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Nonlinear Processes in Geophysics
Discussions
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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.

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General reply to reviewers 1 and 2

The comments of the reviewers point out three main weaknesses of our manuscript:

- There is a lack of the description of the theoretical basis of the work. This was already addressed in our previously published Authors Comments 1 (AC1).
- The description of the sampling method is not sufficiently clear to the reviewers.
- The manuscript lacks a justification for the use of the model in a hydrostatic con-

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Discussion paper

figuration.

All of these issues can be addressed. The first two are a matter of redaction. For the third, we will perform non-hydrostatic runs. Since the sampling method is very computationally demanding, this will take some time and we can only provide a new version of the manuscript after these have been performed.

In order to clarify some misconceptions in the comments of the reviewers, we provide some detailed replies below. We also describe the issue of the use of the hydrostatic dynamics and the way how we will address it.

Theoretical basis

comments from reviewer 1:
"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."

and

"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?"

and

"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 âĹij4 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?"

from reviewer 1.

Reply:

This confusion originates from the fact that the theoretical basis has not been well described in the manuscript. We have already provided an online reply about this to reviewer 1 in AC1.

While it is true that in practice model-error perturbations should be added the the operational model (here with parameterization), the choice for using our *academic* setup, where the model error is artificially introduced by comparing a model-without-parameterization (target) with a model-with-parameterization (truth), has the advantage that the source of the model uncertainty can be singled out and its effect on the simulations studied in detail (see also our reply in AC1). We believe that this is an interesting example of how a specific process-related model error could look like and how it could be modeled. The academic nature of the work was the reason we chose to submit the work to NGP as opposed to more forecast-related journals as MWR and WAF.

We emphasize that we are looking for the sources of the model error. When a model-with-convective-parameterization and a model-without-convective-parameterization both perform one time step in the dynamics (starting from identical initial conditions) their results will be identical. It is only after applying the parameterization schemes that the results will differ. Considering the model-with-parameterization as the truth, the absence of a convective flux is thus by definition the (source of the) model error. In the model-without-parametrization, the absence of the stabilizing convective fluxes will, of course, trigger a response in the dynamics as the instabilities grow large enough to be resolved, creating the typically too strong updraft. Correcting for this model error at the source, by adding the stronger negative perturbations, will thus stabilize the air, preventing the growth of the instabilities leading to the too large updrafts.

The description of the sampling method

comments: “While I am likely confused by their narrative, the choose of the 250hP level as a reference for “sampling the grid column database” is not only not justified by the authors but it is also not accurate. This leaves behind all the convective activity which is associated with shallow clouds of cumulus congests and stratocumulus type. Tropical convective systems are known to involve a rather diverse population of cloud types and one needs to account for all of them in order to represent the life-cycle of organized convection.“

and

“The way the sampling is done is not at all clear. 1. Figure 5 has three distributions, which one is actually sampled. 2. Figure 8, has six fluxes how the two are reconciled? Are you sampled the distributions in Fig.5 or the "grid columns data base"?”

from reviewer 2.

Reply:
The database consists of grid-columns, these entire grid-columns are sampled based on moisture convergence or CAPE (both criteria are used separately). While moisture convergence and CAPE are both typical indicators for convective activity, their use as a sampling criterion is indeed introduced quite arbitrarily. The grid-columns from the database are sampled uniformly and the error fluxes will therefore by definition represent the distributions contained in the database.
The distributions on the 250 hPa level are, however, just an example of what the distributions look like and are not used for the sampling. We understand this might confuse the readers and will clarify this better in a future version.

comment:

"According to the authors, the whole argument for choosing to simple a flux-error database instead of the more or less established Stochastically Per- turbed Parameterization Tendency (SPPT) is rooted from the fact that the error fluxes associated with different variables are only weakly correlated (if they are at all). How- ever, the way they do the sampling while it does assume such correlation it makes it systematic since they sample the grid columns and not the different fluxes indepen- dently as illustrated in Figure 8."

from reviewer 2

Reply:
Concerning the comparison to the multiplicative perturbations used in SPPT, we only make the claim that, looking at the correlation between the flux errors and the corresponding fluxes themselves, there is no support to use multiplicative perturbations as is done in SPPT. We do not make any statements about the SPPT method itself nor do we use this as an argument for our method of using a simple flux-error database. Furthermore, this claim is not in contradiction to sampling grid-columns (keeping the inter-flux correlations), as there is a strong correlation between the different flux errors

(see table 1 in the manuscript).

comment:

“Page 14, lines 1-2: Why are you doing this? Aren’t the cases with zero or weak updraft part of the physics of the problem? This is clearly biased and it is not at all justified.”

Reply:
The idea behind doing this, is to remove the grid-columns with no convective activity (a vertical updraught flux of 0.5 Pa/s is much smaller than the typical updraught flux in columns with convective activity), since at these columns there is also no error present, to reduce the database to a manageable size. (Keep in mind that shallow convection is handled by the turbulence scheme in the ALARO model and is thus correctly represented in all configurations).

The use of the non-hydrostatic dynamical core

comments:
“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.”

from reviewer 1 and comment:

“The use of a hydrostatic model at 4km resolution needs caution–while I doubt that it can be justified, the authors are requested to provides a few words warning their readers that this is not at all realistic!”

from reviewer 2.

