Biogeosciences
Discussions

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Interactive comment on "A novel acclimative biogeochemical model and its implementation to the southern North Sea" *by* Onur Kerimoglu et al.

Anonymous Referee #1

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Review of A novel acclimative biogeochemical model and its implementation to the southern North Sea, by Kerimoglu et al.

Summary

The manuscript describes the application of a recently published acclimative phytoplankton growth method in a 3D coupled hydrodynamics-biogeochemical model of the southern North Sea. Model results of a decade are compared with an extensive set of observations, and to some extent discussed in relation to the hydrography of the area.

General comments

This paper represents a significant effort to implement new process descriptions for phytoplankton cell biology in a biogeochemical model, and apply the model to a coastal

sea region. It is reasonably well-written, and, eventually, deserves publication. However, I'm struggling with a substantial number of issues (detailed below) that need to be addressed. Hence, I recommend major revisions, and re-review. Note that given this, I have not assessed the appendix, nor the supplementary material.

Title

I have two problems with the title: 1) as the acclimatisation scheme has been published previously, I would advise against using the word 'novel'; 2) 'implementation' suggests the presentation and discussion of how the acclimatisation method is implemented in the biogeochemical model, which is included in the manuscript, but not related to the application to the SNS. So I would suggest reformulating to, eg., The application of an acclimative biogeochemical model to the southern North Sea.

Structure

The authors should introduce a separate discussion section.

Comparison

In comparing model results with observations, the text is too qualitative, using expressions such as 'compare well', 'reasonable match', and so on, without defining what these are. This should be tightened up and quantified throughout. The same holds for comparison with previous work in the literature: a small subset of earlier biogeochemical modelling work is referenced, and it is suggested that the current model performs better, but without providing the evidence and quantifying the differences. It is also unclear why these studies were selected, and not others.

Logic/interpretation

The logic and interpretation tend to be hand-waving at best, flawed in some cases, and don't always consider multiple options. Examples are listed in the details section below. This needs to be improved. Separating the discussion will help.

Is it really 'better'?

The authors state at several points in the paper that their acclimative phytoplankton growth method is better what's used in more traditional biogeochemical models. However, unfortunately, they fail to provide any proof of this. In the very least, there should be an in-depth, quantified discussion comparing the current results with those of a suitably wide range of 'traditional' models. I get the impression from the manuscript that the 'novel' biogeochemical model was constructed by stripping an existing 'traditional' biogeochemical model of the relevant parts, and replacing these with the acclimative methods. If this is indeed the case, the authors would strengthen the manuscript immensely by providing and discussing a comparison with a similar run with the earlier model version. The authors will also need to discuss the following in a systematic way. More traditional biogeochemical models may lack (to various extents depending on the model) the full suite of acclimatisation as presented here, but they make up for that at least to some extent by representing several types of phytoplankton. This allows for spatial and temporal changes/patterns in phytoplankton composition. One could argue that the new model reflects this with one type with a range of traits, but it presumably has more flexibility in changing these traits over time for the same biomass than could happen in nature (one type of plankton can not change into another). Also, the authors are suggesting that they plan the inclusion of additional phytoplankton types. That would require curtailing the ranges of acclimatisation. Would that throw the baby out with the bath water, or have they already done so and would this be an attempt to get it back in?

Figures

Not all of the figures are clearly readable, and some information is missing.

Fig 3: I suspect that the colour scale is truncated, both at the high and low end, resulting in artificial saturation of the figure. This must be addressed. Also this figure would benefit from using a wider range of colours.

Fig 4. S and T are partly obscured by the dots, the cursive eta and n are barely visible on my printout

Fig 5-7. These are all too small. I can hardly read the axis legends and legends. Names on maps are cluttered.

Fig 8. Does ICES store chlorophyll? If so it would help if this were included.

Fig 9. Re-plot in colour. I can't work out the route taken from the cruise track figure.

Fig 10, 11, 14. The black contours are partially obscured by the dark blue.

