



Interactive comment on “Addressing the impact of environmental uncertainty in plankton model calibration with a dedicated software system: the Marine Model Optimization Testbed (MarMOT)” by J. C. P. Hemmings and P. G. Challenor

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We thank Philip Wallhead for his constructive comments. We have addressed the specific points raised as described below.

(1) The dw_p/dz term was included in error in Eq. 3. Vertical divergence in the flow should indeed cancel with horizontal terms from Eq. 19 by fluid continuity. Both equations have been corrected and the phrase about neglecting spatial variation in the flow has been removed.

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(2) The literature review material related to cost function weighting has been moved so that it appears before any description of the method.

(3) The clarity of the section that included Eq. 13-15 has been improved. Eq. 15 that included the parameter variance term was not used directly and so probably misleading. It has now been removed. We now state explicitly which errors are considered as random variables in which applications. Our assumptions of zero mean error and normality are also now stated explicitly. The assumption of zero mean in particular is acknowledged as a potential limitation. Dependency between ϵ_{ENV} and ϵ_P is an important issue that does indeed compromise Eq. 13. However, this form of the equation for the simulated state is useful if the errors can be considered in some sense separable. Dependency is addressed in the twin experiments. Our approach is to attempt to separate the error contributions by allowing for uncertainty in ϵ_P when estimating the variance for ϵ_{ENV} .

Eq. 16 is applicable for describing the goodness-of-fit for a model having fixed structure and a fixed parameter set, as well as being a reduction of Eq. 17 for zero structural error. Its expected value is 1 for a perfect model (i.e. if $\epsilon_P=\epsilon_S=0$). It can be minimized by changing the model with respect to its structure and/or its parameter set. This is what was meant by the phrase ‘minimize the model error variance’. Eq. 16 is not intended for use with the true forcings.

(4) The text has been corrected.

(5) This section on other weighting considerations is important background information but did interrupt the flow. It has now been moved and merged with the related literature review text referred to in (2) above.

(6) The growth of error variance at BATS in particular cannot be just due to forcing error accumulation because it extends way below the depth of the ensemble maximum in the mixed layer depth. Forcing variance may play more of a role at OWS-INDIA and the test has been modified to acknowledge this.

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(7) For the purposes of our twin experiments, the ranges of the parameter values were chosen arbitrarily. We have not investigated varying the ranges. More consideration would need to be given to this issue in a real-world experiment.

(8) The broader discussion of MarMOT has been shortened and placed in a separate sub-section at the end of the discussion.

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