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Nonlinear Processes in Geophysics
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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 #2

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The author use a hydrostatic model at 4km resolution without and without a cumulus parameterization (CP) as a basis to construct a probabilistic cumulus parameterization which amount to sampling the flux error distribution between the model with CP and the that without CP. While the procedure and the whole concept is very simple and may seem appealing the paper itself is very poorly written in the sense it is very confusing and hard to really understand that the take-home message really is. This is addition to many other flaws and unjustified choices made in the study. The use of a hydrostatic model at 4 km and expected to represent convective flows in the tropics realistically is, forgive the word, an aberration. The small improved seen with the use of the stochastic are arguably due to this shortcoming more than anything else. It is

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well known that although too coarse to represent the details of individual clouds, CRM at few kilometre resolution (up to 10 km in some cases) represent well organized convection in the tropics; the Japanese did their first global CRM simulation at 7 km and got very realistic MJO, CCWs and MCSs. On the other hand it is also well known that the hydrostatic balances messes up gravity waves at scales of 50 km and less and a fortiori convective flows in this range. The way the sampling of the flux errors is done is not very clear. 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. According to the authors, the whole argument for choosing to simple a flux-error database instead of the more or less established Stochastically Perturbed 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). However, 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 independently as illustrated in Figure 8. The paper has many other incoherent statements to the point that it is not at all clear what the authors want us to learn from their study.

For these reasons and many others (in the specific comments), I personally cannot recommend this paper for publication.

Specific comments

Lines 20-25 of page: This paragraph is misleading when first reading it the following question came to my mind: "Something is not quite right. How can one compare fluxes between two different models that do not necessarily go through the same integral curve in the state space?" It is only after I got to page 4 that I found out that the authors are doing the right thing by comparing flux deviation after only the first step. This needs

to be stated before hand so not to confuse the reader. Line 15, Page 4: “Therefore, the retrieved model error should rather be seen as a lower bound on the error made in the representation of the physical process.” This can be interpreted as that the authors are trying to do better than the reference? This may not be possible since the direction of error can not be quantified in such a large dimensional state space! Page 4, line 27: 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! This is intact a serious flaw in this study. A quick look at the Gerard et al. reference reveals indeed that the cumulus scheme on which this study is based tries to represent non hydrostatic effects (their Eqn. 5), thus it is not surprising if the deficiencies in the NPC model are have more to do with the use of hydrostatic model other than anything else. Line 25, page 5: “This database is not only useful to investigate the statistics of the model error due to deep convection parameterization (Sect. 2.3), but it will also be the basis for a stochasticperturbation scheme that can be applied in an ensemble prediction system (Sect. 3).” Rephrase or delete the whole sentence. It adds nothing to the paper it can only confuse your readers. Isn’t the later statement the main objective of the study? Figure 2: This figure can be clearer. It took me maybe 5 minutes of staring at it before I could make a clear sense of it. The caption could be used to explain the labels and the color coding. Page 6, line 3: Aren’t 72 evaluations too few given there is a high level of correlation in space and time because of the nature of organized convection? Figure 3: What does the label error in red stand for? It isn’t clear at all. The red dots are hardly visible and they don’t constitute and error but their difference does. Maybe draw a red line segment between the two red markers to indicate the error. Page 6, line 5 âĂŤ- page 7, line 9: The discussion in these two paragraphs and Fig 3 seems to be included in order to make the final statement that "Therefore, the total transport flux difference one time step after the switch can be considered as a representative measurement of the error in the transport flux as defined in Eq. (1)." 1) This is empirical observation has no scientific value as such. 2) The model error as defined in (1) is only valid when evaluated at first

step because the states of the two simulations change in subsequent steps. Page 8, line 5: This is not a surprise because the model needs to conserve the water budget. Page 11, line 5: This is not a surprise at all because the model needs to conserve the water budget. Page 13, lines 13-14: 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"? If it is the later how are you doing it? Is it uniformly over all grid columns? Also Conditioning on the basic state would been more appropriate if one wants to genuinely emulate the cumulus scheme. Nonetheless the "success" of the completely random sample in reproducing the results implies that the cumulus parameterization is perhaps not sensitive enough to the environment, which may be problematic. 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. It undermines the role of shallow cumulus and cumulus congests clouds since your distributions in Fig. 5 are based on 250hPa errors. Page 14, lines 8-21: So you are using a convection trigger. Are the two criteria enforced simultaneously or are you using one at a time? Why these particular choices? How do they compare to what the original cumulus scheme does? Figure 9, caption: "Lead times where the ensemble mean RMSE is significantly lower than the NCP control RMSE at the 95 % confidence level are indicated with a filled circle." This is not clear that this is actually true. Maybe showing the absolute errors instead would be more clearer. In any case the difference between the compared errors is probably very small. What is the actual gain really is? Page 15, line 5: This may have something to do with perhaps the fact that you are sampling the flux errors at 250 hPa in Fig. 5. Page 15, lines 7&9: CPâĂŤ>NCP Page 15, lines 12-13: When and where the error in the reference configuration was it defined? You can't really tell since you are not comparing to anything else but the CP run. Please delete this sentence. Page 17, lines 4-7: This is in contradiction with the claim made upfront that a stochastic parameterization would increase the spread by accounting for model error. Page 17, lines 11-13: This applies to any ad hoc and non-

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physically based CP. Page 18, line 15: Where are you looking? Do you mean MOCON and OMEGA? Page 22, lines 5-6: What does this mean? Are the two models evaluated and compared elsewhere? If so please provide the reference and eventually say for which case study it was done. It makes a huge difference if that was done for tropical or non tropical convection site. Otherwise simply delete this sentence. It simply says that in the gray zone the role of a CP is unclear whether it is beneficial or detrimental and this is already known for many years. Page 22, line 7: This isn't true. Figure 16 actually shows the opposite. The NCP ensemble is better than the MOCON ensemble during the first 9 hours. Page 24, lines 4-5: " but for many variables it even outperforms the ensemble system with the deep convection scheme switched on." Where is this shown? Page 24, line 10: spell out EPS

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