time to form and to fall and therefore reflects convection from some time ago. Or is there some time averaging going on? Or do you compute precip rate over some time interval?"'*
Thanks for rising this important point. We are aware of this issue. However, in our LES-studies [4, 5] it has been shown that this lag is not of substantial concern (see also the discussion section of [4]).
The output of precipitation (rate) and the winds is available at intervals of 5 minutes (ICON-PER) and 10 minutes (ICON-PAR) and is computed based on the precipitation sum. Hence, the differential of the precipitation sum was taken at the mentioned intervals. With respect to the winds, the output is available at the same frequencies: 5 minutes (ICON-PER) and 10 minutes (ICON-PAR). To further clarify, the dataset of convective systems covers the first +17 hours lead time (starting at +7 hours) of simulations in both ICON-PAR and ICON-PER.

27. *'" 13. L214: "In past times" – Not only in the past, this is still mostly true today. All operational global NWP models as well as climate models use some form of cumulus parameterization."'*
We agree with the reviewer that convection parameterisation are still (and will remain for some time)

a crucial component of numerical weather prediction and climate models. However, we wanted to highlight that at least for regional NWP there is a strong push towards kilometer-scale models, that do not require a deep convection parameterisation anymore. We will rephrase the sentence to make this more clear.

28. *'" 14. L238+: Why do you reject features farther than 25 km from being a match? This seems like a very strict criterion given the low predictability of convective features."'*
Here we do not refer to matches of convective systems between different ensemble members. The "matches" are between the centers of the boxes (used from the volume integration of divergence and corresponding surface precipitation) and those of the detected "convective precipitation" ellipses in the same simulation. The quality of matches is conditional on the ability of a computer vision algorithm to detect the ellipse correctly and consistently (as it is assured by validation algorithms). Essentially, the center of the convective system X in ensemble Y has to match only with itself - according to a center detection by the computer vision algorithm and one of a moving box. However, the "centers" of both metrics could differ and are allowed to differ by a certain number of grid cells, or equivalently, a specific distance in kilometers. This will be more clearly stated in a revised version.

29. *'" 15. L285: "PAR-profiles also reveal a strong divergence maximum directly underneath the tropopause (see supplementary material: Figure S5) – This figure should be in the paper as one of the main points is comparison between PER and PAR...you could make a two-panel figure for example."'*
Thank you for the suggestion. As, in contrast to the ICON-PER results, there is no time evolution in the profiles evident for ICON-PAR we did consider these less interesting. However, we will re-consider the prior decision to put them in the supplement.

30. *'" Editorial Suggestions: 1. L2: no comma after "both" 2. L132: local area model 3. L194: delete "mostly" 4. L319: space is missing before "Consequently" "'*
We thank the reviewer for the suggestions and hope that we will reduce the editorial, grammatical and sentence structure errors in the revised version. Additionally, in case of a final publication, professional copy-editing by Copernicus will also be conducted.

References

[1] Edward Groot, Patrick Kuntze, Annette Miltenberger, and Holger Tost. Divergent convective outflow in icon deep convection-permitting and parameterised deep convection simulations. *EGUsphere*, 2023:1–32, 2023.

[2] Referee comment 1 on "divergent convective outflow in icon deep convection-permitting and parameterised deep convection simulations.", 2023.

[3] Referee comment 2 on "divergent convective outflow in icon deep convection-permitting and parameterised deep convection simulations.", 2023.

[4] Edward Groot and Holger Tost. Divergent convective outflow in large-eddy simulations. *Atmospheric Chemistry and Physics*, 23:6065–6081, 2023.

[5] Edward Groot and Holger Tost. Analysis of variability in divergence and turn-over induced by three idealized convective systems with a 3d cloud resolving model. *Atmospheric Chemistry and Physics Discussions*, pages 1–23, 2020.