

Interactive comment on “ERSEM 15.06: a generic model for marine biogeochemistry and the ecosystem dynamics of the lower trophic levels” by M. Butenschön et al.

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1 General comments

ERSEM is an important and well-known model in marine biogeochemistry and ecosystem modelling. This manuscript provides a detailed description of the latest version and demonstrates its application in several contexts, including 1D and 3D coupled hydrodynamic models as well as new drivers for 0D simulations ("ERSEM Aquarium") and individual parameterization analysis (Python framework). The new version is a significant update since Blackford et al. (2004) and is presented in unprecedented detail.

This, combined with the new drivers for implementation and testing, ensures that the manuscript and its supplement constitute a novel and useful contribution to the marine modelling community.

Given the scope and complexity of the model the authors have done an admirable job describing it in such detail within a reasonable number of pages. The manuscript is surprisingly readable: I was able to read it through closely over a few sessions, and can imagine that it could be read fairly quickly by a modeller shopping around for a new model. I can therefore see it functioning both as a reference and as an introduction for potential new users. The example implementations and figures towards the end are particularly useful in this latter regard.

Where I think there is the most room for improvement is in the explanation and justification of the model. The model structure and formulation represents a large number of modelling choices: the more these can be explained/justified on rational or empirical grounds, the more useful will be this paper, I believe. Citation is a good way of doing this, but in lieu of that even modelling anecdotes could be helpful. The overall ratio (citations : modelling choices) is acceptable in the present manuscript, but I think it could be higher, and there are a few places where I feel that more explanation is clearly needed. I have indicated some places where more explanation is desirable or needed in my specific comments.

Overall, I am pleased to recommend this manuscript for publication subject to minor revisions.

2 Specific comments

p7068, Eqn 1. The last term is not explained. If it is already covered by the fluxes across the sea floor (p7069, l3) then the term should be deleted. If it represents some biogeochemical transformations of pelagic state variables which are particular to the

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bottom layer and not covered by the \mathcal{F} s, this should be explained here.

Section 2.2 is a nice addition, very useful for work on coupling ERSEM to physical models.

p7074, Eqn 4. Might be worth explaining the basis for neglecting nutrient excretion by phytoplankton (e.g. Puyo-Pay et al., 1997).

p7074, Eqn 5. It seems that the ERSEM treatment of nutrient limitation departs from Geider et al., 1997, 1998 and Fasham-type models (Fasham et al., 1990) in another important sense. In ERSEM, nitrogen and phosphorus limitation do not impact the gross primary production (as do silicate and iron limitation) but instead increase the rate of excretion and lysis, and also limit chlorophyll synthesis (Eqns 6, 7, 10). This seems to be a key structural difference and presumably has a physiological/experimental basis — I would like some more explanation/references for this difference in the treatment of limitation by different nutrients. A consequence seems to be that ERSEM phytoplankton in nitrogen-limited regimes, such as the surface waters of the subtropical gyres in summer, will go on happily converting DIC to DOC. Might this help to explain the "paradoxical" summer drawdown of DIC at BATS? Other aspects that may deserve explanation/references: 1) Eqn 5 uses a negative exponential form for the saturation of photosynthesis with irradiance, consistent with target theory / a Poisson process (Sakshaug et al., 1991); 2) Eqn 5 predicts that carbon fixation becomes insensitive to temperature and nutrient limitation at low light (physiological justification?).

p7076, Eqns 9-10. I think it may be better swap the order here. For me, the "acclimated quota" is really defined by Eqn 10, and then parameterized by Eqn 9. Also, I find the term "acclimated quota" confusing — perhaps a better term would be "nutrient-replete ratio of chlorophyll synthesis to carbon uptake". The word "acclimated" is confusing here because it would seem to imply a ratio under conditions of balanced growth, when C:Chl ratio has adjusted to the ambient light levels. Equation 9 rather seems to parameterize the non-acclimated ratio (cf. Eqn 4 in Geider et al. 1997). Under accli-

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mated conditions, the Chl:C ratio in the denominator might be related to E_{PAR} (cf. Eqn 5 in Geider et al., 1997).

p7076-7077, Eqns 11-13. It is not obvious to me that the loss rates from excretion, respiration, and lysis should be the same for both carbon and chlorophyll. Can these assumptions be justified? For example Geider et al. (1997) do not immediately assume that the chlorophyll respiration rate equals the carbon respiration rate.

