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Geoscientific Model Development Discussions

Interactive comment on "The role of ecosystem function and emergent relationships in the assessment of global marine ecosystem models: a case study with ERSEM" *by* L. de Mora et al.

Anonymous Referee #3

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General comment

I would first like to apologise with the Authors for this late review and the late release of the comments on their manuscript. However, the reason of my delay is partly to attribute to the manuscript itself. I was very much captured by the title, which sounded definitely different from the typical works published by GMD and I thought there was really something new. Nevertheless, when I started reading, I was distracted by some overstatements, the several misuses of ecological and biogeochemical terms and by the lack of references to the most relevant literature on ecological functions, macro-ecology and stoichiometric concepts in biogeochemistry (see main comments below).

My initial perception was that the authors have learned about the functioning of marine ecosystems only through the model reality and that they only used references from the modelling literature, if not from their modelling group only. In addition, the English needs some care, words are doubled in many sentences ("is that that is is") and there are quite some repetitions of the same concept as well.

The choice of GMD implies that the presented methods or models are either linked to specific model versions of public availability, or have more general validity and should help other scientists in the application of new approaches or use innovative validation methodologies. I do not think it is the aim of the authors to demonstrate that this specific model (one of the various existing flavours) can do things that other models cannot, because this manuscript does not specifically address where this model is superior. Although I do realize that there exist two schools of thought, one that prefers to have the discussions done together with the presentation of the results, and one that prefers a dry presentation with a combined discussion of the presented results at the end, I do think that the way results are presented and discussed seem to be there only to state that the model is good and does what it should do.

But at last, I came to Sec. 3.4 and immediately the manuscript made sense to me; I perceived the underlying power of this work and the reason why it should be eventually published by GMD. That section is worth the whole paper and it is very well written. However, the entire manuscript in this current form is not ready and major changes are needed to streamline the concepts. I do hope that this review does not sound too negative, because it is indeed an encouragement to work further and contribute soundly to the science of biogeochemical modelling.

Summary of main comments

1. The introduction is too much philosophical and much less methodological as it would be required by a journal like GMD. The authors do not make clear the distinction between biogeochemistry and ecosystem modelling; the use of concepts related to both disciplines is not always appropriate and the choice of literature is often limited to modelling papers from the same group of the authors. See detailed comments below for some examples of both.

2. In particular, there is one main statement that the authors make that would deserve further (indeed philosophical) considerations. In Sec. 3 they state that "The emergence of a coherent natural relationship in a simulation is a strong indication that the model has a appropriate representation of the ecosystem functions that create the emergent relationship. If the emergent relationship is not seen in the model, this implies that the ecosystem functions that bring about the emergence are not correctly implemented in the model". There is a kind of analogy to the Type I and Type II errors in statistics here. I think the authors should better justify why the emergence of an observed macro-ecological property in the model does guarantee that this is not a "false positive", or maybe better expressed, that this is not "right for the wrong reasons". This equivalence - same behaviour in ecosystem function means "all" underlying biogeochemical and ecological parameterizations are correct - is reported many times in the text, but I do not think the authors have provided enough evidence that this is a truism.

3. A much more detailed discussion is required at the end, that collects all the points quickly mentioned when presenting the results. As it stands, the discussion is a summary of the same considerations made in the Results section but does not provide further insights or avenues for discussion. Some important points are not taken further (see detailed comments below)

4. Are the emergent properties a concept of general applicability for the validation of any biogeochemical model or is it only specific to ERSEM? (as a sentence at page 6103, lines 12-13 seems to imply). The final section on code availability would become a more substantial added value to the manuscript if the data used to assess the ecosystem functions are made publicly available. I am not specifically referring to the tools to make the comparison (like the python scripts, which can actually be subject to a direct request to the authors as already stated in the manuscript). Most equations of the various statistical fits and ranges can actually be derived from the provided tables, but it would be very useful if the model data shown in the various figures and the Martiny et al distribution of Fig. 5 would also be provided in the author's website. This would allow other modelling groups to perform the same analyses and compare with the ERSEM model as a reference.

