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Biogeosciences
Discussions

Interactive comment on "Optimizing models of the North Atlantic spring bloom using physical, chemical and bio-optical observations from a Lagrangian float" *by* W. Bagniewski et al.

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Results of three different biological models, which all resolve plankton production and organic matter export, were examined with respect to their ability to reproduce 1) data collected by a Lagrangian float, 2) ship-based silicate measurements, and 3) data from floating sediment traps. The study of Witold Bagniewski and co-authors provides an interesting data-model synthesis, covering a fairly large region south of Iceland (North Atlantic), where the float operated for a period of 51 days (from April 4'th to May 25'th). Measurements from three ship cruises complemented float data but were also used for calibration of the float sensors. The three different biological modules were coupled to the General Ocean Turbulence Model whose physics was nudged to temperature and salinity observations with a nudging time scale of 6 hours. All three biological model setups were thus subject to physics close to reality and were run for a period of 34 days (April 20'th to May 24'th). Distinctive features of this study are the optimisation of model parameters to identify best model fits to data and subsequent sensitivity analyses that provide posterior error estimates of the optimised model parameter values.

The text is well written and comprehensible. At first glance I was deeply impressed by the approach of the study and the overall aims. In retrospect, I must admit that this initial impression faded away, the more carefully I read the discussion paper. A major revision is necessary to sort out a series of concerns that equally address model structure and the assessment of parameter uncertainties. The idea and the general approach deserve special recognition, as this study deals with data and their value for constraining model parameter values. This is rarely done. On the other hand, the primary conclusions inferred from the model comparison are unacceptable. In the following, these concerns will be described while referring to the three aims written on page 8482, at line 8: "Our aim was to find the optimal biological model variant for simulating the bloom and quantifying the associated carbon export, and to investigate the importance of including diatom aggregation triggered by low silicate concentrations."

1 General comments:

1.1 "Our aim was to find the optimal biological model variant for simulating the bloom..."

Parameter values were optimised for each model version while minimising a cost function that describes the misfit between data and model result. The cost function requires the specification of error/uncertainty of data. According to the author's description, the cost functions included constant errors for each observational type, which is fine. But in their analysis, these constant errors seem non-identical between the different model versions. The authors explain that these error weights were first derived from the initial data-model misfit (of each observational type) and then used for minimisation (page 8489 and page 8490). If this was the case, the weights applied to the individual cost function terms (representing each observational type) are also sensitive to the goodness of initial parameter guesses. Thus, a model assessment according to the cost function's minima would be invalid, because the cost functions include different weights for identical observational types. In other words, the cost functions are not the same, as they depend on model version and initial parameter guesses. For assessing the optimised models it is mandatory that the relative error weights remain identical for the different models. Different cost function weights strongly affect the posterior error estimates, which were calculated as the inverse of the second derivatives of the cost functions. I hope that this issue is just a matter of misunderstanding due to an unclear description in the text. To include a table where the error weights are explicitly listed is therefore needed for clarification.

The authors explain that the minimisation failed for a series of different initial parameter guesses. I experienced this myself but here it may actually be related with a poor specification of the error weights, as discussed before. To vary initial parameter guesses is very good but it is usually associated with large variations of cost function values as well. Provided that the procedure for assigning weights to the cost function was done as currently described by the authors, the cost function's sensitivity to parameter variations can become artificially reduced - with the consequence that the optimisation algorithm fails to descent to the minimum. The authors write that the minimisation failed because of local minima. Whether local minima existed or not is very difficult to assess. The authors assume that there were some local minima, but it might simply be a failing in convergence because of poor cost function weights.

To compare the cost function values between the models is not ideal for model selection in particular when they vary in number of parameters. Although it was stated that the models differ in terms of degree of freedom, the authors did not consider this in their evaluation. This can easily be corrected by calculating an information criterion (Akaike or better Bayesian) where the number of parameters and (in case of the Bayesian) the number of observations are also included.