p7077-7078, Eqns 15-18. Again I think a change in order would make for easier reading, so that the reader is not left wondering why "nutrient demand" should be calculated at all. I would start with Eqn 18 to calculate nutrient uptake, then explain that this is limited by internal cellular "demand" and an upper limit imposed by the capacity to actively take up nutrient at the cell surface (here termed "availability", but maybe "max uptake" would be better?). Might also help to remind that the affinities have units [$\text{carbon}^{-1} \text{time}^{-1}$] unlike the other "r"s. On a scientific note, surely the assumption of a linear dependence of (maximum) uptake rate on external nutrient concentration deserves some comment/references (e.g. Aksnes and Egge, 1991; Franks, 2009)? When a nutrient starved cell is suddenly exposed to a very high external nutrient concentration, it seems likely that the cell-surface uptake capacity would be saturated, which is inconsistent with the linear formulation of Eqns 16, 17. However, internal constraints on nutrient uptake rate (via S_{gpp} and r_{nlux}) would then presumably limit the realized nutrient uptake rate to realistic levels, such that a saturation parameter for uptake at the cell surface might be redundant. . . ?

p7079, Eqns 21-23. Should explain why silicate gets this special treatment. Something to do with lack of internal storage...?

p7080-7081, Eqns 27-31. Again it would be good to briefly explain where this more elaborate multi-source feeding parameterization comes from. As far as I can tell, it is equivalent to a Fasham-type Michaelis-Menten formulation (Fasham et al., 1990, Eqns 8, 9) with the feeding preference constants multiplied by Michaelis-Menten type

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"detectability ratios". But it is not clear to the reader what extra is gained by the f_{min} parameters. Chasing down the reference I find that the ERSEM parameterization is a "Class 2D passive switching model" (Gentleman et al., 2003, Table 3a). But can we say anything about why this particular choice was made for ERSEM, among the many possibilities?

p7082, Eqns 32-34. The parameterization of trophic transfer appears to be a large source of sensitivity/uncertainty in biogeochemical models (Anderson et al., 2013). Can anything be said about how ERSEM developers arrived at this particular formulation?

p7086, Eqns 45-46. Why is the maximum uptake flux of R by bacteria capped at a value of rR ? What does this represent ecologically? I would have expected a maximum flux proportional to bacterial biomass (B), in which case no capping would be needed. . .

p7092, 17-15. This is not entirely clear. For example: Does the small POM receive iron input directly from the grazing fluxes of all zooplankton on nano- and picophytoplankton?

p7098. What about aragonite dynamics?

p7099, Eqn 92. This makes me uneasy about mass conservation. Sedimentation redistributes the living phytoplankton biomass (Eqn 1). But here the sedimentation flux divergence of living phytoplankton contributes directly to the calcite dynamics without any biogeochemical transformation. Wouldn't this "create" carbon from nothing in the lower levels? Doesn't it duplicate the sedimentation term in Eqn 1 applied to calcite?

p7103, Eqn 111. It's not obvious to me why the remineralization flux of dissolved organic iron might be assumed proportional to the grazing flux from medium POM to mesozooplankton. What exactly is the sequence of events that is being parameterized here? Wouldn't it be better related to zooplankton excretion fluxes?

p7104, 15. Would be nice to have a reference for silicate remineralization being confined

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to the benthos.

p7105, Eqn 125. How is the calcium ion concentration calculated? From salinity?

p7111, l21. If I have understood correctly from reading further, the benthic state variables describe the total content per square metre of all three layers combined (corresponding to the c_b in Eqn 138), so there is strictly no explicit vertical resolution, even between the three layers. When it is necessary to account for layer-specific habitat and predation ranges, the individual layers contents are calculated from the total content and an implicit vertical resolution model (Eqn 151), and a vertical line is used to denote the restriction. However, only the unrestricted total contents are evolved dynamically. Please add something at this point and/or later to clarify this to the reader.

p7113, Eqn 139. I assume this comes from parameterizing the physical exchange as a linear mixing flux and setting the overall tendency to zero? A little more explanation might help.

p7113, Eqn 140. Please explain where this comes from, and why a different equation is needed when $c_p > c_b$. Moreover, why do we care about c_{bed} ?