Reply:

The ALADIN System can be run both with a hydrostatic and a non-hydrostatic dynamical core (Termonia et al 2018). We have performed simulations for the setup and the test period of the paper both with the hydrostatic and the non-hydrostatic version and found that the difference are very small, see Figs. 1 and 2 at the bottom of this document where the RMSE of the three configurations (hydrostatic-with-deep-convection-parameterization (CP), hydrostatic-without-deep-convection-parameterization (NCP) and non-hydrostatic-without-deep-convection-parameterization(NH-NCP)) is compared for different variables (filled circles indicate significant differences w.r.t. the NCP configuration)

As the reviewers know very well running EPS systems is very computationally demanding. We have chosen the hydrostatic version since it is more stable and allows thus longer time steps to perform this research being convinced ourselves that the results would not substantially be different than for the non-hydrostatic setup.

We plan to apply the found stochastic method in the non-hydrostatic version of the model. While we are convinced that the results will be essentially the same, we will do so to remove any doubts that could be raised concerning this issue. This will take time. We will write a new manuscript where the final results will be presented for the non-hydrostatic setup.

specific comments

The specific comments will be addressed in a new manuscript after the non-hysdrostatic runs have been performed.

Interactive comment on Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2019-26, 2019.

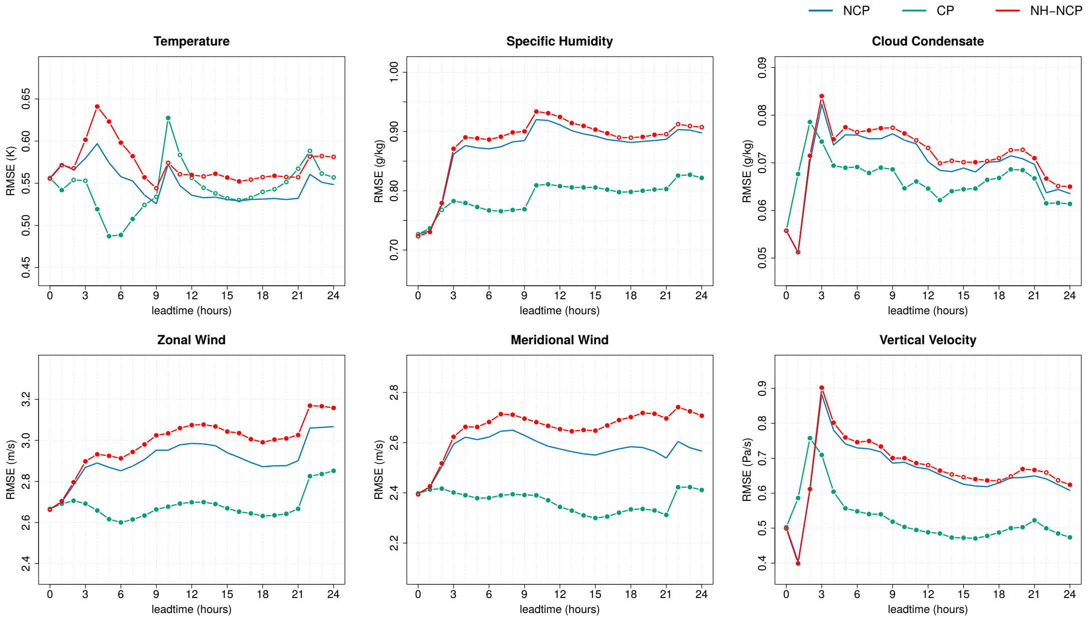

Fig. 1. 500 hPa RMSE of the different model output variables for the hydrostatic configurations with (green) and without (blue) convective parameterization and the non-hydrostatic configuration (red),

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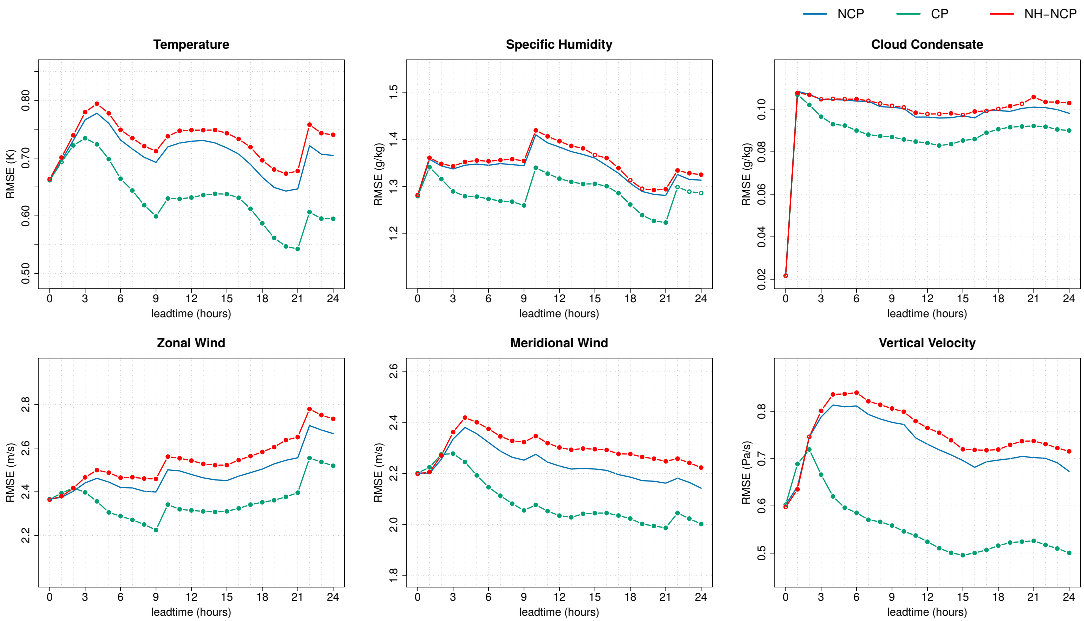

Fig. 2. 850 hPa RMSE of the different model output variables for the hydrostatic configurations with (green) and without (blue) convective parameterization and the non-hydrostatic configuration (red),