1.2 "...to find the optimal biological model variant for ...quantifying the associated carbon export..."

The simplest model (1p1s) describes a single phytoplankton group, two nutrients (DIN, Si), two types of detritus (Det_N and Det_{Si}) and zooplankton. The extended model (2p1s) explicitly resolves the diatom biomass. Both models differ in that the 1p1s assumes zooplankton to graze on the entire phytoplankton pool (including diatoms) whereas in 2p1s diatoms are not grazed at all. During the onset of the bloom the entire phytoplankton biomass is subject to grazing whereas in 2p1s a substantial fraction of the entire phytoplankton pool (namely the diatoms) escapes any possible grazing pressure from the beginning. Both models differ fundamentally and 2p1s cannot be interpreted as an extension of 1p1s. This difference is important as it points towards the difficulty in specifying the relative amounts of grazing versus formation and export of aggregates. The "non-grazed" diatom biomass of 2p1s is transformed into a detritus pool that was allowed to sink faster (additional degree of freedom) than the detritus pool of 1p1s. It is no surprise that this model version yields results with increased export. The interesting question here actually is: which *small* refinements are needed for 1p1s to better reproduce the data? Instead, the authors proposed two alternative model versions (2P1s and 2p2s) that differ fundamentally from 1p1s.

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1.3 “...to investigate the importance of including diatom aggregation triggered by low silicate concentrations.”

The 2p2s model can be interpreted as an extension of 2p1s. The export dynamics of model 2p2s must be extremely sensitive to the choice of the critical silicate concentration (Si_{Cys}) where all biomass is transferred instantly to a cyst-detrital pool that sinks fast. I expect essential difficulties when introducing such a “switch”. What happens when silicate concentrations fluctuate around this critical silicate concentration? The strong non-linearity that was introduced to 2p2s can be avoided by introducing a weak but effective non-linearity, e.g. quadratic terms for the transformation of phytoplankton (including diatoms) to detritus. In my view, this alternative approach needs to be tested as well, because I expect that solutions similar to 2p2s can be found after optimisation. Currently, the assumptions that enter the 2p2s (and 2p1s) model exclude important aspects like grazing of diatoms in general. I always thought that low silicate concentrations make diatoms more vulnerable to grazing because frustules become thinner. Were meso-zooplankton like *Acartia* observed? With the information presented in the current version, the superiority of 2p2s, if compared with 1p1s, is not unambiguous.

2 Specific comments:

page 8478, line 16: How much larger is the export flux?
page 8470, line 23: Why are the cysts assumed to be so important for the export flux? What was the motivation for resolving them in one of the models?

page 8481, line 8: “...ranging from three to up to thirty biological variables” and even more.
page 8481, line 26: I had to learn myself that manual (or intuitive) tuning of parameter

values is just as valid as a systematic approach.
page 8482, line 1: To find an optimal parameter set is not exclusively restricted to a systematic approach. A quantitative measure for the misfit between data and model results can also be calculated for "hand-tuned" model solutions.
page 8482, line 9: "...the bloom..." do you mean *the* North Atlantic bloom? Please explain why the location south of Iceland representative for the bloom in the North Atlantic?

page 8485, line 10: Evans and Parslow (1985) provided an analytical solution for equation (5), based upon few assumptions. Is the analytical solution for equation (5) used? Equation (5) is a function proposed by Smith (1936).

page 8489, lines 5ff: The distribution of PON among the different model state variables is confusing. When using the "adjustable" parameter values listed in Table 3, 95% of measured initial PON is distributed between the model variables in 2p2s, 80% in 2p1s and only 78% in 1p1s. What happened with the remaining PON? Why not using the constraint: $(1-r_{dead}) = r_{zoo} + r_{phy} + r_{dia}$?

