In the present paper the authors present an in-depth comparison of two methods of parameterisation. The methods compared are homogenisation as used by Majda et al. and the weak coupling method as developed by Wouters and Lucarini. The model used is the MAOOAM spectral coupled ocean- atmosphere model. Both the model and the methods used are relevant. The present work adds a considerably to the existing evidence of the efficacity of stochastic methods of parameterisation.

The model is used not very realistic, but it is at the upper boundary of the size of system that can currently be intensively studied numerically, so I consider it to be a good choice. The chosen parameterization methods are to my knowledge the only ones with a derivation from first principle, making them sound choices. Perhaps a comparison to a data-driven method would have further improved the work, but I don't consider this to be a necessary requirement.

Another drawback of this study is the assumption that $\varepsilon = \delta = 1$. It is encouraging that even with this rather crude assumption the methods show a good result, but it does make one wonder whether the shortfalls of some of the methods are innate to the method or are due to the assumptions made. Calculating the decorrelation time scales of the slow and fast dynamics and setting $\delta = \tau_Y \,/\, \tau_X$ would be a less crude assumption.

The metrics of comparison are appropriate and to my opinion constitute sufficient evidence for the conclusions the authors have drawn.

Based on the manuscript and the research scope of the journal, I believe this work falls within the desired scope of NPG and meets the review criteria.

I have a few minor suggestions that I list below as bullet points.

- p.2 l.22: over which → on which
- p.2 footnote: over which → on which
- p.3 l.5: mode → modes
- p.4 l.20: "It results": what does "it" refer to? Please clarify.
- p.6 eq.17: there is an ε and ε^2 missing in the second and third term on the RHS
- p.7 l.5: Vanden-Eijnden
- p.7 l.10: In term → In terms
- p.7 eq.23-24: note that in homogenization theory F_X and F_Y can also depend on both X and Y
- p.8 eq.32-33: The choice of decomposition is arbitrary without information of the size of different terms in the dynamical equation. If you agree, please insert a comment (here or elsewhere) that other decompositions may improve or decrease the performance. Could you also comment why you consider the assumptions of a time-scale separation or weak coupling to be valid in the current system?
- p.9 l.4-10: Either method should work as long as the fast dynamics has an ergodic invariant measure (with some extra assumptions). Please comment on the reason for choosing two different decompositions for the two methods.
- p.9 l.13-14: "we consider these two different assumptions": this is contradictory to p.10 l.27: "we will here consider only the dynamics of (34)".
- p.10 l.1: please add a few words explaining the problem of "dead" scales

- p.13 l.6: setting → settings
- p.30 l.12: "We will also assume...": this is not an extra assumption if you already consider a Gaussian Ornstein-Uhlenbeck process.
- p.30 eq. A5 and A7: the $1/\delta^2$ factors here should be 1
- p.31 l.17: the backward KE describes the evolution of expectation values, not densities
- p.32 l.5-6: incomplete sentence
- p.32 footnote: Additional to the diagonality, one needs assumption A4 of Majda et al. (2001)
- p.34 eqs A34: check the factors $1/\delta^2$
- p.36 l. 11-12: Have any tests been performed to conclude that the process can be approximated with an Ornstein-Uhlenbeck?