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Geoscientific Model Development Discussions

# *Interactive comment on* "A generic approach to explicit simulation of uncertainty in the NEMO ocean model" *by* J.-M. Brankart et al.

**Anonymous Referee #1**

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Review

This paper describes a strategy for extending a widely used ocean general circulation model to generate probabilistic simulations; a few examples are provided. I found the article well written, and covering a very important work that is usually neglected, and that is fundamental for data assimilation, predictability studies and operational oceanography. Providing a tool for generating perturbation in a community model is a very appreciable effort, which in the future will ease ensemble generation in a variety of different contexts. The article is mature in my opinion. I therefore recommend it for publication after a few changes are considered, mainly to improve its readability for a generic public.

Specific Comments



1) I found nice the concept of separating the "model world" (A) and the "real world" (B) in Figure 1 and Intro. However, it seems to me that the sketch is not complete. First, "small" vs "large" scale can be easily confused in resolution dependent definitions (eg mesoscale vs basin scale, respectively), while - as the authors write - the main point is the separation between the spectral window of the resolutions resolved and unresolved, which holds also for eg eddy-resolving models. Second, there is a variety of other uncertainty sources (parametrizations, numerical schemes, dynamics, external forcing in general, machine accuracy etc. etc.) that do not enter the sketch. I suggest to make the separation between A and B as generic as possible, accounting in principle for all sources of uncertainties.

2) I appreciate that the authors warn the users that perturbation code is not plug-and-play, in the sense that tuning of the perturbation parameters is a necessary intermediate step. Nevertheless they already bring ideas that can be generalized. For the smaller scales, they use a higher resolution configuration to estimate the effect of neglected small scales. Similarly, the impact of biogeochemistry perturbations may be achieved through comparison with a BGC model with greater number of species, or the sea-ice perturbation tuning through comparison with a model with a greater number of ice categories. Perhaps the authors can generalize this concept.

3) The formalism of Equation 5 and 7 is a nice sketch. However, there is in principle an analogous when the fluctuations concern the forcing (eg M(x,u+du,p,t) ). This is straight forward to achieve in an ocean model context, eg perturbation of atmospheric forcing, river runoff, batymetry (as a forcing to barotropic circulation for instance) and it is usually performed eg in ensemble ocean reanalyses. I think it is worth mentioning for completeness.

4) Intro and Conclusions. The authors claim the importance of ensemble generation for weakly constraint data assimilation, but in principle I would not limit to this case: imagine a data assimilation with ensemble-derived background-error variances but analytical cross-variable balances, this is strongly constrained in the sense that multivari-

ate balances come from a “perfect model”, but the system still requires an ensemble of realizations to estimate variances.

5) I found the term “unresolved diversity” slightly misleading sometimes, because in the context of BGC models it may mean “unresolved biogeochemical diversity” or in the context of sea ice model “unresolved ice category diversity”, while in facts the authors span the uncertainty of the model parametrizations (eg uncertainty in the SMS terms, or ice strength P*) through the SPPT approach. I suggest to make this concept clearer eg in Section 2.4 and 3.2. For me the key point is the uncertainty coming from simplifications in models, perhaps “unresolved diversity” is not generic enough.

Technical Comments

P616L2 not that “simple”, I would just take out this humble adjective

Throughout the manuscript references are often not in brackets as they should be

P623L5 better to specify the “ECMWF ensemble forecasting system” since “operational” may be confused for deterministic

P623L20 : the acronym “SPPT” should be defined at the beginning of the Section

P626L12 : “gradient”

P628L9: “all” is repeated twice by mistake

P631L1 : “understand”

P632L5: it seems the P* perturbation leads to increased thickness systematically, does this come from the fact that simplified sea-ice model do not bring info on sea-ice age, thus under-estimating the thickness in the perennial sea-ice interior? Would be nice if the authors can comment on this

P632L17: “comparison”

P635, Algorithm 2: it seems that “initialize seeds for random number generator” is done



before eventual reading of seeds from restart file, at the end of algorithm

P636 Lines 7 to 14: I found this discussion proper for the “Conclusions” section and not for the Appendix, and suggest moving there these lines.

Figures 2 and 4 : labels should report what the individual panels show (currently only in the text)

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