5. The concept of "independency from hydrodynamical models and physical conditions of the ocean" which is expressed in the introduction and in the conclusions should be better explained. The authors do see that some functionalities break in certain regions (both in model and observations) and, most of all, they have not demonstrated that the results are the same if single biogeochemical provinces are considered. It may be that the large spread is due to bad performances of the model in certain regions. I personally do not think that ecosystem functioning is independent of physical processes, though I do understand the concept that by pulling together data from various regions and only looking at macro-ecological properties we may overcome the limitations of global ocean models.

Detailed comments

P6097_L27- Does this mean that matching "observables" does not imply a proper representation of ecosystem behaviour? Please explain

P6098_L4- The emergent property of “high-chlorophyll = diatom domination” should be backed up by references to the literature from real observations. The authors only mention Holt et al (2014), which is a modelling validation exercise, and then say that this relationship is “seen in many in situ datasets” (L10). Some explicit references should be given. One may think that diatoms are the only functional group capable of large blooms in models because diatoms are among the most studied organisms; the behaviour of the other functional groups is less known and we are seeing diatoms emerging because the others are more inadequately parametrized.

P6099_L4-8 I would say complementary and not more valuable. Especially when data are scarce and scattered over large regions.

P6100_L1-3 A web search of the ERSEM model gives the Baretta et al. (1995) paper as first reference. Why are the authors not mentioning this, especially if they say at the end that this shows the validity of a model initially meant for regional applications?

Section_3 This is a key part of the manuscript and deserves some attention. I think the authors use quite some jargon and do not provide a clear definition of the terms they use. I do not understand if they implies a difference between “ecosystem functions” (the title of the section says function, used as singular) and “ecosystem functioning” as it was initially introduced in Sec. 1. Is there an analogy with the state functions of thermodynamics? Moreover, they use properties of ecosystem and biogeochemical properties as interchangeable, but I think most of the properties they present are more related to biogeochemical considerations rather than ecosystem-based.

P6103_L17- These sentences are full of errors or approximate concepts. I think phytoplankton cell sizes influence ecological and physiological processes, not ecosystem processes. Light absorption IS an internal process (I thought chloroplasts

were in the cell) and nutrient uptake, metabolism and light harvesting are all physiological processes. Individual effects implies that they are different between individuals, which is not the case for unicellular organisms. What does it mean that phytoplankton function based classifications are also used? I thought this was a description of the properties considered in the functional group approach. This description mixes ecology, physiology and the specific choice of functional groups in ERSEM all in a bunch of sentences.

P6104_L4 Desirability? Do you mean palatability?

P6104_L7-11 This sentence is a repetition (actually much better stated than the previous confused concepts).

P104_L11-13 I presume none of the existing biogeochemical models has an explicit parametrisation of dominance.

P6105_L1-4 This text is already in the figure caption

P6105_L17 This point recurs in the whole manuscript, related to Fig. 2, 3,4 and 5. The authors refer to density histogram, which is actually misleading. It is either a density distribution (i.e. normalized to 1) or a histogram distribution (i.e. counts). I presume they use 2-dimensional histograms as data numbers are shown for every bin. Also see the comment below for Fig. 5.

P6105_L18 Is this the same fit shown in Fig. 1?

P6106_L17-18 “more similar in shape”. Say in which part of the curve. I would say that it is more similar to Brewin

P6108_L3-6 I do not completely understand what the authors mean here. There is no need to discuss the shape of the model distribution as you only show the fits from the other satellite-based models. It is a model-model comparison and I would limit the analysis to the fitted curves. I think the ERSEM fit follows the Brewin curve because of the constrain of fitting the picophytoplankton equation first.

