

Interactive comment on “Introducing the Probabilistic Earth-System Model: Examining The Impact of Stochasticity in EC-Earth v3.2” by Kristian Strommen et al.

Anonymous Referee #1

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Introducing the probabilistic Earth-System Model: Examining the impact of stochasticity in EC-Earth v3.2 By Kristian Strommen, Hanna Christensen, Dave MacLeod, Stephaan Juricke and Tim Palmer.

General Comment: This paper compares different stochastic perturbation schemes for the parametrization tendencies in the context of the EC-Earth model. STTP, ISTTP and an stochastic parameter perturbation for soil model are compared. The impact of stochastic parametrizations upon atmospheric and coupled models is a highly relevant topic and this paper performs experiments towards the implementation of stochastic-coupled modeling systems. The paper shows the impact of these stochastic

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parametrization approaches upon a global model and particularly over the mean state over a relatively long simulation.

The paper needs to be improved in some important aspects before it can be published.

Major points

1) The order of the figures needs to be revised. (E.g. line 17, page 7, Figure 3 is mentioned before Figure 2).

2) Some choices in the implementation of the stochastic perturbation needs to be motivated. For example what is the motivation behind the 3 temporal and spatial scales associated to STTP and ISTTP (Line 8, page 4). Also why the amplitude of the multiplicative perturbation factor is tapered in the boundary layer (given that the PBL scheme is a source of the kind of model errors that the stochastic perturbations are trying to represent?). Another choice that is not motivated is the use of parameter perturbation instead of stochastic tendency perturbation in the LAND experiment (Section 2.3).

3) What is the motivation behind using the same perturbation for convection and large scale condensation in the ISPPT approach? The tendencies produced by these parametrizations are sometimes anti-correlated since when convection fails to remove instability from an atmospheric column large scale condensation tries to do that.

4) It would be good to provide more discussion about the pathways in which the stochastic perturbations can change the mean. I agree in that the impact of SPPT and ISPPT suggests that the convective scheme is activated more frequently, however the discussion on how the stochastic perturbations can lead to this is not clear (e.g. line 14, page 10). In the discussion section it is stated that some perturbations can trigger convection in areas in which the unperturbed state has conditions close to those required to activate the convective scheme. However the opposite is also possible, some columns in which the unperturbed state is sufficient for the initiation of deep moist con-

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vection can be perturbed leading to a state in which these conditions are not met any more.

5) As part of a first evaluation of the impact of SPPT, ISPPT and LAND upon the EC-Earth model it would be good to present some scores related to atmospheric circulation. Like for example MSE and biases for wind at different atmospheric levels and also for temperature at these same levels. The goal of the paper is focused on surface fluxes, but atmospheric circulation is also examined by studying for example the impact upon the Hadley cell. Although the impact upon the Hadley cell is relevant (particularly because SPPT and ISPPT seems to produce a large impact upon tropical convection), it would be good to provide these other scores for comparison with other systems.

6) It is not clear for me what is the motivation to study the QBO in the context of this paper. I understand that the impact upon different aspects of the atmospheric dynamics should be investigated but the inclusion of this particular aspect in a first evaluation has to be better motivated.

Minor points

1) Line 10, page 5. What does exactly mean that parameters are correlated? Estimated parameters based on observation studies show that the value of these parameters in different soil types and conditions are correlated or that the joint sensitivity of these two parameters shows a certain degree of compensation between the impact of these two parameters (i.e. the effect of the increase in one of the parameters can be compensated by changes in the other parameter).

2) Why performing 5 periods of 20 years each instead of a longer simulation. Using 5 different periods as ensemble members can artificially increase the ensemble spread and reduce the significance of the results. Also spin-up issues may be more important when several shorter periods are considered, particularly in the soil variables.

3) Line 29, page 4. Remove parenthesis and "for details".

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4) Line 29, page 6. Missing space before Spatial. Also indicate instead of indiate.

5) Since convective precipitation is part of the products generated by the convective scheme and is linked to the other tendencies, is precipitation rate perturbed in the same way as the other tendencies produced by the parametrization? Same question but for the large scale condensation scheme.

6) Line 6 page 14. More frequent convective scheme activation can also explain why the PBL is drier.

7) Figure 4 a, shows the biases in the precipitation for the control run. This bias pattern is strong and shows a clear maximum in the tropics. The authors indicate that the control configuration has been extensively tuned, however has the tuning been performed with this same model resolution?

8) Line 16, page 15. Changes in the Hadley cell are caused by changes in evaporation? Or these two changes are driven by changes in tropical convection?

9) Figure 6: The changes in T2m over the sea ice in the ISPPT and LAND are very strong. It is surprising to see these changes in both experiments since none of these experiments seems to directly affect the sea-ice parametrization in any way (SPPT for example do not show a strong change in bias in this region). I suggest to check the sea-ice distribution and temperature in these experiments.

10) Figure 11. I suggest to use the same names as in the rest of the manuscript.

11) Figure 8. Please correct the caption since the colors do not correspond to the ones on the legend (I assumed that the legend is correct).

12) Line 10, page 8. This sentence is not clear, I can not see "each model simulation" but something that seems to be the mean of all simulations.

13) It would be better to use the same color scale for all panels in figures 3, 4, 5, 6 and 7. In most cases the range is similar. Another possibility is to show in all cases the

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bias with respect to ERA (again since the magnitudes are similar this should clearly show the improvement produced by the stochastic schemes and would be more easy to analyze). Also in this figures indicate what “M=” stands for. I assumed that this is the mean bias over the global domain.

14) Since the main goal is to perform analysis towards the development of a coupled stochastic modeling system, why a SPPT+LAND or ISPPT+LAND experiments where not performed?

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