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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Comparison of stochastic parameterizations in the framework of a coupled ocean-atmosphere model" *by* Jonathan Demaeyer and Stéphane Vannitsem

Anonymous Referee #3

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This very nice work presents an in depth numerical analysis of two stochastic parametrization schemes, homogenization (aka MTV) and the recently proposed method by Wouters and Lucarini (WL), and their ability to be used to model unresolved scales in an underresolved model. The work uses a coupled ocean-atmosphere model of intermediate complexity, MAOOOAM, and the authors have made their code publicly available. Independent of the actual valuable results which the authors report on, the simple fact that the paper introduces this software package makes this manuscript in my view extremely valuable and will have some positive impact. This is very commendable.

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Discussion paper

The authors find that both parametrization methods are able to capture the empirical probability function of the full system. Their performance was found to be very sensitive to a good estimate of the correlation structure of the unresolved scales.

I have only some minor comments:

1) At the beginning of Section 3.1.2 the authors state that “These methods are applicable for parameterization purposes if the problem can be cast into a backward Kolmogorov equation.” This is not a correct statement: Every dynamical system (deterministic or stochastic) can be cast into a backward Kolmogorov equation. Casting a nonlinear dynamical system into the linear backward Kolmogorov equation (or alternatively the linear Fokker-Planck equation) allows for the machinery of perturbation theory of linear systems to derive explicit formulae for the homogenized equations. The authors may want to reference here the monograph “Multiscale Methods: Averaging and Homogenization” by Pavliotis and Stuart.

Furthermore, the authors provide references for the mathematical justification of MTV (including Papanicolaou). These are references if the underlying dynamical system is stochastic; for the applicability of the MTV procedure for deterministic systems, the references are Melbourne and Stuart (2011), Nonlinearity 24, pp 1361, Gottwald and Melbourne (2013), Proc. Roy. Soc. A 469, pp 2013020 and Kelly and Melbourne (2017), Journal of Functional Analysis 10, pp 4063.

The choice of notation with \rho for the backward Kolmogorov equation which propagates expectation values, is odd. \rho is usually reserved for densities. I also found the backward in time integration with the expectation value being defined at the final time cumbersome; why not have a positive sign on the left hand side of (25) and (26), and have the expectation value equal to f(X) at t=0? Also, the generator \mathcal{L} is not defined when it is introduced.

2) In the case when the unresolved scales consist of the wavenumber 2 atmospheric variables, the authors found that the WL approach leads to unstable dynamics. I am

surprised by that. The authors relate this to a cubic form. Can the authors comment on which assumption of WL is being violated by this choice of unresolved variables? Are higher-order corrections needed?

There are a few typos in the text:

Eqn (24): missing full stop.

Above (42): let -> left

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