Fig 11. I understand that these are surface values. Please also provide the bottom values.

Fig. 12. The colour scale is symmetrical around the centre, making it impossible to distinguish spring and autumn values. Please re-plot.

Grammar and language

Please check the grammar. There are quite a few anomalies that even a grammar checker would pick up (I'm not going to list them all).

Also use past tense to describe the results throughout.

Further detailed comments

p. 1

l. 1. autotroph: autotrophic?

l. 5. is based on novel concepts

l. 11 'sparce measurements'. Not clear what these are.

l. 13. delete prevalently

l. 14 shows significant seasonal and spatial variability

l. 14-16. not clear what is meant here

Section 2.1, title. 'Data' can originate from anywhere, including models. Use 'Observations', apply throughout.

p. 2

l. 18 monitoring stations used here(?)

p. 3

l. 11 in the benthos

l. 14 and rivers considered

l. 19 accessory: access to?

l. 24 give value for flexibility constant. Give rang of i, and values for q_i

l. 29 why use 'B' for phytoplankton (most models use P, and B for bacteria)?

p. 8

l. 6. Other explanations could be that: 1) the river-runoff is too high, or 2) the set of open boundary conditions used for the hydrodynamics and disolved components restricts the amount of flushing, leading to an accumulation of fresh water, nutrients, etc.. Or a combination. Please discuss.

l. 15 Now the rivers do come up, but the sentence is unclear, and I don't understand the link to grid resolution.

l. 18. Trends. These figures are not suitable to identify trends.

l. 19. 'in general well reproduced': this too qualitative, I list it here to present an example, but the paper is littered with these kinds of statements (I will not list them all).

l. 20/21. 'rather realistically represented': another one.

p. 9

Fig 4. There seems to be a 1:1 relationship, but with an anomaly on top. Does the anomaly in T correspond to the low values of S that bend away from the 1:1 line? Does this cluster represent a particular geographic area (front?)? Or a particular event/year (2010?)?

l. 2-3. this should be easy to test?

l. 5. Earlier. Than what?

l. 5. 'mostly well reproduced': difficult to see on the small graphs; quantify.

l. 6. 'probably'. other potential causes?

l. 10. 'is entirely reversed': I don't see this...

l. 11-p10 l. 3: this is discussion

p. 11

l. 1. 'easier': than what?

l. 3. variability matches very well: I don't see this/quantify.

l. 6. 'might be': why?

l. 8. 'typical': give numbers

p. 14

l. 2. grammatically incorrect.

p. 15

l. 6. 'were not able to ... observations': but this model doesn't do this, either...

l. 6-13. Please provide evidence for this.

l. 28. 'intuitively predictable': this is a contradiction in terms.

l. 29-34. This seems a rediculous over-interpretation of a potential contribution by estuarine overturning circulation. There's no evidence of overall higher nutrient concs in bottom waters (fig 8). Providing bottom values in fig 11 will likely support this. What's happening is that the nutrient-rich riverine waters enter/mix with the coastal waters, which are trapped by the coastal density(salinity) front.

p. 16

l. 8-p17 l. 5: It's not very clear what the function and message of this section are.

p. 17

l. 7. higher chlorophyll concentrations

l. 9-12. This is an unfair comparison. The observations in fig 5 are instantaneous, whereas the satellite composites are 3-monthly averaged. It's obvious that the satellite values presented in this way should be lower! This statement requires a proper comparison.

l. 13-14. this sentence trips over the various averages. Reformulate/clarify.

p. 18

l. 8. nutrient and turbidity gradients?

p. 19

l. 11-14. Please provide evidence for this.

p. 20

l. 5. ignorance of: ignoring

Figure captions

fig 2. Fe-P is not in the figure. Explain bAP in the caption.

fig 4. delete 'abbreviated' (2x)

fig 5. Observations (circles) and model estimates (lines) ... correlation coefficients (r),... data points (n)

Fig 10, 11, 14. Specify what the black contour lines represent.

fig 13. Mention that this is a log scale. Explain rho and eta.