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Comment

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 the reviewer for their valuable comments. Our response to individual comments is described here.

Page 1949: We agree that parameter penalty terms in the cost function are valuable from a Bayesian perspective. They are also helpful in providing additional constraints in many experiments. Our intention had not been to criticize their use but to indicate they were not necessarily required and that there were pros and cons associated with

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them. The text has been revised as suggested to make the discussion more balanced.

Page 1950, Eq. 3: The term including the vertical velocity divergence was included in error: it does indeed drop out because the velocity divergence terms sum to zero as a result of fluid continuity. (The actual simulations were correct.) We have taken the opportunity to provide a more general form of the equation in which it is not assumed that the active velocity relative to the water w_i is constant. A vertical divergence term is included for this velocity since fluid continuity does not apply. This is preferable to just dropping the term as it allows us to make a clear distinction in the text between the treatment of w_p and w_i .

Page 1952: Lateral processes are taken to be the cause of perturbation, as the reviewer suggest they should be; there is no causality associated with the relaxation term. The relaxation term is used in a corrective sense to “nudge” the solution towards a relevant estimate of the local state (e.g. climatology) in the absence of other information. It does this because advective perturbations tend to reduce the relevance of the local state provided by the model in response to local physics, thereby reducing its quality as a state estimate. There is not assumed to be an unperturbed pool of surrounding water. We are combining different estimates of the local properties. The rate at which this should occur for a given scale of perturbations depends on the relative quality of the two estimates and is determined by the relaxation control factor parameter ψ . ψ controls the significance of the relaxation tendencies relative to the perturbation tendencies. The description of combined perturbation and relaxation for real-world state estimation has been clarified (now in an appendix).

Page 1958: We have changed the text to indicate clearly that the observation error ϵ_{ijkOBS} refers to the sum of measurement error and representativeness error.

Page 1959, Eq. 15: The review of Stow et al 2009 contains a good introduction to cost functions in particular, as well as model-data error in general, and is now cited in our revised section on cost function design.

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Page 1959, Eq. 16: The cost function is indeed based on a commonly used variant of the chi-squared statistic. The expected value of 1 for M_{ijk} is intended to imply expected value over an infinite number of samples. (M is now replaced by the complete cost function J for clarity.)

Page 1959, last line: It is true that the structural error in particular is difficult to estimate. We acknowledge the issue as a problem worthy of future research. It is not possible to give much in the way of further guidance within the scope of this study, other than to suggest that progress might be made even if structural error is neglected. The reasoning is that there is often not a clear conceptual distinction between parameter error and structural error in plankton models. Therefore, allowing parameters to compensate for structural error is not necessarily a problem. This point has been included in our revision.

Page 1960, line 11: Text clarified as suggested.

Page 1963, paragraph beginning on line 16: Text clarified as suggested.

Page 1964, line 1-2: Horizontal flux divergence is parameterized in terms of monthly means and anomaly standard deviation for shorter time scales. It is only the anomaly part that is treated as uncertain in our twin experiments. The text was confusing and has now been clarified.

Page 1968, line 10: The text describing the “maximin” criterion was incorrect and has been changed to read “. . . the hypercube design is selected that maximizes the smallest Euclidean distance between pairs of sample points”.

Page 1989, references: The numbers following the years do not form part of the text of our submitted manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1941, 2011.

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