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Interactive comment on "Simulating model uncertainty of subgrid-scale processes by sampling model errors at convective scales" *by* Michiel Van Ginderachter et al.

Anonymous Referee #1

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Review of "Simulating model uncertainty of subgrid-scale processes by sampling model errors at convective scales" by Ginderachter et al.

The authors present a methodology to diagnose model error associated with deep convection. They create a data-base of the diagnosed error and construct a stochastic parameterization of Monte-Carlo-type to sample the error to apply perturbations to their "target" models' total transport flux terms. While the paper is well written, and the construction of the database ensuring a vertical and cross-variable correlation provides some strength to the methodology, I do not recommend publication of this manuscript in its current form, as the study lacks justification of its basic theoretical assumptions,

mainly:

-The difference between the two model configurations is not the model error, but rather one representation of model uncertainty. At 4 km resolution, this uncertainty will pertain systematic differences between the two configurations chosen in this study, and sampling from the data base would mean consistently sample perturbations with the same systematic error. Using the differences between two configurations where one has a known systematic deficiency needs to be better justified, if at all possible.

-The main short-comings of this study is the computation of the "error" (uncertainty) itself. Here the authors turn off the deep convection parameterization and claim "it is assumed that the turbulence (together with shallow convection) and resolved condensation schemes might compensate for the absence of parameterized convective transport". And they proceed to compute the "error" as the difference in total transport (where one experiment is now missing the convective transport terms). This assumption is highly questionable. Just because there is no parameterized convection contributing to the transport flux of e.g. specific humidity, doesn't mean that there is no convective transport. In the "no parameterized convection" experiment this is now taken care of by the resolved dynamics, and the "compensation" discussed will be seen in the tendency of the dynamics. In fact, the authors do point out in the introduction that studies have shown that turning off the convective parameterization at ∼4 km can lead to unrealistically strong updrafts. What is the scientific justification for systematically adding a positive (or stronger negative) perturbation to the total transport when the convective transport is missing by construction, and is now resolved?

-The simulations should be made with non-hydrostatic dynamics, for the dynamics to be able to (have a chance) to realistically simulate vertical motions generated by convection.

-The perturbations are applied to the model considered the "target" forecast – which does not use a convective parameterization. Now you systematically introduce a larger parameterized convective transport in a run with resolved convection. This seem to imply that the scale awareness of the model impose a reduction of the resolved convective transport, such that the improvement that you see relative to the control run (e.g. Figure 9), basically comes from again implicitly “activating” the convective parameterization (by systematically introducing a larger convective transport in the physics parameterizations).

-Why the first time-step after turning off the deep convection parameterization is a representative time of the model uncertainty needs to be better justified. The uncertainty due to convection ought to grow as a function of lead time. Figure 3 simply shows total transport with and without convection.

-Another aspect that is rather confusing in the experiment design is why the study is constructed such that the perturbations are applied to the model configuration that has no deep convection at 4 km, when the operational model uses the convective parameterization? Wouldn't it be more desirable to create a perturbation scheme that could be applied to the operational ensemble system at that resolution?

-Lastly, the perturbations in the distribution are not applicable to any general model system, but tied to this very particular experiment setup, and thus does not provide a general guidance for development of stochastic parameterizations. What happens if the model is used at 10 km or 1 km? Which configuration is now considered the ‘perfect’ model?

I can certainly appreciate the effort going into this work, and I hope the work on the cross-variable perturbation technique, which is a clear strength of this paper, can be published in some form. If the editorial decision of ’manuscript revision’ is reached, I would suggest putting more emphasis on this aspect. I would also urge to change the underlying theory diagnosing the ’model uncertainty’ rather than ’model error’. The reason I do not suggest major revision rather than reject is that I find the experimental design and underlying assumptions very unsatisfactory. One way to add some

scientific justification to the study would be to have the target model being the current operational configuration, then the ‘perfect’ model would be a very high resolution (non-hydrostatic) convection resolving version that you coarse grain back to the 4 km grid. Here you can compute the ‘true’ sub-grid variability of the flux, and use this in your data-base to perturb the transport fluxes in the 4km run deep convection scheme. The method you propose is just systematically correcting a “flawed” system, there is no random error component involved.

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