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Ocean Science Discussions

# *Interactive comment on* "Estimation of positive sum-to-one constrained zooplankton grazing preferences with the DEnKF: a twin experiment" *by* E. Simon et al.

**Anonymous Referee #1**

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This paper investigates the estimation of zooplankton grazing preferences from chlorophyll observations using twin experiments performed with the ensemble Kalman filter. Uncertainties in the parameters represent an important source of errors in ecosystem models, and any progress towards the estimation of unknown parameters from the available observations is valuable to the ocean community. Moreover, grazing preferences are positive and sum-to-one constrained parameters, which is incompatible with the standard formulation of the ensemble Kalman filter (even with anamorphosis transformation). The authors thus propose original and interesting developments (using two kinds of change of variables) to solve this problem. Nevertheless, I found that many mathematical developments are particular to specific choices that the authors made in



the definition of the prior statistics. I therefore believe that the paper could be substantially improved by separating more clearly what is general (the transformations) and what is specific (the prior statistics for the parameters), and even by removing some secondary mathematical digressions (see main comment below). I am confident however that this can easily be taken into account in a revised version of the manuscript, so that the paper can be published in Ocean Science.

**Main comment**

I think that the methodological development would be more understandable by more clearly separating what is general and what is specific. In my view, the most general things in the method are the changes of variables given by Eqs. (3), (4) and (5), transforming the grazing preferences $\pi_i$ into the new parameters $\phi_i$. Such a transformation is the basic idea allowing to apply the ensemble Kalman filter to positive sum-to-one parameters (and maybe also to a broader class of problems, see comment 1 below). Then, the authors make quite specific assumptions about the prior probability distribution for the parameters: (i) the prior probability distribution is specified for the transformed parameters $\phi_i$, and the prior uncertainties on the $\phi_i$ are assumed independent; (ii) the original parameters $\pi_i$ have equal expected values. Everthing that follows Eq. (6) and all developments in Appendix A depend on these two restrictive assumptions. Then, as an additional parameterization, the authors assume that (iii) the prior probability distribution for each $\phi_i$ is a triangular distribution [with one free parameter that is tuned so that the condition (ii) is verified]. Eq. (14) and all developments in Appendix B depend on this additional parameterization. To make the paper more understandable, this sequence of assumptions should be made clear to the reader from the very beginning (in section 2.3).

a) In section 2.3, I would present assumptions (i) and (ii) as just one possibility to define the prior probability distribution for the parameters. For instance, it would have been possible to make any kind of assumption for the prior distribution of the original parameters $\pi_i$ (e.g. a trunctated Gaussian or any other distribution verifying the constraints), sample this distribution to obtain an ensemble for the $\pi_i$, and transform the ensemble using the inverse of the transformation in Eq. (5) (which can easily be obtained, see comment 2 below). I understand that, in this way, the joint prior probability distribution for the $\phi_i$ could not be made perfectly Gaussian (with anamorphosis). But the positiveness and sum-to-one constraints would be verified all the same. Why is it so important that the prior distribution be so perfectly Gaussian, whereas it is never as perfectly verified for the biogeochemical variables? Don't you believe that assumptions (i) and (ii) may be a high price to pay for this?

b) Since the authors used the assumptions (i) and (ii), I think that the Appendix A can be kept in the paper, but I would urge the authors to simplify and clarify the mathematical developments as much as possible. An alternative would be to remove the appendix, and to derive directly Eq. (14) from the condition (ii) using the transformation (5) and the distribution (13). The only consequence would be that the user would have to redo the computation of the expected value for the $\pi_i$ for any other assumption (iii) [which is not necessarily more difficult than computing the characteristic function in Eq. (6)].

c) The paper would be more understandable if the assumption (iii) [i.e. the paragraph with Eqs. (13) and (14)] was moved at the end of section 2 (as the particular choice that is done in the application). Actually, it is only when I saw Eqs. (13) and (14) that I understood the purpose and the meaning of Eq. (6).

d) Appendix B is not useful and should be removed. The purpose of the appendix is to show that a solution to Eq. (6) [condition (ii)] exist in the particular case of a parametric triangular distribution [condition (iii)]. And the result of the appendix is that a solution exists for less than 3 parameters, which amounts to saying that Eq. (14) [a simple equation without any parameters] does have a solution. I think that this is a very small mathematical detail that should not be published in an oceanography journal.



**Other comments**

1) It would maybe be useful to say somewhere that the method could be easily generalized to variables constrained inside any triangle (in the plane) or any pyramid, using an additional linear tranformation.

2) It would also be interesting to say somewhere that Eq. (5) is easily (recursively) invertible.

3) The same kind of constraints on the parameters could also be taken into account using a truncated Gaussian assumption, as described in Lauvernet et al. (2009), with the constraints: $\pi_1 \geq 0$, $\pi_2 \geq 0$, $\pi_3 = 1 - \pi_1 - \pi_2 \geq 0$. It would be interesting to give the relative advantages of the two methods. I would say: more generality in the inequality constraints in the work of Lauvernet et al. (any set of linear inequality constraints), and more freedom in the specification of the prior probability distribution for the parameters with this method.

4) p. 1091, l. 6–13: This paragraph is not very clear. Please clarify your statements.

5) p. 1096, l. 10: I see no reason to mention that Matlab has been used to solve Eq. (14), since it can be solved by elementary root finding methods, and since everybody can easily verify that the values provided are solutions of the equations.

6) p. 1097, l. 8: I think that the asymmetry of the transformed parameters is a possible difficulty of the method, in particular regarding the assumption (i) above. A word of caution would be welcome.

7) p. 1098, l. 14–19: The purpose of the ensemble described here is not obvious. Please reorganize the explanation.

8) Figs. 3 and 4 are too small to be readable in the printed version of the paper. Please enlarge the fonts or split the figures to make them larger.

Interactive comment on Ocean Sci. Discuss., 9, 1085, 2012.