Review of egusphere-2023-664

Summary:
This is a study about deep convective outflow and differences between simulations with resolved vs. parameterized convection. Overall, I don't think this paper has the quality needed to be a journal article. I have a difficult time understanding the motivation, the methodology and the results. The text leaves me with an overall impression of having been crafted without much care and suffers from muddled scientific writing. I do think there is some worthwhile science hiding, but it would need to be brought out by completely rewriting the manuscript. I'd also recommend having a professional editing service go over the manuscript to improve readability.

Recommendation:
Reject

Paper Strengths:
The experiment set up is interesting and, when the manuscript is much improved, the results warrant publication.

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? These are very different concepts. In summary, the introduction is very weak and doesn't fit the purpose of the paper in my opinion.
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.
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.
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”)

5. Section 7: I am not sure how the “Dimensionality hypothesis” fits into this study.
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?

<u>Minor Comments:</u>

1. L1: What kind of event? This should be made clear (i.e., “convective event”)
2. L26+: You should specifically state the hypotheses. It’s unclear what you’re after.
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.
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?
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?
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.
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.
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.
16. Fig. 6a. The caption says “divergence-latent heating relationship”, but the figure shows divergence vs. precip rate. Please reconcile.

Editorial Suggestions:

1. L2: no comma after “both”
2. L132: local area model
3. L194: delete “mostly”
4. L319: space is missing before “Consequently”