P6108_L6- This final part of the section is a thorough discussion that should go in the Discussion section. It is linked to other results found in the next result sections and should be discussed together. What about the Southern Ocean where diatoms are usually dominating? Also, make clear if you discuss the fit or the distribution. I see no reason to comment the distribution as you only show the fit from Hirata and Brewin works.

P6109_L14-16 This sentence requires a reference

P6109_L17 Scientific usage? I think all usages are scientific in this context. A reference would seem appropriate here as well.

P6109_L23-25 I would think that photoacclimation was not described first by Polimene et al. (2014). Previous literature should be considered, as for instance, MacIntyre, H., T. Kana, T. Anning, and R. Geider (2002), Photoacclimation of photosynthesis irradiance response curves and photosynthetic pigments in microalgae and cyanobacteria, J. Phycol., 38, 17–38.

P6110_L1-5 Please provide a reference for this sentence.

P6110_L8 What do you mean by mechanical?

P6110_L17 Data is plural

P6110_L27 Quantile. A quartile is 25%

P6111_L24-25 See comment above on density histogram

P6111_L25 Average of data or all the model levels within 40 m?

P6111_L27 Define the extension of Arctic and Antarctic oceans (also for the previous section)

P6112_L23-24 This is a repetition of the previous concept

P6113_L1-2 I do not understand why this is a consequence of the previous analysis. See main comment 2.

P6113_L3-4 You can only compare the fits. There may be points in the original distribution of Sathyendranath et al (2009) that also fall below the line. Actually, these points do affect the slope and you can comment on that. This should also be discussed further at the end.

P6113_L14 I would say just "carbon cycle". Carbonates in the ocean are one of the components of the carbon cycle, not a separate one.

P6114_L13-17 This is now an empirical probability density function and it is called histogram. Why the tick labels are hidden? The distributions should share the same scale if the areas are normalized to 1.

P6115_L16-22 This statement is also linked to the main comment 2 and should be put in the discussion. Why would you exclude the possibility to get an overall acceptable relationship for dysfunctional reasons? See for instance Flynn, K. J. (2010), Ecological modelling in a sea of variable stoichiometry: Dysfunctionality and the legacy of Redfield and Monod, Prog. Oceanogr., 84(1–2), 52–65.

P6115_L23-26 I cannot understand the reason of this comment here? Seems like a fragment from another discussion.

P6116_L1-9 This sentence should also go in the discussion. The issue of grid resolution is pertinent to all the analyses done and not only to the POC:PON ratio.

P6116_L10-16 It is not clear why the Gaussian distribution is mentioned here. Parameter estimation is clearly a function of the sample size, but 40k data are usually sufficient to capture the major shape of the distribution. I think the authors could do an analysis of skewness and kurtosis if they want to qualify the differences in the distributions. Or just limit the comment to the one at lines 17-18.

P6116_L19-21 Does this happen because of model parameter constraints? Also, I cannot clearly see that excess POC:PON is better captured. measuring the skewness would probably help to quantify this.

P6116_L29 common, not comment

P6117_L5-6 Why only in modelling? I think the authors should add some references here, as for instance taken from Sterner, R. W., and J. J. Elser (2002), Ecological stoichiometry: the biology of elements from molecules to the biosphere, Princeton University Press, Princeton, NJ.

P6117_L6-7 Redfield connected this ratio to the one found in particulate organic matter, thus linking its origin to living organisms.

P6117_L27 Michaelis

P6118_L26-P6119_L4 It is not completely clear to me the reason for this comment here. It would certainly be pertinent in an overall discussion of the validation method, that demonstrates how all the ecosystem functional properties analyzed here are actually neglecting (or better making implicit) the role of bacteria which is instead considered in models of the ERSEM type.