p7114-7115, Eqns 144-147. I would start by assuming Eqn 147 but with a general e-folding depth (say λ). The total c_b is then be given by Eqn 144 with D replaced by λ . I think Eqn 145 actually only applies for $d \gg \lambda$ (note the "uv" term = $-\lambda d e^{-d/\lambda}$ when integrating by parts). So then we can say that in the limit $d \gg \lambda$, the mean penetration depth $D \approx$ the e-folding scale λ . Eqn 144 as written then follows.

p7115, Eqns 148-150. I'm afraid you lost me here. What is the basis for Eqn 148? Eqn 149 appears to relate a function of depth on the LHS to a constant on the RHS. How does this lead to Eqn 150?

p7117, l13-14. Reference to support exclusive feeding on particulates by anaerobic bacteria?

p7117, l15-17. Reference to support preferential uptake of organic nitro-

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gen/phosphate?

p7118, I8. Anaerobic bacteria really only excrete particulate matter? Please provide a reference.

p7119, Eqn 163. Doesn't the oxygen dependence only apply to aerobic bacteria?

p7120, Eqn 166. Why do we have the food preference constants in the detectability fraction, unlike in the pelagic (e.g. Eqn 27)? Same comment for Eqn 168.

p7125, Eqn 181-182. I find this whole derivation a bit dubious. Eqn 182 implies that burial only occurs when the mean penetration depth D is changing, but in a system in quasi-equilibrium I would expect a constant burial flux even with a constant D . The argument seems to be based on approximating the burial flux as the product of a 'burial velocity', independent of the concentration, and the concentration at the total depth. But this sounds more like an advective flux, whereas the sediment system is earlier assumed to be diffusion-dominated for inorganic states (Eqn 138). I would have rather expected an argument based on a diffusive flux at the total depth. Assuming the exponential decay profile and a constant organic matter diffusivity ν_{odiff} , this diffusive flux would result in a burial rate independent of the rate of change of D :

$$\left. \frac{\partial Q}{\partial t} \right|_{bur} = \frac{\nu_{odiff} Q}{D^2(1 - e^{-d/D})} e^{-d/D} \quad (1)$$

Perhaps there is in fact a good foundation for Eqn 182 but if so it should be better explained here (noting that the Kohlmeier 2004 reference is in German).

p7135, I6. Only the slowly or never degrading part of the sediment matter is eroded?

p7136, I3. Not clear how this slope (units mass-length⁻⁴) is translated into a time scale.

p7151, I1-11. It looks like there is also an persistent underestimation of summer nutrient levels, consistent with the weak secondary blooms mentioned in the text. Perhaps the benthic system is not remineralizing fast enough (cf. silicate), or GOTM is not cap-

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turing enough summer mixing events. . . I notice also an apparent decreasing trend in the surface oxidized nitrogen, perhaps also because of too-weak benthic return fluxes. It's also notable that the interannual variability in the model seems consistently weaker than in the data (Figures 2 and 3). Perhaps some aspect of the forcings is responsible?

3 Technical comments / typos

"food web" not "food-web"

"North Sea" not "North-Sea"

"case study" not "case-study"

p7065, l1. "Given the importance of these applications, transparent descriptions. . ."

p7065, l9. "occurred"

p7065, l19. "a scientific tool"

p7065, l22. "Allen et al. (2001) adopted"

p7065, l23. "Holt et al. (2012) and Artioli et al. (2012)"

p7065, l24. "Blackford et al. (2004) applied"

p7065, l25. "Barange et al. (2014) used applications of the model in the major coastal upwelling zones of the planet, and. . ."

p7066, l1. "(2014) have assessed the skill of the model, demonstrating. . ."

p7066, l9. "climate change"

p7066, l21. "nitrogen, phosphorus,"

p7066, l24. "The present paper provides a full description of all model components ,

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simple case studies illustrating the model capabilities in an idealised mesocosm type framework and three vertical water-column implementations of opposing character, and a brief illustration of a full-scale three dimensional application."

p7067, l4. "licence" assuming this is UK English.

p7067, l17. "feedback"

p7070, l7-8. Actually the \mathcal{F} is used in many instances to denote rates with units $[\text{time}^{-1}]$ rather than fluxes with units $[\text{concentration}\cdot\text{time}^{-1}]$ (e.g. Eqns 14, 20, 23, . . .). Perhaps those \mathcal{F} s should be changed to \mathcal{S} s?

p7070, l17. "equations"

p7070, l26. "exception"

p7071, l14. "radiation"

p7071, l16. "coefficients"

p7071, l20. Latex failure.

p7072, l3. "numerical"

p7072, l19. "heterotrophic nanoflagellates"

p7072, l25. "silicic"

p7073, l5. "simplicity; their pathways. . ."

p7073, l7. "dissolved"

p7073, l19. "a net result"

p7074, l15. Shouldn't this be Geider et al., 1998?

p7075, Eqn6. Q_{exc} should be the fraction excreted, but the RHS appears to be 1 minus this fraction.

