Review of "Divergent convective outflow in ICON deep convection-permitting and parameterised deep convection simulations", by Edward Groot, Patrick Kuntze, Annette Miltenberger, and Holger Tost.

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I find this article a very interesting read and I hope that it is published.

The relation between net latent heating (quantified as the surface precipitation rate) and divergent outflow in the upper troposphere is investigated, using ICON simulations of a real case of severe thunderstorms over Southern Germany. This builds on the author's idealised LES study already published.

Consistent with the earlier LES work, the present study finds that upper-level divergent outflow increases with latent heating, but increases more slowly as the organisation / clustering of the convective updrafts increases. This effect is found in 1km resolution simulations, but is absent from simulations at 13km resolution. It is suggested that resolving small-scale gravity-waves is key to representing the relative slowing of upper-level divergence with increasing convective organisation. The relation between upper-tropospheric outflow and convective momentum transport is also investigated.

However I have a number of fundamental questions about the theory, methodology and how the results are interpreted. I hope that in answering these questions, the paper can be made clearer and stronger. In its current form, it is not clear to me that all of the stated conclusions are proven by the results shown. So I would recommend significant revisions, if that is still possible at this stage?

I also note that this paper was originally submitted 9 months ago, so has been held up for a long time already (I understand one of the previous reviewers is now unavailable, which is why I have been invited to review the article so late in the process). Many apologies if my queries introduce further delay!

A breif summary of my main concerns:

- The divergence profiles shown in figure 6 do not vertically-integrate to zero for the 1km simulations, implying there is strong net convergence of mass into the defined boxes (see my point 6 below for detail). Whilst the model may solve the compressible equations, the magnitude of this mass imbalance does not seem realistic, which makes me worry that there has been mistake in the analysis?

- The method of defining regions of interest and calculating the mean upper-level divergence differs between the 1km explicit versus 13km parameterised simulations, so it is not a like-for-like comparison (the text implies that the 1km run divergence field was first filtered onto a horizontal scale significantly coarser than the resolution of the parameterised run, yet no filtering was applied to the parameterised run for consistency; also much larger boxes were used in the parameterised run).

- In various places the paper claims that the dependence of outflow strength on convective organisation cannot be represented when using parameterised convection. But the results don't support this conclusion. The organisation quantified using the fitting of ellipses to the precipitation field is largely on scales that are resolvable in the parameterised convection simulation. Any resulting gravity-waves connected with the mesoscale heating structure would similarly be resolvable. And the results in figure 8b show that turning off the convection parameterisation (while keeping all else equal) made no difference to the divergence-precipitation relation.

Below is a more-detailed full list of queries...

1. Introduction: $\sim$ L20 and onwards.

   A key argument of this paper is that the amount of upper-level divergent outflow driven by a given amount of latent heating within convective clouds varies as a function of storm horizontal structure. However, surely the total (time-integrated) amount of ascent (and hence upper-level divergence) is constrained to be that required to restore the heated air to neutral buoyancy? This is the basis of "Weak Temperature Gradient" scaling argument, which is key to the dynamics of moist convection and its interaction with the stably-stratified free troposphere. Latent heating will temporarily make the cloudy updraft air buoyant relative to its surroundings; it then ascends relative to its stably-stratified surroundings until it reaches its neutral buoyancy level, and the amount of upper-level divergence forced by this must be constrained by mass-continuity. If the time-integrated upper-level divergence was less than this constraint, then the cloud column would remain buoyant after ascent had stopped. This should only be possible if the resulting warm anomaly is held in place by a rotationally-balanced flow, which will not occur at convective scales.

   I think the paper's findings can be reconciled with this argument because it actually considers the *transient* response of the upper-level divergence to the latent heating, not the total *time-integrated* response. We would expect the upper-level divergence to continue after the latent heating has stopped, until the warm anomaly is removed, but this final stage of the outflow is not considered in this study.

So, the authors find significant variation in the transient *rate* at which the upper-level divergence spins-up in response to latent heating, as a function of storm structure. This is a very interesting result, especially the discrepency of this between different model resolutions.

I would just ask the authors to clarify that their results apply for the transient response and not the time-integrated total response of upper-level divergence to latent heating.

e.g. replace the often repeated term “amount of upper tropospheric divergence” with something like “instantaneous rate of upper tropospheric divergence”?

