**Geoscientific
Model Development
Discussions**

# *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

**P. Wallhead**

phil.wallhead@legos.obs-mip.fr

Received and published: 8 September 2011

I think this is an important contribution on an understudied subject, but it could benefit from some modification, mainly trimming. Generally the paper seems to have two aims: first to address a scientific question (how to account for environmental uncertainty in plankton model calibration), and second to publicise a new software system (MarMOT). I think it would be far better to treat these two aims in two separate publications. This would allow a more concise treatment of the science question (which transcends any particular modelling environment), making reference to MarMOT in the Methods, whilst

leaving a complete exposition of MarMOT for elsewhere.

Specific comments:

1) In equation (3), shouldn't the dw_p/dz term cancel with similar horizontal terms by fluid continuity? Then equation (19) should be -u_h.grad(C^1/2), whereupon equation (18) would give the correct contribution -u_h.grad(C) without having to neglect spatial variation in u_h.

2) Section 3.1. Literature review seems to have leaked into the Methods sections, rather than be confined to the Introduction/Discussion.

3) Equations (13-15). If the epsilons have variances, they must be random variables. If the error due to parameter error, eps_P, is a random variable, the parameters must be defined as random variables. However, an assumed prior distribution of the parameter values is never defined. A non-Bayesian interpretation is possible if we assume that (13,14) applies after the parameters have been fitted. Then the error due to residual parameter error would be a random variable, but it would not be independent of the other errors. Whatever the interpretation of eps_P, the additive decomposition of eps_ENV and eps_P is dubious because the forcings affect the output via the parameters.

A more conventional formulation would omit eps_P whilst keeping the environmental, structural and observational errors as random variables. Under this modelling hypothesis, assuming that the random variables are independent, Gaussian, zero-mean with known variance, and that the parameters are fixed, unknown constants, equation (17) gives the criterion for maximum likelihood estimates of the parameters. A noteworthy assumption is that eps_ENV has zero mean – errors in the forcings could easily bias the model output. On the other hand I cannot see a statistical justification of equation (16) except as a reduction of (17) for zero structural error. It is justified in the text as an attempt to 'minimize the model error variance'. Perhaps this means to minimise the total variance between the truth and the fitted model with the true forcings inserted (replacing those used in the fit – see p1974, line8). However it is not clear why this skill

criterion should be optimized by minimizing equation (16).

4) p1959, line19: 'data give us...'

5) Section 3.1.2. Is this section really essential to the argument of the paper?

6) p1967, line23. Couldn't the growth of error variance be explained just by the accumulation of forcing error?

7) p1968, line 7. How are the ranges of the parameter values (limits of the hypercube) chosen? This could be a critical aspect of putting the method into practice. Are the results robust to varying these ranges?

8) The Discussion could be much reduced by excluding the broader discussion of Mar-MOT (5 paragraphs).

Bottom line I think that the scientific content of this paper is novel and important, but the presentation could be better focussed to aid clarity and breadth of readership.

---