P6119_L4 better add “dissolved” detrital fields

P6119_L15-26 This is a very interesting original contribution that extends the work done by Moore at al. (2013). Did you use the spatial max and min of the climatological

distribution? Please make sure that the figure caption report that this is not an estimate from Moore et al. paper but it’s your own original work

P6120_L4-5 I am a bit confused here with these ratios. Fig. 6 shows N:C and the original paper showed C:N. I think you mean that their lower cut off value (in their original figure) was 2.0, not in Fig. 6

P6120_L14-15 This is also a bit confused. Please make clear from the beginning (Pag. 6119, when presenting the Moore estimates and your original contribution for inorganic ranges) that the reported ranges are a combination of existing literature values and additional estimates .

P6120_L18-19 You use the word “observed”, but according to your previous discussion there ratios are estimated because computed from ranges of numerators and denominators that are not necessarily correlated.

P6120_L20-21 I think this should be expanded in the discussion

P6120_L21-24 Does this mean that the organic component retain more P? I think this deserves discussion because it may be linked to the “coastal” origin of the ERSEM model, where P is usually the limiting nutrient. I would link this to the discussion suggested above.

P6121_L1-4 This sentence is not completely clear to me. Are you referring to the model or observation data set? Also, I see this occurring only below a certain value of the inorganic range, and for Fe and P. This definitely deserve some more discussion in a later section.

P6121_L10-15 What is the dashed line in Fig. 6? It seems purple, so is that related to Si? Are diatoms allowed to store Si internally in the cell? How is this variability regulated? Please refer to the ERSEM equations when possible.

P6121L16 Why do you use the term deficit? I think the model is just following the allowed rules, that is varying between the minimum structural ratio and the maximum luxury storage (if considered). This implies that there is a discrepancy between the parametrised values and the ones found in nature. I do completely agree that the final surface iron concentration is a tuned outcome of the combination between atmospheric deposition and scavenging rates, and this should be reported in the discussion and conclusions. The authors are not aware of this but there is an upcoming new paper by Tagliabue that does show that all the existing global ocean iron models have the same surface iron distribution but completely different combinations of input and scavenging rates. This means that their analysis have been capable to find this!

P6122_L5-19 I think this discussion on the role of external sources should be in the "Discussion"! It is general and not only linked to this section

Discussion This is a summary, not a discussion. I think that it is an overstatement to say that many of the features seen here would not be visible in a flat comparison of model and data. This analysis is indeed powerful, but I see it as complementary to the other analyses (see main comment 2). There are other points that needs to be expanded, including some of the discussion that has been already done at the end of the result presentations and that are common to more than one emergent property. Some more specific comments follow

P6123_L15-30 how is the behaviour of diatoms connected to the ratios shown in Fig. 6? You should give more context here because it is difficult to get back to the results and look for the discussion you are referring to.

P6123_L28: you said that the distribution is not Gaussian, so why mentioning this here in the discussion?

P6124_L6 I think the word nutrients is a bit misleading here. Do you refer to inorganic or

organic nutrients? The power of the ERSEM-like parametrisations (originally from the ERSEM of the 90's) are that nutrients are indeed biogeochemical constituents and they can flow between various forms and components.

P6124_L12 I would say a combination of deposition and scavenging

P6124_L24-28 I agree, but I recall that it was already demonstrated in 2007 by Vichi, M., S. Masina, and A. Navarra (2007), A generalized model of pelagic biogeochemistry for the global ocean ecosystem. Part II: numerical simulations, J. Mar. Sys., 64, 110–134.

P6125_L2 I think this statement is too generic and requires some more discussion. Not all physiological parameterizations are well known, otherwise we would not have so many biogeochemical models with different combinations of terms.

P6125_L7 Was there a benthic parameterization?

Conclusions It would be good to have a future outlook on the applications of such a method: use it to compare with other models with different degrees of complexity or with fixed quota for certain nutrients. Also, the statement related to hydrodynamics needs some more discussion as suggested in my main comment 5.

Interactive comment on Geosci. Model Dev. Discuss., 8, 6095, 2015.