2. One factor not considered in this paper is the influence of the vertical structure of the latent heating profile on the mass divergence rate. If heating is concentrated in a layer of the troposphere with higher (lower) static stability, then less (more) ascent is required to restore the heated air to neutral buoyancy, and hence less (more) upper-level divergence will result. One possible hypothesis that might be worth checking is whether part of the variation in upper-level divergence per unit latent heating can be explained in this way by variation in the vertical structure of the heating profile, rather than storm horizontal morphology. It might be that the coarse simulations have deficient variability in the vertical structure, and hence give less variability in outflow strength.

3. L136: Why would it not be possible to represent the impact of storm morphology on upper-level divergence patterns in a simulation at 13km resolution with parameterised convection? Presumably the gravity-wave dynamics thought to mediate this impact could be at least partially resolved at this resolution, and represented by the model's dynamical core rather than the convection parameterisation? I similarly query the discussion at L439-440: "little or no information on geometry of the convective systems can be represented with a parameterisation"; surely much of the geometry (as quantified by the ellipse fitting in this study) is mesoscale, so occurs on the resolved-scale in the 13km grid PAR simulations. So the gravity-wave response to the storm structure could be captured. The results shown in figure 8b show that the divergence-precipitation relationship is not sensitive to the choice of parameterised versus explicit convection at 13km resolution, so the paper's argument that the overly-linear response in PAR is due to the convection parameterisation (e.g. L680, L710) is not supported.

   It might be that the PAR simulation simply fails to produce enough emergent mesoscale organisation on the grid (even though it could be resolved). However, figure 7(c,d) appears to show plenty of mesoscale structure in the divergence field for the PAR simulation.

4. L192: It is stated that the divergence fields in the PER simulation are low-pass-filtered to remove scales below 45km; is the same low-pass fil-

tering also applied to the divergence in the PAR simulations, to ensure fair comparison? Since PAR has a grid-size of 13km, it may have some variability at scales smaller than 45km, so should be filtered in the same way as the PER simulations?

Also, why is it nessecary to low-pass-filter the fields first, given that the analysis only considers the divergence spatially averaged over large boxes anyway? Is there a danger that the low-pass filtering may spread some of the divergence across the box boundaries, so that it is spuriously missed in the analysis?

5. L298 - 305: I am struggling to understand this paragraph (describing the identification of the convective systems in the PAR simulations); consider clarifying? It says that "corresponding precipitation moves together with conditionally unstable or lifted air masses" and "A typical (mesoscale) convective system is easily contained within a box of several to tens of grid cells in each horizontal direction for ICON-PAR" This seems to say that the PAR simulation should produce well-resolved regions of precipitation which move with the flow, but then the text says that only 3 static boxes are used to define the convective systems in PAR. Why not define moving boxes to track the systems, in exactly the same way as is done for the PER simulations?

6. Figure 6: Do the profiles of ensemble-mean box-mean divergence satisfy mass continuity?? In ICON-PER, it looks as though the vertically-integrated lower-troposheric convergence is significantly greater than the upper-tropospheric divergence. Assuming the rate of change in the mass of air contained in the box is small, we should have:

$$\int_0^{z_{top}} \rho \left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} \right) \mathrm{d}z = 0$$

and assuming small deviation from hydrostatic balance ($\mathrm{d}p = -\rho g \, \mathrm{d}z$):

$$\int_{p_{surf}}^{p_{top}} \left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} \right) \mathrm{d}p = 0$$

But if I try to vertically-integrate the divergence in figure 6 over the y-axis pressure coordinate "by eye", it does not balance and there is strong net convergence into the box (especially at earlier times). e.g. considering the time-averaged profile for PER (fig 6b, black line), we have convergence between 900 and 400 hPa averaging around 0.4×10^{-4} s^{-1}, giving:

5×10^4 Pa $\times 4 \times 10^{-5}$ s$^{-1}/g \approx 0.2$ kg m^{-2} s^{-1}

And we have divergence between 400 and 180 hPa averaging around 0.5 $\times 10^{-4}$ s^{-1}, giving:

2.2×10^4 Pa $\times 5 \times 10^{-5}$ s$^{-1}/g \approx 0.1$ kg m^{-2} s^{-1}

So the convergence and divergence seem to be out of balance by about a factor of 2. The mass imbalance of the order 0.1 kg m^{-2} s^{-1} would increase the mass of air in the box by $\sim$ 25% during the 7 hour period shown, which is clearly not realistic.

