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

Edward Groot, Patrick Kuntze, Annette Miltenberger, Holger Tost

March 27, 2024

1 General

We thank the referees for reading our updated manuscript and formulating the feedback [2]. In this document, the **latest version** of the referee's comments are copied in red, with corresponding responses directly below in black.

2 Referee 3: reply to RC3 [2]

'" I am happy to accept the paper, pending some minor corrections discussed below. My requests for a more "like for like" comparison between the ICON-PER and ICON-PAR simulations, by using identical analysis methods for both, have not been addressed. But I am satisfied that making these changes to the method would be very time-consuming / require rerunning computationally-expensive data-processing, and would be unlikely to significantly affect the results. So I am happy to accept the article without this change. I thank the author for acknowledging / clarifying that the study did not find sensitivity of the precipitation - divergence relationship to the choice of parameterised versus explicit convection; the sensitivity found appears to be dominated by model-resolution alone. This point just ought to be clarified in a couple other places in the paper (I give some suggestions below)."'

We thank the reviewer for this feedback.
The suggestions of the reviewer are taken into account for the next revision and further addressed in the below.

1. *'"Many thanks for the additional discussion L74-83, and for clarifying in various places that the transient response is studied here. This nicely addresses the query "'*

   We would like to thank the reviewer once more for raising the issue and appreciate that the issue has been resolved in the revised manuscript.

2. *'"My argument does not depend on any horizontal variation in the stability profiles, or variation in stability between different models. Rather it is variation in the vertical structure of the heating profile, and how this correlates with vertical variation of environment stability. E.g. consider two different convective storms occuring in the same environment and static stability profile, and suppose (as is often observed) that the static stability N^2 is greater in the lower free troposphere than it is in the upper troposphere. Now, suppose storm 1 is a relatively young cell, so that most of the latent heating occurs in the lower troposphere, whereas storm 2 is a mature system with a substantial stratiform anvil region generating more latent heating in the upper troposphere. Even if the storms are producing equal precipitation rates, storm 2 will produce stronger upper- level divergent outflow, just because the heating is concentrated at upper-levels where N^2 is less, so that stronger ascent is needed to make the buoyancy removal by vertical advection (wN^2) balance the heating rate ($\frac{g}{T_v}\frac{\partial T_v}{\partial t}$). Even under a horizontally homogeneous environment and latent uniform neutral buoyancy level, storms in different states of organisation may produce differently-shaped vertical profiles of heating rate, which will give different upper-level divergence if N^2 varies with height?"' and '"Many thanks for pointing out the interesting experiment in the authors' LES study, where the latent heat of condensation was varied. Note however that the idealised temperature profile used here (from Weisman & Klemp 1982) has arteficially*

weak variation of N^2 with height (potential temperature increases nearly linearly). So this may not have sampled the sensitivity to vertical structure. I don't think the present study requires a detailed investigation into dependence of outflow strength on vertical structure (this might be an interesting topic for future work). But it might be worth breifly mentioning this possibility in the discussion?"'

We agree that this effect of differences in heating profiles as a function of differential convective organisation affects the local magnitude, and hence sharpness, of the divergence pulse, i.e., outflow at the tropopause (with large N^2). This localised pulse will strongly amplify with increased N^2, but be mostly locally confined to a thin near-tropopause layer for certain heating profiles. On the other hand, deep outflows of $300 - 400$ hPa or half the tropospheric depth (typically corresponding to substantial positive net heating throughout the troposphere) will typically reduce the sharpness of the near-tropopause peak of divergence and replace it with more gentle outflow at lower levels. Also, both our current work and the LES-simulations of [3] show broadly similar divergence profiles across the ensemble members (and over time), which could be seen as a small limitation.

We agree that the vertical distribution of mass divergence will be strongly affected by the heating profiles and hence by factors such as convective organisation, but consistent with earlier studies, we do not see a substantial variation of the vertically integrated magnitude of the divergence with the heating profile in our results. This is supported by consistency of the LES study and the current study (e.g. Figure 6a and some of the left panels of Figure S5).

Nevertheless, the current study (and the LES-study) does not span a large enough variation in vertical stratification and heating profiles to draw definitive conclusions on this aspect - we just intend to cover a range of deep convection variability and - in this regard - come to the conclusion we have stated.

We thank the reviewer for raising this discussion point and have included some discussion of this point in section 7.3.2 of the revised manuscript.