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p7075, Eqn7. Doesn't this blow up (or give poor numerics) as either limitation factor approaches zero?

p7076, l5. Break this sentence in two, e.g.: "This formulation differs from the original formulation of Geider et al. (1997) in its asymptotic limit of the carbon to chlorophyll a synthesis at high PAR. In the original formulation. . ."

p7076, l16. Remove "consequently".

p7079, Eqn 24. Missing parentheses around pCO₂-379.48.

p7081, Eqns 28-30. The notation may be a bit confusing here. Eqn 28 uses a "specific uptake capacity" \mathcal{S} , but it is not specific to the uptaker concentration (as it was for phytoplankton uptake of nutrients), but rather to the concentration of "total available prey" (this could be made clearer by a second equality in Eqn 30). Seems it would have been better to define \mathcal{S}_{growth} via Eqn 28 with Pr substituted for Z (and adjust Eqn 29). Maybe too dangerous to redefine anything now. Perhaps the best solution is to replace "specific" in l1 with "total prey-specific" and in l5 with "prey-specific".

p7082, l5. "activity-related"

p7082, Eqn 33. $\left. \frac{\partial Z}{\partial t} \right|_{growth}$ is not defined.

p7084, Eqn 38. It would be better to write this as a sum of concentrations multiplied by layer thickness, divided by the total water column height.

p7086, Eqns 45-46. Again I think it would have been better to define the rate S_{upt} as a flux specific to bacterial biomass instead of available DOM.

p7087, Eqns 49-50. r_{lab} should be replaced with r_{rel} ?

p7087, l16. "occurs"

p7091, Eqn 64. Might be clearer to divide by $q_{refN:C}$, so that the ratio $q_{N:C}/q_{refN:C}$

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can be seen as a factor accounting for nutritional status (from the point of view of the decomposing bacteria that are not explicitly resolved).

p7098, Eqns 86 and 87. I think there are three typos: "upt" in overhead of Eqn 86, and "lab" in two overheads in Eqn 87, unless I missed something.

p7098, l16. Replace "where" with something like: "The dynamics of particulate inorganic carbon (or "calcite") may be decomposed as:"

p7100, l1. Insert something like "(plus scavenging of dissolved inorganic iron)"

p7108, l8. "non-modelled forms of inorganic matter and the background. . ."

p7110, l10. The R for calcite has changed into an L .

p7111, l23-24. "the silicate and iron cycles are simplified, bypassing the living functional types in a similar manner to the pelagic part of the model"

p7111, l27. Clash of singular "a particularity" with plural "are" - rephrase.

p7113, l2. "biogeochemical"

p7114, l9. Should be c_b not c I think.

p7115, Eqn 151. Surplus "/"

p7117, Eqn 158. Shouldn't the R s be Q s for the benthos?

p7118, Eqn 159. Shouldn't that be a Q instead of H in the first term on RHS?

p7121, l2. "capable of feeding on itself"

p7121, Eqns 170-171. The uptake terms should be specific to the ψ ("upt, ψ "), or use the \mathcal{F} s.

p7125, l1. "Note that this. . ."

p7125, l3. "does not"

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p7131, l2. "atmospheric inputs, otherwise denitrification. . ."

p7133, Eqn 209. Shouldn't the "depo" and "sed" be subscripts and the "cp" overhead (cf. Eqn 136)?

p7133, l10. Is it an R or an L for calcite? Be consistent!

p7135, l2. "In the case"

p7135, l13-14. "towards equilibrium"

p7136, l23. "cycle"

p7137, l16. "identical between"

p7138, Eqn 229. Should the G be an O ? The "s" is also not defined in the text.

p7144, l7. Should be $>$ or $<$ 1?

p7145, Eqn 258. pcrowd on the LHS and RHS?

p7149, l10. Better "strong nutrient limitation"?

p7149, l11. "microbe dominated"

p7149, l14. "an order of magnitude"

p7155, l22. Shouldn't this read "product of the chlorophyll a content and PAR"?

p7157, l2. "pigment complements"

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