I can't see this imbalance in the divergence profiles for the PAR simulation (fig 6b, green line, has similar upper tropospheric divergence to PER, but much weaker lower-tropospheric convergence, so looks in-balance). Similar for the PAR divergence profiles in figure S5.

Does this suggest there is something wrong, either with the model or the analysis method in PER? Could the discrepency in PER be somehow due to the low-pass filtering?

7. Comparing figure 4 with figure 7, it appears that the boxes used to define the convective systems (and compute spatially-averaged divergence and precip rate) have quite different size in PER and PAR, so it is not a fair comparison? The example box in PER shown in figure 4 appears to be a square with side length about 1 degree in latitude ($\sim$ 110 km), whereas the boxes for PAR shown in figure 7 are rectangles with side lengths 2-4 degrees. The use of systematically larger boxes in PAR might risk including more of the background subsidence within the box, so that the spatially-integrated divergence is reduced?

8. L393 / figure 8: The text says that "from here on vertically integrated values (of upper-level mass divergence) are used". But the quantity plotted on the y-axis says it has units kg m^{-3} s^{-1}, which is presumably just $\rho\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}\right)$? Shouldn't the units be kg m^{-2} s^{-1} if this has been vertically-integrated?

   Also the lines shown in the figure for comparing PAR with PER (and the discussion around L435) indicate a correction to the divergence to account for the different thicknesses of the outflow layers, but this should not be needed if vertical integrals were plotted as suggested in the text.

   I would recommend calculating the vertical integral of mass divergence for both PER and PAR and comparing those:

$$\int_0^{z_{top}} L(z)\rho\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}\right)\mathrm{d}z$$

   where $L(z)$ is 1 in the outflow layer, zero at other heights. The result then has units kg m^{-2} s^{-1}, which can be compared more directly with precipitation rate (which can be expressed in the same units!)

   Also Re the discussion around L444 (where the text suggests that the apparent reduction in divergence-precipitation ratio for the 13km simulation with only shallow convection parameterised is due to the outflow layer extending lower-down); this could easily be remedied by plotting the above vertically-integrated divergence, and setting the outflow-layer mask

$L(z)$ interactively so that it always captures all model-levels within the time-varying outflow layer depth (instead of setting $L(z)$ to zero below a fixed height).

9. L537, L580, section 7.2.2 (relation between CMT and outflow strength); The study finds a positive correlation between momentum flux and upper-level divergence (with both normalised by precipitation rate). From this, they conclude that convective momentum transport has some impact on the upper-level divergence. However, correlation does not imply causality. It might be that upper-level divergence (or some aspect of storm structure which is correlated with it) has modified the convective momentum transport? e.g. more organised convective systems may have quite different pressure gradient forces acting on the horizontal momentum of the updrafts, and this will influence the CMT.

10. Minor corrections:

    - L59: missing reference (?) - maybe just need to rerun bibtex and latex?)
    - L230: Seems to say that u'w' and v'w' are *vertically-integrated* up to model-level 25 to obtain the vertical integral of CMT acceleration up to this height? Is this correct? Surely CMT acceleration is obtained by *vertically differentiating* the eddy fluxes u'w' and v'w'? Therefore, the vertical integral of CMT acceleration below level 25 is just the value of u'w', v'w' at level 25, and no vertical integration is needed? (table A1 seems to say that the analysis does indeed just use the mean values at level 25; consider clarifying this in the text?)
    - L247: typo "quantity" should be "quantify"?
    - L397: Typo "symbolsIn" (missing full-stop and space between sentences).
    - L477: says "Figure 9 suggests that low A generally corresponds with low D at high precipitation intensities ($>$ 6 mm/h)". However, nearly all the data-points in figure 9 with precip-rate $>$ 6 mm/h are in the same bin for A ($<$ 0.45), so any dependence of D on A in this region of the plot cannot be seen by the reader?