3. *'"The reviewer's experience with NWP models is that they routinely do produce both gravity-waves and updrafts / downdrafts very near the grid- scale (excessive single-grid-point updrafts are a common problem!) Whilst a model can only simulate these features with significant numerical error, the poor resolvability does not stop them from happening. The idea that models contain a truncation scale of 8 grid-lengths below-which all features are strongly damped has come from large eddy simulation, where this has been deliberately imposed by specifying a suitably large mixing-length parameter in the 3D Smagorinsky turbulence scheme (or by using a highly diffusive numerical method for advection). Imposing such an 8 grid-length truncation scale on an NWP model (with 13km grid-size and parameterised convection) would not be done operationally, as it would degrade the model skill. So I don't agree with the author; features significantly smaller than 100km can and will be simulated by the 13km NWP model, but with diminished accuracy. This is evident in figure 7(c,d); the ensemble standard deviation of divergence in ICON-PAR has a lot of structure on scales of around half a degree (50km), and the individual ensemble members will presumably contain smaller-scale structure that gets smoothed-out when looking at the standard deviation over all members?"'*and (now from item number 4!)*"'As discussed earlier, this notion is consistent with LES, but is often not the case in NWP models (especially at coarse resolution with parameterised convection, when they typically do not employ a highly diffusive 3D turbulence scheme to enforce the truncation scale). On the other hand, it might be that ICON is more diffusive than other NWP models that the reviewer is familiar with. This is critically dependent on the model's advection scheme and any horizontal diffusion used (e.g. as part of a 3D turbulence scheme"')*

   The authors do agree that near-grid features as updrafts, downdrafts and gravity waves do widely occur in storm-resolving simulations. However, the fact that they may - regularly and excessively - occur does not mean that the representation and interactions with nearby features are realistic and never spurious. Furthermore, if we look at spectral analysis, even of storm-resolving simulations, it can be seen that these spectra are still strongly suppressed below a certain wavelength [4], corresponding to about 5-10 grid spacing. Furthermore, representation of wavelike perturbation features smaller than 4 times the grid spacing gets virtually impossible (the perturbations cannot propagate downstream based on simple numerics, but rather will tend to dissipate by nature), so that propagation and scale interactions can only become realistic in the 5-10 grid spacings range.

   Nevertheless, we do agree, reduced or sometimes potentially excessive variability (relative to the real atmosphere) at small near-minimal length scales (say waves of about 6-8 times grid spacing) can significantly differ between different models and configurations, depending on details in treatment of advection and turbulence schemes and other means of exchange across scales.

   In the end, there is the intrinsic property of a truncation scale, which strictly dissipates (hence: removes) all perturbations that cannot propagate with the flow. As to what we see in our ensemble members, we do agree that perturbations of order 50 km exist in the ensemble means. Given the small ensemble size in ICON-PAR, this certainly (to some extent) reflects the effective size of features in individual ensemble members (which can just have different amplitudes locally, across the ensemble). In the ICON-PER ensemble there is somewhat more effective smoothing across the ensemble, but we believe that spectral properties of smallest resolved scales (like those in [4]) also affect our simulations.

'"Many thanks for the changes to the text following this. But there are a few other parts of the paper which still imply that the divergence-precipitation relation depends on the choice of parameterised versus explicit convection, even though no evidence for this has been presented (the sensitivity is dominated by model-resolution alone, as is now stated by the added text at L467). L750-751: maybe change: "feedback from deep convection to its surroundings at larger scales is likely not accurately represented with parameterised deep convection, even if the precipitation climatology is well represented: parameterised ensembles seem to be underdispersive in terms of corresponding dynamical variability at a given precipitation rate" to something like: "feedback from deep convection to its surroundings at larger scales is likely not accurately represented at coarse resolution, even if the precipitation climatology is well represented: ensembles with 13km grid-size seem to be underdispersive in terms of corresponding dynamical variability at a given precipitation rate" Maybe also clarify this in the abstract for consistency, e.g. change L3-4: "Near-linear response of deep convective outflow strength to net latent heating is found for parameterised convection, ..." to e.g. "Near-linear response of deep convective outflow strength to net latent heating is found for 13km grid-spacing with either parameterised or explicit convection, ..."'

We will carefully reconsider the wording. However, we would also like to point out that causality may seem to be implied to a very quick reader, but it is not really strictly implied by our wording: we do agree about dominance by model resolution, but also want to emphasise that a parameterisation often follows the choice of a certain resolution automatically.

We think that it would not be bad to use the statements as we do for the short and concise abstract, as usually coarse resolution directly correlates with parameterised deep convection. Nothing in the abstract is untrue, we highlight typical configuration A and typical configuration B, without mentioning alternative configuration C yet (i.e. explicit convection at 13 km grid spacing).

In the body of the paper, readers will realise that this is a simplification, and you could possibly try to resolve the convection explicitly at a grid spacing where this is typically not done, but this is rather atypical. In the discussion section, a sentence has been added to (re-)emphasise the results for explicit convection experiments at 13 km resolution in the revised manuscript.

"Many thanks for this insight! While a significant amount of mesoscale structure (in both the updrafts and the radiated gravity-waves) will be present on the 13km grid, it might be the smaller (km-scale) variability within the wider mesoscale structures that is key? Even though a 50km- long island can be represented on a 13km grid, the bays and inlets along its coast cannot. Refining the grid to 1km may increase the length of the coastline by an order of magnitude. By analogy, the possibility for gravity-waves radiated from within the "island" to collide with eachother from different directions will vastly increase? But perhaps clarify at L462-463 that with "little or no information on geometry of the convective systems can be represented", this is really about geometry of smaller-scale structures within the convective systems (i.e. km-scale, rather than mesoscale)?"

