Comparison of stochastic parameterizations in the framework of a coupled ocean-atmosphere model – Response to the 2nd reviewer

Jonathan Demaeyer [1] and Stéphane Vannitsem [1]

[1]Institut Royal Météorologique de Belgique, Avenue Circulaire, 3, 1180 Brussels, Belgium

We thank the reviewer for his/her comments on the manuscript and address his/her proposed suggestions:

1. The correlation plots (Figs. 4, 6 and 9) do not seem to be autocorrelations but autocovariances since the value at lag 0 is not 1. I think it would be easier to compare if the authors plot the autocorrelation function.

   Answer: Depending on the background, "autocovariance" can mean "autocorrelation". In this case, what the reviewer denotes as "autocorrelation" would be called "normalized autocorrelation". Besides these terminology issues, we believe that the "autocovariances" are more complete because they they illustrate both the time scale and variability properties of the variable.

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2. The are a few recent review papers on stochastic modeling. Including them in the introduction would make it more informative: Berner, Judith, et al. "Stochastic parame- terization: Toward a new view of weather and climate models." Bulletin of the American Meteorological Society 98.3 (2017): 565-588.
   Franzke, Christian LE, et al. "Stochastic climate theory and modeling." Wiley Interdis- ciplinary Reviews: Climate Change 6.1 (2015): 63-78.

   Answer: We agree on this point, they have been added in the introduction.

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3. In the introduction the authors seem to distinguish between stochastic parameteri- zations and backscatter schemes. Backscatter schemes can also be stochastic so can be similar. I also think stochastic parameterizations are implicitly also based on the idea to "backscatter" energy from the unresolved scales into the resolved.

   Answer: Our point was not to distinguish the backscatter methods as being non-stochastic but that these method are based on specific turbulence closure models which are different from the other non-specific method proposed. We have however restated in the text that we have stochastic version of these methods in mind:

   > *They provide promising alternatives to other stochastic methods such as the ones based on the reinjection of energy from the unresolved scale through backscatter schemes (Frederiksen and Davies (1997); Frederiksen (1999), see also Frederiksen et al. (2017) for a recent review) or on empirical stochastic modeling methods based on autoregressive processes (Arnold et al., 2013).*

Of course, every stochastic methods can also be seen as a kind of backscatter scheme, since it introduces energy onto the large scales, coming in principle from the unresolved small-scales.

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4. Page 2, footnote 2: I do not understand the meaning here.

   Answer: This footnote just enumerate a few of the underlying hypothesis used to develop parameterization methods. For more clarity, we have incorporated this footnote in the text.

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5. MAOOAM should be defined at first use.

   Answer:

   Ok.

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6. Are "weak coupling" and "time scale separation" equivalent in a mathematical sense? How would one measure weak coupling in the real atmos-ocean system? For me weak coupling is rather opaque concept whereas time-scale separation is more tangible (at least I know how to estimate this from real data). Some comments on this would be appreciated.

   Answer:

   This is a very interesting question. While in some setting, a kind of equivalency can be made between time-scale separation and weak coupling (see Wouters et al. (2016)). On more general ground, such equivalence is far from trivial, and research could certainly be done in this direction.

   Weak coupling is difficult to measure in data, and should rather be assessed by a proper modeling of the components of a system (dimensionalization). We agree that time-scale separation is easier to assess by computing the decorrelation time in data. We now comment at the end of Section 3.2 in the paper about that:

   > *In some particular cases, it is possible to establish an equivalence between these two assumptions (Wouters et al., 2016). However, in general, the relation between the two is far from trivial. Time-scale separation is easy to assess, by considering the decorrelation times in the output data of the model. On the other hand, weak-coupling is difficult to measure in data, and appears in general as a small coupling parameter resulting from the proper modeling of the components of a system.*

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7. In the "seamless MTV" procedure of Franzke et al. (2005) we generalized MTV and do not need to assume an Ornstein-Uhlenbeck process any longer but just one stochastic process with Gaussian statistics.

Answer: Once the formula have been obtained with an Ornstein-Uhlenbeck process, they can easily be reused in seamless way, considering Gaussian statistic. This is the kind of approach that have been taken in the article, considering the odd-moments of the $\boldsymbol{Y}$-dynamics to be zero. We comment on Appendix Section A2 about this:

> *The formulas presented here are valid for an Ornstein-Uhlenbeck process, but as stated above, the covariance and correlations of the dynamics (A2) can be used directly as well, provided that the right assumptions are fulfilled. In the present work, we have always used the statistic of Eq. (A2) to compute the tensors.*

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8. In Eqs. 36-39 are the coupling and time-scale separation parameters included. Which values have been used for the experiments?

Answer: Yes, and as stated on page 10 line 22-25:

> *In addition, the parameters δ and ε appearing in Eqs. (36)-(39) will be set to 1, meaning that we consider the natural timescale separations and coupling strengths of the model. Nevertheless, the study of the impact of these parameters is important (Demaeyer and Vannitsem, 2018) and should be carried out in forthcoming works.*

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9. (and 10.) In my work I use a split integration scheme: Runge-Kutta 4th order for the deterministic part and Euler-Maruyama for the stochastic part. Such a split scheme might solve parts of your numerical problems.

In MTV the cubic terms are nonlinear damping. This has been shown in Majda, Andrew J., Christian Franzke, and Daan Crommelin. "Normal forms for reduced stochastic climate models." Proceedings of the National Academy of Sciences 106.10 (2009): 3649-3653. Peavoy, Daniel, Christian LE Franzke, and Gareth O. Roberts. "Systematic physics constrained parameter estimation of stochastic differential equations." Computational Statistics & Data Analysis 83 (2015): 182-199. Perhaps one can also ensure that the cubic term in WL is negative definite then the system should be stable.

Answer:

Since the instability takes place in the deterministic integral part of the integro-differential equation, in the cubic terms, we do not think that it would solve the problem. In fact, with the same configuration and the same cubic terms, the MTV method does not diverge, showing that it is the nature of the WL equation which is a problem (integro-differential equation).

In addition, we think that the appelation “cubic terms” is more directly identifiable by the reader, but we mention now in the text:

These cubic terms are nonlinear dampings, as shown in Majda et al. (2009) and Peavoy et al. (2015).

We thank again the reviewer for his/her suggestions.

References

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