page 8489, lines 20ff: As mentioned above, this paragraph needs clarification.
page 8490, line 15: How small was δp chosen?
page 8490 , line 17: "...from randomly chosen initial parameter values..." I did not find any comment on the variational range.
page 8491, line 10: "w_{cys} was set to -55 m d^{-1},..." But it varied between -50 and -100 m d^{-1}.
page 8491, line 19: "We approximated the Hessian..." Here, the reader gets the impression that the full Hessian matrix was approximated. Better write: "We approximated the diagonal elements of the Hessian...".

page 8492, line 11: Were F+F_R used for the Hessian diagonals (of 2p1s and 2p2s) or only F?
page 8492, line 12: "..., again indicating that the 2p2s model gives the best fit". Use information criterion and you will find a slightly different conclusion.

page 8495, lines 16ff: "In the 2p1s and 2p2s models, zooplankton concentrations decrease during the first half of the simulation, when phytoplankton concentrations are still very low, which seem unrealistic." Well, in 2p1s and 2p2s the zooplankton is starving because they are not allowed to consume diatoms.

page 8497, lines 24ff: No, not all biological parameters were optimised. For the apparently best model version, a subset of parameters entered the optimisation procedure.

page 8498, lines 16ff: "However, the most straightforward interpretation, in our case, is that the more complex model fits the data better because it better represents the biological processes" I doubt that this is a fair interpretation, given the fact that the most complex model transforms phytoplankton biomass to detritus linearly, disregards grazing of diatoms and includes an instant (infinitely fast) formation of fast sinking diatom aggregates together with cysts.

page 8498, line 22: "Exceptions include the studies by Matear (1995), Prunet et al. (1996) and Fennel et al. (2001)". This reads as if only these studies were concerned with a posteriori errors of parameter estimates. There are certainly more studies that could be referred to. The sentence should either be rewritten no to raise this impression of exclusiveness for the three cited studies or the authors include all studies where a posteriori errors were approximated. Personally, I think that the in-depth

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model analysis of **Faugeras et al. (2003)** should be mentioned here. In their study, they approximated the full Hessian matrix while applying a one-dimensional model.

page 8499, lines 4ff: This paragraph is not very helpful. It is more interesting to know how affective oxygen data was to constrain the model solutions. The issue of grazing versus sinking of diatoms could possibly discussed here.

page 8501, line 17: "More importantly, the study demonstrates that models that accurately simulate the upper 100 m..." Nothing new, e.g. see discussion on particle export in Schartau and Oschlies (2003, Part II: Standing stocks and nitrogen fluxes, Journal of Marine Research, 61, 795-821).
page 8502, line 21: "Thus, increased complexity in biological models may still be justified based on ecological and theoretical considerations." Certainly this is a valid statement, but the ecological and theoretical considerations of 2p2s are not justified.

page 8502, line 26: "This emphasizes the importance of validating carbon cycle models in the twilight zone." This final sentence/statement is not connected with the content of the study presented here. The authors cannot infer any conclusion for the twilight zone. I would argue that constraints that help to distinguish between aggregation and grazing in the upper layers is at least as important as processes in the twilight zone, where the dynamics of dissolved organic matter is believed to be of greater importance than the formation and export of cysts.

3 Suggested additional literature:

Faugeras, B., Lèvy, M., Mèmery, L., Verron, J., Blum, J., Charpentier, I. (2003) Can biogeochemical fluxes be recovered from nitrate and chlorophyll data? A case study assimilating data in the Northwestern Mediterranean Sea at the JGOFS-DYFAMED station. Journal of Marine Systems, 40-41, 99-125.

Gunson, J., Oschlies, A., Garçon, V. (1999) Sensitivity of ecosystem parameters to simulated satellite color data using a coupled physical-biological model of the North Atlantic. Journal of Marine Research, 57, 613-639.

Vallino, J. J. (2000) Improving marine ecosystem models: Use of data assimilation and mesocosm experiments. Journal of Marine Research, 58, 117-164.

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