We highly appreciate that the analogy has clarified the content here. The reviewer is right: we agree that the (heating) structures of individual cells within convective systems will be of high importance. We will use the discussion here to strengthen the content of the manuscript further and utilise the analogy better!

4. *'"No, my argument was that the analysis would be simpler and the manuscript shorter if the low-pass filter was just omitted (it should make little difference as long as the box-averages consider areas much bigger than the filter-scale)."'*and *'"I am happy to take the authors' word that the low-pass filtering does not significantly affect the results anyway. So it is probably not worth rerunning significant parts of the work-flow to pursue this further."'* and

5. *'"This is a reasonable argument; the moving boxes in ICON-PER have been setup manually, repeating this exercise for every ensemble member and sensitivity-test within ICON-PAR would be very laborious, and the effort should be focussed on analysing the more interesting behaviour found in ICON-PER. However, one of the paper's main conclusions is drawn from comparing ICON-PER with ICON-PAR. It would therefore be stronger if more attention was given to ensuring like-for-like comparison and using the same analysis methods when comparing them. e.g. just for the comparison with PAR, the divergence-precip relation in PER could be computed on the same large static boxes as PAR (while still using the smaller moving boxes for the more detailed investigation of relationship to storm structure etc in PER) But again, at this late stage it may not be worth rerunning the work-flow for a consistency-check which is unlikely to affect the results much."'*

We are glad that the reviewer is happy with the analysis as is and shares the thought that the existing workflow is at least in practice beneficial for the study. Furthermore, we thank the reviewer for further elaborating on their thoughts.

6. *'"Many thanks for investigating and correcting this. It is very reassuring that it was essentially only the units shown on the colour-scale (6a) and x-axis (6b) that were wrong before! This has been corrected by removing an erronious factor of air density from the plotted data, so that they are now consistent with the stated units. This increases my confidence in the paper overall, by removing my worry that something else had gone wrong with the analysis."'*
We are glad that the reviewer is reassured.

7. *'"As discussed earlier it would be possible to ensure like-for-like comparison between PAR and PER, by separately computing the precipitation and divergence in PER over the same large static boxes as were used for PAR. But again it is probably not worth rerunning such a large part of the workflow at this late stage. I am not overly concerned about the subsidence assumption in the convection parameterisation here. In the reviewer's experience with mass-flux convection schemes, the assumption of "local mass compensation" has little impact, as the model's dynamical core still acts so-as to quickly restore a Weak Temperature Gradient in the horizontal after the parameterised convective heating is applied. This has the effect of immediately undoing the local subsidence imposed by the convection scheme, and redistributing it elsewhere, so that the end result is the same as explicit convection with the equivalent heating profile. The present study's results in figure 8b comparing the divergence response for parameterised versus exlicit convection at 13km grid-size support this."'*
We agree that having the same convective system in the same model would be the most **optimal** fair comparison and have mentioned a couple of reasons at the top of this reply (and in the previous reply) why this is apparently too challenging and unfeasible in practice for the selected case.
We are happy that the reviewer shares very little concern about subsidence effects and are glad that the opinion on this has been shared in this thread of replies.

8. *'"Thank you for the edited text at L415, which now clarifies that the quantity shown is the vertical integral divided by outflow depth. Omitting the normalisation by outflow depth would have made the comparison in figure 8 between PER (8a) and PAR (8b) simpler, allowing a slightly more succinct manuscript (the discussion about the correction for differing outflow depths would not be needed). But it does not matter that much. "'*
We are glad that the referee's opinion closely agrees with ours and would like to thank the reviewer for elaborating on this point.

9. *'"Many thanks for the extensive discussion around the relation between heating, the pressure field, and vertical momentum transport (and the dependence of the "turbulent" transport on model resolution). This made for an interesting and thought-provoking read, and I agree this wider subject is beyond the scope of the present study. I am happy with the slight rewording at L612-613, in the synthesis section. Maybe consider also mentioning the uncertain direction of causality in section 6.3, e.g. at L568-569?"'*
We are also happy about the exchange on this particular point and thank the reviewer for an active participation in this discussion.
In our opinion, the direction of causality is something for the synthesis and discussion, so that the focus in Section 6.3 is (as it has been in the latest revision) at the corresponding statistics. We would argue that this is a choice to streamline the content of the paper and keep it focused; any of the two choices could probably be preferred by an arbitrary subset of readers.

10. *'"Many thanks; all the previously listed minor corrections have been addressed. Except: L263: "Ellipse fitting and verification are used to quantity the geometry" should be "Ellipse fitting and verification are used to quantify the geometry?" I also just spotted one more trivial typo: L562: "...while Figure Figure 10b shows...""'*
Correct; we thank the reviewer for carefully reading the revised manuscript and correct the manuscript accordingly.

References

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

[2] Referee comment 3 on "divergent convective outflow in icon deep convection-permitting and parameterised deep convection simulations.", 2024.

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

[4] F. Judt. Atmospheric predictability of the tropics, middle latitudes, and polar regions explored through global storm-resolving simulations. *Journal of the Atmospheric Sciences*, 77(1):257 – 276, 2020.